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NEW CONSTRUCTIONS IN GRAPHICAL STATICS.

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Written for Van Nostrand's Magazine.

VII.

THE ARCH OF MASONRY.

Arches of stone and brick have joints which are stiff up to a certain limit beyond which they are unstable. The loading and shape of the arch must be so adjusted to each other that this limit shall not be exceeded. This will appear in the course of the ensuing discussion.

Let us take for discussion the brick arch erected by Brunel near Maidenhead England, to serve as a railway viaduct.

It is in the form of an elliptic ring, as represented in Fig. 14, having a span of 128 ft. with a rise of $24\frac{1}{2}$ feet. The thickness of the ring at the crown is $5\frac{1}{4}$ ft., while at the pier the horizontal thickness is 7 ft. 2 inches.

Divide the span into an even number of equal parts of the type bb, and with a radius of half the span describe the semicircle gg. Let $ba=24\frac{1}{4}$ ft. be the rise of the intrados, and from any convenient point on the line bb as b_5 draw lines to a and g. These lines will enable us to find the ordinates ba of the ellipse of the intrados from the ordinates bg of the circle, by decreasing the latter in the ratio of bg to ba. For example, draw a horizontal through g_3 cutting b_5g at i_3, then a vertical through i_3, cutting b_5a at j_3, then will a horizontal through j_3 cut off a_3b_3 the ordinate of the ellipse corresponding to b_3g_3 in the circle, as appears from known properties of the ellipse.

Similarly let $bq=64$ ft. + 7 ft. 2 in., and with bq as radius describe a semicircle. Let $bd=24\frac{1}{4}$ ft. + $5\frac{1}{4}$ ft. be the rise of the extrados, and from any convenient point on bb, as b_5' draw lines to d and q. These will enable us to find the ordinates bd of the ellipse of the extrados, from those of the circle, by decreasing the latter in the ratio of bq to bd. By this means, as many points as may be desired, can be found upon the intrados and extrados; and these curves may then be drawn with a curved ruler. We can use the arch ring so obtained for our construction, or multiply the ordinates by any convenient number, in case the arch is too flat for convenient work. Indeed we can use the semicircular ring itself if desirable. We shall in this construction employ the arch ring ad which has just been obtained.

We shall suppose that the material of the surcharge between the extrados and a horizontal line tangent at d causes by its weight a vertical pressure upon the arch. That this assumption is nearly correct in case this part of the masonry is made in the usual manner, cannot well be

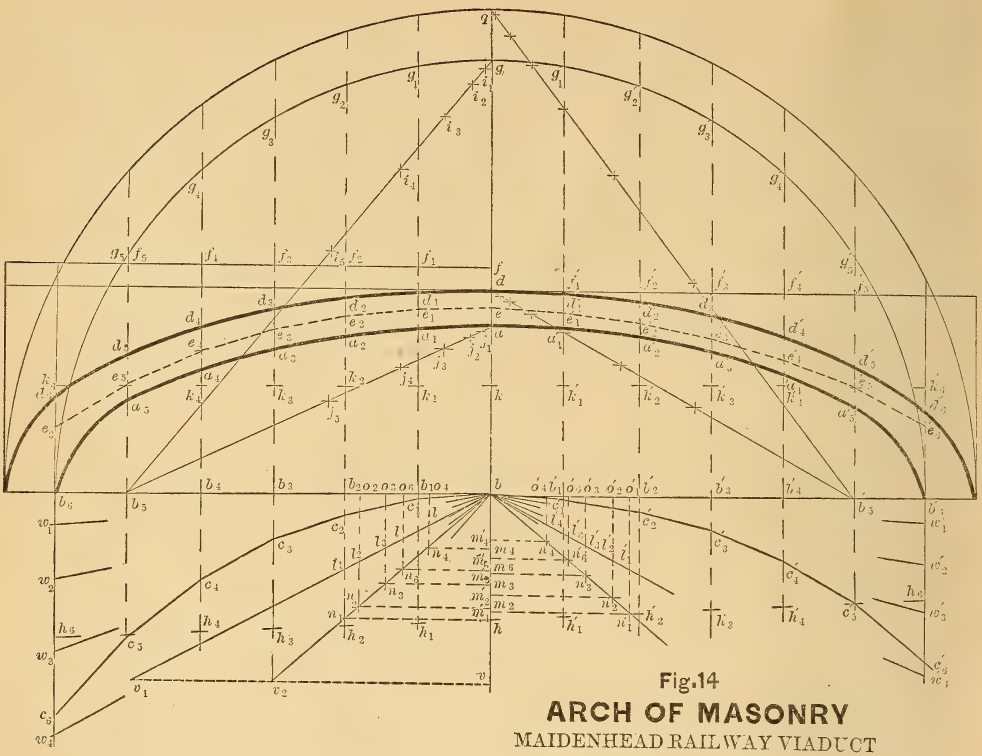

Fig.14

ARCH OF MASONRY

MAIDENHEAD RAILWAY VIADUCT

doubted. Rankine, however, in his Applied Mechanics assumes that the pressures are of an amount and in a direction due to the conjugate stresses of an homogeneous, elastic material, or of a material which like earth has an angle of slope due to internal friction. While this is a correct assumption, in case of the arch of a tunnel sustaining earth, it is incorrect for the case in hand, for the masonry of the surcharge needs only a vertical resistance to support it, and will of itself produce no active thrust, having a horizontal component.

This is further evident from Moseley's principle of least resistance, which is stated and proved by Rankine in the following terms.

"If the forces which balance each other in or upon a given body or structure, be distinguished into two systems, called respectively, *active* and *passive*, which stand to each other in the relation of cause and effect, then will the passive forces be the least which are capable of balancing the active forces, consistently with the physical condition of the body or structure.

For the passive forces being caused by the application of the active forces to the body or structure, will not increase after the active forces have been balanced by them; and will, therefore, not increase beyond the least amount capable of balancing the active forces."

A surcharge of masonry can be sustained by vertical resistance alone, and therefore will exert of itself a pressure in no other direction upon the haunches of the arch. Nevertheless this surcharge will afford a resistance to horizontal pressure if produced by the arch itself. So that when we assume the pressures due to the surcharge to be vertical alone, we are assuming that the arch does not avail itself of one element of stability which may possibly be employed, but which the engineer will hesitate to rely upon, by reason of the inferior character of the masonry usually found in the surcharge. The difficulty is usually avoided, as in that beautiful structure, the London

Bridge, by forming a reversed arch over the piers which can exert any needed horizontal pressure upon the haunches. This in effect increases by so much the thickness of the arch ring at and near the piers.

The pressure of earth will be treated in connection with the construction for the Retaining Wall. On combining the pressures there obtained with the weight, the load which a tunnel arch sustains, may be at once found, after which the equilibrium polygon may be drawn and a construction executed, similar in its general features to that about to be employed in the case before us.

Let us assume that the arch is loaded with a live load extending over the left half of the span, and having an intensity which when reduced to masonry of the same specific gravity as that of which the viaduct is built, would add a depth df to the surcharge. Now if the number of parts into which the span is divided be considerable, the weights which may be supposed to be concentrated at the points of division vary very approximately as the quantities of the type af. This approximation will be found to be sufficiently exact for ordinary cases; but should it be desired to make the construction exact, and also to take account of the effect of the obliquity of the joints in the arch ring, the reader will find the method for obtaining the centers of gravity, and constructing the weights, in Woodbury's Treatise on the Stability of the Arch pp. 405 *et seq.* in which is given Poncelet's graphical solution of the arch.

With any convenient pole distance, as one half the span, lay off the weights. We have used b as the pole and made $b_6w_1 = \frac{1}{2}$ the weight at the crown $= \frac{1}{4}(af+ad) = b_6'w_1'$, $w_1w_2 = a_1f_1$, $w_2w_3 = a_2f_2$, etc. Several of the weights near the ends of the span are omitted in the Figure; viz., w_4w_5, etc. From the force polygon so obtained, draw the equilibrium polygon c as previously explained.

The equilibrium polygon which expresses the real relations between the loading and the thrust along the arch, is evidently one whose ordinates are proportional to the ordinates of the polygon c.

It has been shown by Rankine, Woodbury and others, that for perfect stability,—*i.e*, in case no joint of the arch begins to open, and every joint bears over its entire surface,—that the point of application of the resultant pressure must everywhere fall within the middle third of the arch ring. For if at any joint the pressure reaches the limit zero, at the intrados or extrados, and uniformly increases to the edge farthest from that, the resultant pressure is applied at one third of the depth of the joint from the farther edge.

The locus of this point of application of the resultant pressure has been called the "curve of pressure," and is evidently the equilibrium curve due to the weights and to the actual thrust in the arch. If then it be possible to use such a pole distance, and such a position of the pole, that the equilibrium polygon can be inscribed within the inner third of the thickness of the arch ring, the arch is stable. It may readily occur that this is impossible, but in order to ensure sufficient stability, no distribution of live load should be possible, in which this condition is not fulfilled.

We can assume any three points at will, within this inner third, and cause a projection of the polygon c to pass through them, and then determine by inspection whether the entire projection lies within the prescribed limits. In order to so assume the points that a new trial may most likely be unnecessary, we take note of the well known fact, that in arches of this character, the curve of pressure is likely to fall without the prescribed limits near the crown and near the haunches. Let us assume e at the middle of the crown, e_5' at the middle of $a_5'd_5'$, and e_5 near the lower limit on a_5d_5. This last is taken near the lower limit, because the curvature of the left half of the polygon is more considerable than the other, and so at some point between it and the crown may possibly rise to the upper limit. The same consideration would have induced us to raise e_5' to the upper limit, were it not likely that such a procedure would cause the polygon to rise above the upper limit on the right of e_5'.

Draw the closing line kk through e_5e_5', and the corresponding closing line hh through c_5c_5', and decrease all the ordinates of the type hc in the ratio of hb to ke, by help of the lines bn and bl, in a manner like that previously explained. For example $h_3c_3=n_3o_3$, and $l_3o_3=k_3e_3$. By this means we obtain the polygon *e* which is found to lie within the required limits. The arch is then stable: but is the polygon *e* the actual curve of pressures? Might not a different assumption respecting the three points through which it is to pass lead to a different polygon, which would also lie within the limits? It certainly might. Which of all the possible curves of pressure fulfilling the required condition, is to be chosen, is determined by Moseley's principle of least resistance, which applied to the case in hand, would oblige us to choose that curve of all those lying within the required limits, which has the least horizontal thrust, *i.e.* the smallest pole distance. It appears necessary to direct particular attention to this, as a recent publication on this subject asserts that the true pressure line is that which approaches nearest to the middle of the arch ring, so that the pressure on the most compressed joint edge is a minimum; a statement at variance with the theorem of least resistance as proved by Rankine.

Now to find the particular curve which has the least pole distance, it is evidently necessary that the curve should have its ordinates as large as possible. This may be accomplished very exactly, thus: above e_1 where the polygon approaches the upper limit more closely than at any other point near the crown, assume a new position of e_1 at the upper limit; and below e_4' where it approaches the lower limit most nearly on the right, assume a new position of e_4' at the lower limit. At the left e_5 may be retained. Now on passing the polygon through these points it will fulfill the second condition, which is imposed by the principle of least resistance.

A more direct method for making the polygon fulfill the required condition will be given in Fig. 18.

It is seen in the case before us, the changes are so minute that it is useless to find this new position of the polygon, and its horizontal thrust. The thrust obtained from the polygon *e* in its present position is sufficiently exact. The horizontal thrust in this case is found from the lines *bn* and *bl*. Since $2vv_2$ is the horizontal thrust, *i.e.* pole distance of the polygon *c*, $2vv_1$ is the horizontal thrust of the polygon *e*.

By using this pole distance and a pole properly placed, we might have drawn the polygon *e* with perhaps greater accuracy than by the process employed, but it being the process employed in Figs. 2, 3, etc., we have given this as an example of another process.

The joints in the arch ring should be approximately perpendicular to the direction of the pressure, *i.e.* normal to the curve of pressures.

With regard to what factor of safety is proper in structures of this kind, all engineers would agree that the material at the most exposed edge should never be subjected to a pressure greater than one fifth of its ultimate strength. Owing to the manner in which the pressure is assumed to be distributed in those joints where the point of application of the resultant is at one third the depth of the joint from the edge, its intensity at this edge is double the average intensity of the pressure over the entire joint. We are then led to the following conclusion, that the total horizontal thrust (or pressure on any joint) when divided by the area of the joint where this pressure is sustained ought to give a quotient at least ten times the ultimate strength of the material. The brick viaduct which we have treated is remarkable in using perhaps the smallest factor of safety in any known structure of this class, having at the most exposed edge a factor of only 3½ instead of 5.

It may be desirable in a case like that under consideration, to discuss the changes occuring during the movement of the live load, and that this may be effected more readily, it is convenient to draw the equilibrium polygons due to the live and dead loads separately. The latter can be drawn once for all, while the former being due to a uniformly distributed load can be obtained with facility for different positions of the load. The polygon can be at once combined into a single polygon by adding the ordinates of the two together. Care must be taken, however, to add together only such as have the same pole distance. In case the construction which has been given should show that the arch is unstable, having no projection of the equilibrium polygon which can be inscribed within the middle third of the arch ring, it is possible either to change the shape of the arch slightly, or increase its thickness, or change the distribution of the loading. The last alternative is usually the best one, for the shape has been chosen from reasons of utility and taste, and the thickness from consideration of the factor of safety. If the center line of the arch ring (or any other line inscribed within the middle third) be considered to be an equilibrium polygon, and from a pole, lines be drawn parallel to the segments of this polygon, a weight line can be found which will represent the loading needed to make the arch stable. If this load line be compared with that previously obtained, it will be readily seen where a slight additional load must be placed, or else a hollow place made in the surcharge, such as will render the arch stable. In general, it may be remarked, that an additional load renders the curvature of the line of pressures sharper under it, while the removal of any load renders the curve straighter under it.

The foregoing construction is unrestricted, and applies to all unsymmetrical forms of arches or of loading, or both. As previously mentioned, a similar construction applies to the case of an arch sustaining the pressure of water or earth; in that case, however, the load is not applied vertically and the weight line becomes a polygon.

RETAINING WALLS AND ABUTMENTS.

Let $aa'b'b$ in Fig. 15 represent the cross section of a wall of masonry which retains a bank of earth having a surface aa_6. Assume that the portion of the wall and earth under consideration is bounded by two planes parallel to the plane of the paper, and at a unit's distance from each other: then any plane containing the edge of the wall at b, as ba_0, ba_1, etc., cuts this solid in a longitudinal section, which is a rectangle having

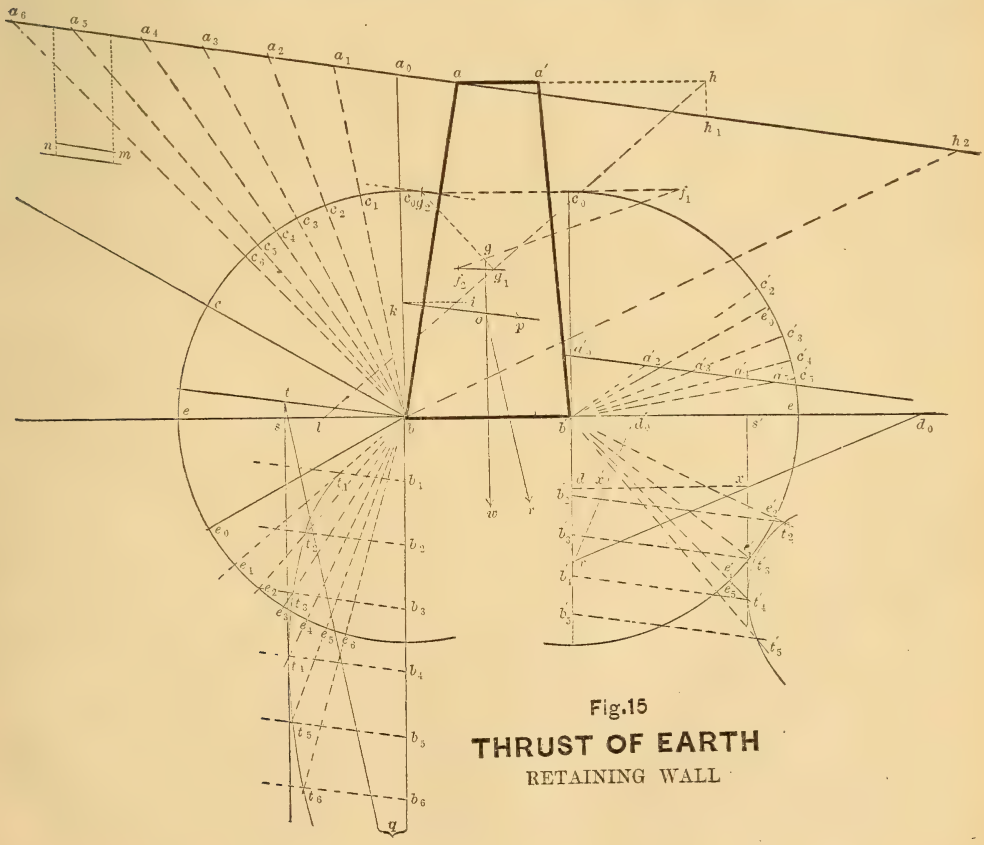

Fig.15

THRUST OF EARTH

RETAINING WALL

a width of one unit, and a length ba_0, ba_1, etc.

The resultant of the total pressure distributed over any one of these rectangles of the type ba is applied at one-third of that distance from b: *i.e.* the resultant pressure exerted by the earth against the rectangle at ba_0 is applied at a distance of $bk=\frac{1}{3}\ ba_0$ from b.

That the resultant is to be applied at this point, is due to the fact that the distributed pressure increases uniformly as we proceed from any point a of the surface toward b: the center of pressure is then at the point stated, as is well known.

Again, the direction of the pressures against any vertical plane, as that at ba_0, is parallel to the surface aa_5. This fact is usually overlooked by those who treat this subject, and some arbitrary assumption is made as to the direction of the pressure.

That the thrust of the earth against a vertical plane is parallel to the ground surface is proved analytically in Rankine's Applied Mechanics on page 127; which proof may be set forth in an elementary manner by considering the small parallelopiped mn, whose upper and lower surfaces are parallel to the ground surface. Since the pressure on any plane parallel to the surface of the ground is due to the weight of the earth above it, the pressure on such a plane is vertical and uniformly distributed. If mn were a rigid body, it would be held in equilibrium by these vertical pressures, which are, therefore, a system of forces in equilibrium; but as mn is not rigid it must be confined by pressures distributed over each end surface, which last are distributed in the same manner on each end, because each is at the same depth below the surface. Now the vertical pressures and end pressures hold mn in equilibrium: they therefore form a system in equilibrium. But the vertical pressures are independently in equilibrium, therefore the end pressures alone form a system which is independently in equilibrium. That this may occur, and no couple be introduced, these must directly oppose each other; *i.e.* be parallel to the ground line aa_5.

Draw $kp \parallel aa_4$, it then represents the position and direction of the resultant pressure upon the vertical ba_0. Draw the horizontal ki, then is the angle ikp called the *obliquity* of the pressure, it being the angle between the direction of the pressure and the normal to the plane upon which the pressure acts.

Let $ebc=\Phi$ be the *angle of friction, i.e.* the inclination which the surface of ground would assume if the wall were removed.

The obliquity of the pressure exerted by the earth against any assumed plane, such as ba_3 or ba_4, must not exceed the angle of friction; for should a greater obliquity occur the prism of earth, a_0ba_3 or a_0ba_4, would slide down the plane, ba_3 or ba_4, on which such obliquity is found.

For dry earth Φ is usually about 30°; for moist earth and especially moist clay, Φ may be as small as 15°. The inclination of the ground surface aa_4 cannot be greater than Φ.

Now let the points a_1, a_2, a_3, etc., be assumed at any convenient distances along the surface: for convenience we have taken them at equal distances, but this is not essential. With b as a center and any convenient radius, as bc, describe a semi-circumference cutting the lines ba_1, ba_2, etc. at c_1, c_2, etc. Make $ee_0=ec$; also $e_0e_1=c_0c_1$, $e_0e_2=c_0c_2$, etc.: then be_0 has an obliquity Φ with ba_0, as has also be_1 with ba_1, be_2 with ba_2, etc.; for $a_0be_0=a_1be_1=a_2be_2=90^\circ+\Phi$.

Lay off bb_1, bb_2, bb_3, etc., proportional to the weights of the prisms of earth a_0ba_1, a_0ba_2, a_0ba_3, etc.: we have effected this most easily by making $a_0a_1=bb_1$, $a_0a_2=bb_2$, $a_0a_3=bb_3$, etc. Through b, b_1 b_2, etc. draw parallels to ki; these will intersect be_0, be_1, be_2, etc., at b, t_1, t_2, etc. Then is bb_1t_1 the triangle of forces holding the prism a_0ba_1 in equilibrium, just as it is about to slide down the plane ba_1, for bb_1 represents the weight of the prism, b_1t_1 is the known direction of the thrust against ba_0, and bt_1 is the direction of the thrust against ba_1 when it is just on the point of sliding: then is t_1b_1 the greatest pressure which the prism can exert against ba_0. Similarly t_2b_2 is the greatest pressure which the prism a_0ba_2 can exert. Now draw the curve $t_1t_2t_3$, etc., and a vertical tangent intersecting the parallel to the surface through b at t; then is tb the greatest pressure which the earth can exert against ba_0. This greatest pressure is exerted approximately by the prism or wedge of earth cut off by the plane ba_4, for the pressure which it exerts against the vertical plane through b is almost exactly $b_4t_4=bt$. This is Coulomb's "wedge of maximum thrust" correctly obtained: previous determinations of it have been erroneous when the ground surface was not level, for in that case the direction of the pressure has not been ordinarily assumed to be parallel to the ground surface.

In case the ground surface is level the wedge of maximum thrust will always be cut off by a plane bisecting the angle cbc_0, as may be shown analytically, which fact will simplify the construction of that case, and enable us to dispense with drawing the thrust curve tt.

The pressure tb is to be applied at k, and may tend either to overturn the wall or to cause it to slide.

In order to discuss the stability of the wall under this pressure, let us find the weight of the wall and of the prism of earth aba_0. Let us assume that the specific gravity of the masonry composing the wall is twice that of earth. Make $a'h=bb'$, then the area $abb'a'=abh=abh_1$; and if $ah_2=2ah$, then ah_2 represents the weight of the wall reduced to the same scale as the prisms of earth before used. Since aa_0 is the weight of aba_0, a_0h_2 is the weight of the mass on the right of the vertical ba_0 against which the pressure is exerted.

Make $bq=a_0h_2$, and draw tq, which then represents the direction and amount of the resultant to be applied at o where the resultant pressure applied at k intersects the vertical gw through the center of gravity g of the mass $aa_0bb'a'$. The center of gravity g is constructed in the following manner. Lay off $a'h=bb'$, and $bl=aa'$; and join hl. Join also the middle points of ab and $a'b'$: the line so drawn intersects hl at g_1 the center of gravity of $aa'b'b$. Find also the center of gravity g_2, of aba_0, which lies at the intersection of a line parallel to aa_0, and cutting ba_0 at a distance of $\frac{1}{3}\,ba_0$ from a_0 and of a line from b bisecting aa_0. Through g_2 and g_1 draw parallels, and lay off g_2f_1 and g_1f_2 on them proportional to the weights applied at g_1 and g_2 respectively. We have found it convenient to make $g_2f_1=\frac{1}{2}ah_2$, and $g_1f_2=\frac{1}{2}\,aa_0$. Then f_1f_2 divides g_1g_2 inversely as the applied weights; and g, the point of intersection, is the required center of gravity.

Let or be parallel to tq; since it intersects bb' so far within the base, the wall has sufficient stability against overturning. The base of the wall is so much greater than is necessary for the support of the weight resting upon it, that engineers have not found it necessary that the resultant pressure should intersect the base within the middle third of the joint. The practice of English engineers, as stated by Rankine, is to permit this intersection to approach as near b' as $\frac{1}{8}bb'$, while French engineers permit it to approach as near as $\frac{1}{5}bb'$ only. In all cases of buttresses, piers, chimneys, or other structures which call into play some fraction of the ultimate strength of the material, or ultimate resistance of the foundation as great as one tenth, or one fifteenth, the point should not approach b' nearer than $\frac{1}{3}\,bb'$.

Again, let the angle of friction between the wall and the earth under it be Φ': then in order that the thrust at k may not cause the wall to slide, the angle wor must be less than Φ'.

When, however, the angle Φ' is less than wor it becomes necessary to gain additional stability by some means, as for example by continuing the wall below the surface of the ground lying in front of it. Let $a_0'a_5'$ be the surface of the ground which is to afford a *passive* resistance to the thrust of the wall: then in a manner precisely analogous to that just employed for finding the greatest active pressure which earth can exert against a vertical plane, we now find the least passive pressure which the earth in front of the wall will sustain without sliding up some plane such as $b'a_3'$ or $b'a_4'$, etc. The difference in the two cases is that in the former case friction hindered the earth from sliding down, while it now hinders it from sliding up the plane on which it rests.

Lay off $e'e_0'=ee_0$; then taking any points $a_2'a_3'$, etc. on the ground surface, make $e_0'e_2'=c_0'c_2'$, $e_0'e_3'=c_0'c_3'$, etc.

Lay off $b'b_2'=a_0'a_2'$, etc., and drawing parallels through b_2', b_3', etc., we obtain the thrust curve $t_2't_3'$, etc.

The small prism of earth between $b'a_0'$ and the wall adds to the stability of the wall, and can be made to enter the construction if desired, in the same manner as did aba_0.

The vertical tangent through s' shows us that the earth in front of the wall can withstand a thrust having a horizontal component $b's'$ measured on a scale such that $b'b_2'=a_0'a_2'$ is the weight of the prism of earth $a_0'b'a_2'$.

This scale is different from that used on the left. To reduce them to the same scale lay off from b', the distances $b'd_0$ and $b'd_0'$ proportional to the perpendiculars from b on aa_4 and b' on $a_1'a_4'$ respectively. In the case before us, as the ground surfaces are parallel, we have made $b'd_0=ba_0$ and $b'd_0'=b'a_0'$.

Then from any convenient point on $b'b_4'$, as v, draw vd_0 and vd_0': these lines will reduce from one scale to the other. We find then that $x'd$ is the thrust on the scale at the left corresponding to $xd=b's'$ on the right: *i.e.*, the earth under the surface assumed at the right can withstand something over one fourth of the thrust sb at the left.

It will be found that a certain small portion of the earth near a_0' has a thrust curve on the left of b', but as it is not needed in our solution it is omitted.

If any pressure is required in pounds, as for example sb, it is founds as follows:—the length of ah_2 is to that of sb as the weight of $bb'aa'$ in lbs. is to the pressure sb in lbs.

Frequently the ground surface is not a plane, and when this is the case it often consists of two planes as ad, da_7 Fig. 16.

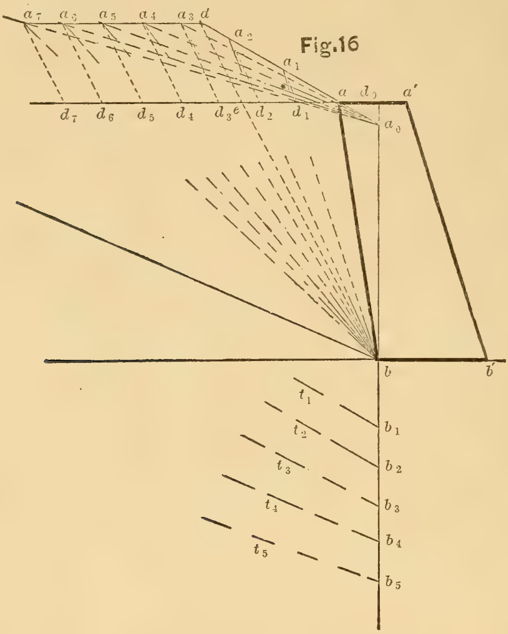
Fig.16

In that case, draw some convenient line as ad_7, and lay off ad_1, d_1d_2, etc. at will, which for convenience we have made equal. Draw d_1a_1, d_2a_2, etc. parallel to bd, and join ba_1, ba_2, etc.: then are the triangles bda, bda_1, bda_2, bda_3, etc. proportional in area to the lines ea, ea_1, etc. Hence the weights of the prisms of earth baa_1, baa_2, etc., are proportional to ad_1, ad_2, etc.

In case ab slopes backward the part of the wall at the left of the vertical ba_0 rests upon the earth below it sufficiently to produce the same pressure which would be produced if baa_0 were a prism of earth. The weights of the wedges which produce pressures, and which are to be laid off below b, are then proportional to $d_0d_1=bb_1$, $d_0d_2=bb_2$, etc. The direction of the pressures of the prisms at the right of bd are parallel to ad; but upon taking a larger prism the direction may be assumed to be parallel to a_0a_3, a_0a_4, etc., which is very approximately correct. Now draw $b_1t_1 \parallel a_0a_1$, $b_4t_4 \parallel a_0a_4$, etc.; and complete the construction for pressure precisely as in Fig. 15, using for resultant pressure the direction and amount of that due to the wedge of maximum pressure thus obtained.

In finding the stability of the wall, it will be necessary to find the weight and center of gravity of the wall itself, minus a prism of earth baa_0, instead of plus this prism as in Fig. 15; for it is now sustained by the earth back of the wall.

When the back of the wall has any other form than that above treated, the vertical plane against which the pressure is determined should still pass through the lower back edge of the wall.

In case the wall is found to be likely to slide upon its foundations when these are level, a sloping foundation is frequently employed, such that it shall be nearly perpendicular to the resultant pressure upon the base of the wall. The construction employed in Fig. 15 applies equally to this case.

The investigation of the stability of any abutment, buttress, or pier, against overturning and against sliding, is the same as that of the retaining wall in Fig. 15. As soon as the amount, direction, and point of application, of the pressure exerted against such a structure is determined, it is to be treated precisely as was the resultant pressure *kp* in Fig. 15.

In the case of a reservoir wall or dam, the construction is simplified from the fact that, since the surface of water is level and the angle of friction vanishes, the resultant pressure is perpendicular to the surface upon which the water presses. It is useful to examine this as a case of our previous construction. In Fig. 17, let *abb′* be the cross-section of the dam; then the wedge of maximum pressure against ba_0 is cut off by the plane ba_1 when $cba_1 = 45°$, *i.e.* ba_1 bisects cba_0 as before stated.

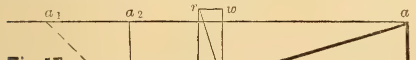
Fig.17

This produces a horizontal resultant pressure at *k* equal to the weight of the wedge. Now the total pressure on *ab* is the resultant of this pressure, and the weight of the wedge aba_0. The forces to be compounded are then proportional to the lines $a_1a_0 = bv_0$ and aa_0. By similarity of triangles it is seen that *ro* the resultant is perpendicular to *ab*.

It is seen that by making the inclination of *ab* small, the direction of *ro* can be made so nearly vertical that the dam will be retained in place by the pressure of the water alone, even though the dam be a wooden frame, whose weight may be disregarded.

We can now construct the actual pressures to which the arch of a tunnel surcharged with water or earth is subjected. Suppose, for example, we wish to find the pressure of such a surcharge on the voussoir $a_4d_4d_5a_5$ Fig. 14. Find the resultant pressure against a vertical plane extending from d_5 to the upper surface of the surface and call it p_5. Draw a horizontal through d_4 and let its intersection with the vertical just mentioned he called d_5''. Find the resultant pressure against the vertical plane extending from d_5'' to the surface, and call it p_5'. Now let $p_5'' = p_5 - p_5'$ and let it be applied at such a point of d_5d_5'' that p_5 shall be the resultant of p_5' and p_5''. Then will the resultant pressure against the voussoir be the resultant of p_5'' and the weight of that part of the surcharge directly above it.

FOUNDATIONS IN EARTH.

A method similar to that employed in the determination of the pressure of earth against a retaining wall, or a tunnel arch, enables us to investigate the stability of the foundations of a wall standing in earth.

Suppose in Fig. 15 that the wall *abb′a′* is a foundation wall, and that the pressure which it exerts upon the plane *bb′* is vertical, being due to its own weight and the weight of the building or other load which it sustains. Now consider a vertical plane of one unit in height, say, as bb_1; and determine the resultant pressure against it on the supposition that the pressure is produced by a depth of earth at the right of it, sufficient to produce the same vertical pressure on *bb′* which the wall and its load do actually produce. In other words we suppose the wall and load replaced by a bank of earth having its upper surface horizontal and weighing the same as the wall and load. Call the upper surface *z*, and find the pressure against the vertical plane *zb* due to the earth under the given level surface; similarly, find the pressure against zb_1. The surface being level, the maximum pressure, as previously stated will be due to a wedge cut off by a plane besecting the angle between bz and a plane drawn from b at the inclinatian Φ, of the limiting angle of friction. This enables us to find the horizontal pressures against zb and zb_1 directly: their difference is the resultant active pressure against bb_1.

Next, it must be determined what passive pressure the earth at the left of bb_1 can support. The passive resistance of the earth under the surface a against the plane ab as well as that against the plane ab_1 can be found exactly as that was previously found under the surface a'. The difference of these resistances is the resistance which it is possible for bb_1 to support. Indeed bb_1 could support this pressure and afford this resistance even if the active pressure against ab were, at the limit of its resistance, which it is not. The limiting resistance which is thus obtained, is then so far within the limits of stability, that ordinarily, no further factor of safety is needed, and the stability of the foundation is secured, if the active pressure against bb_1 does not exceed the passive resistance. This construction should be made on the basis of the smallest angle of friction Φ which the earth assumes when wet; that being smaller than for dry earth, and hence giving a greater active pressure at the right, and a less resistance at the left.

ON WELDING.

By RICHARD HOWSON.

Journal of the Iron and Steel Institute.

In the paper which I had the honor to read at the last meeting of this Institution, I endeavoured to show that the rationale of welding, although it might be esteemed trivial in itself, had really a very important bearing on the subject of puddling, especially puddling by machinery on a large scale. In the discussion which followed some criticisms fell from members, which induce me to claim the indulgence of the meeting for a few additional remarks, in order more clearly to elucidate the views then brought forward.

I drew attention on that occasion to the fact that the process of manufacturing wrought iron was, from beginning to end, a process of welding, whether it is effected by successive doubling, or whether the so-called homogeneous process is adopted.

In both cases it was maintained that the presence of silica, or some equivalent flux, was an advantage in the whole series of operations, in the same manner that it is of use in uniting two pieces of iron in a smith's forge.

Mr. Bell took the ground, which others have also done, that a flux is not necessary. He instanced in the first place the case of platinum, which unites of itself at a certain temperature by pure metallic contact. Now nothing could better confirm the view which I then took than this example. Platinum is an unoxidisable metal, and, consequently, during the process of heating, no film of oxide is formed on its surface to interfere with its proper union. Where there is nothing present which requires fluxing, a flux would be quite superfluous. Iron, on the contrary, is a highly oxidisable metal, and its oxide, in common with those of all metals, is of a very refractory character when pure, although its affinities are such that a very small addition of silica imparts to it comparative fusibility.

This leads to the second remark of Mr. Bell, that it is not necessary in welding to add any flux, that iron in fact can be welded without the use of sand. In reference to this observation I can only refer to my former remarks, the whole course of which distinctly implied that the outer skin of wrought iron and the interstices between its fibres consisted of cinder (where cinder was present), which was not a pure oxide but a basic silicate, which is equivalent to saying that it carried its own flux. In all the processes of manufacturing finished iron by piling, it is certain that no flux is added, but this is because sufficient is already present, being derived from the silicon contained in the pig iron, and from the silica and phosphoric acid which are always more or less mixed with the fettling of the puddling furnace. If I dwelt at unnecessary length on the frequent use of sand in the operations of forging, it was only for the purpose of illustrating the utility of that material, and tracing its mode of action in the larger operations of puddling, hammering, and rolling.

Referring now to the case of platinum, it follows that if cast steel, or any other metal which has been fused and contains no cinder, can be preserved from oxidation during the process of heating—that is to say, if the surfaces to be united can be made to come together in a perfectly clean state, a sound weld ought to be obtained, and the addition of a flux would be not only superflous, but injurious. It is known that two surfaces of a soft metal, such as lead, may be united even cold in this manner. In the case of cast steel, where heat is required, the following experiment is conclusive:—Two pieces of cast steel were planed and filed each on one face perfectly flat and true. The two clean surfaces were then placed on one another; the pieces were bound together with soft iron wire, and the edges luted round with fire-clay to prevent the entrance of air. After being brought in a smith's fire to a sub-white heat, the two were united with half-a-dozen blows of a sledge hammer. The weld is perfect—so thorough, indeed, that it cannot be detected when broken through. Its course can only be guessed at by following the indications on the rough outside edges.

We have here an instance of union exactly parallel to that of platinum, by unalloyed metallic contact. On the other hand, in order to exhibit the influence of an intervening refractory oxide, the following experiment was tried:—Two pieces of cast steel were prepared by planing and filing exactly as before, but the two level surfaces were painted with a thin film of jeweler's rouge, which is a tolerably pure form of peroxide of iron. A small well was also drilled in the middle of one piece, which was filled with the same rouge. The same process of binding, luting, heating, and hammering was then carried out, and the result is a bad weld, or rather no weld at all.*

On splitting open the two faces, which was readily done with a chisel, and examining their condition, the oxide in the well appears to have been imperfectly fused, while there are indications that the thin film has become partially reduced to the state of wrought iron by the action of the carbon in the steel, the oxygen no doubt escaping. However this may be, the experiment affords an exact parallel to what takes place when the attempt is made to weld two pieces of cast steel, by heating them separately in a fire without any flux. In the latter case, the oxide formed by the action of the blast would be quite as pure as the rouge which in the experiment was applied artificially, it would be equally refractory, and there would be more of it. The utility of a flux is here self-evident, and need not be dwelt on further. In the case of wrought iron, as already explained, there is not the same necessity for its employment, as it is already present in sufficient quantity, unless the iron is of an unusually dense character.

Referring now shortly to the subject of machine puddling, I propose only to consider it strictly in its relation to the foregoing conclusions. The more refined chemistry of the process I leave to abler hands.

The most suitable lining for a revolving furnace, in fact the only admissible one, so far as is known at present, is the purest oxide of iron that can be procured. It is, indeed, somewhat surprising to find a single material combining in itself so many good properties, and that no substance is capable of replacing it in its chemical adaptation for eliminating all the impurities of pig iron. It is, however, highly refractory, and this refractoriness, although an advantage in one repect, acts unfavorably on the final result. In charging the furnace, a small quantity of impure silicious cinder is usually thrown in, so as to assist in procuring a certain degree of fusibility, and enable the iron to revolve in a liquid bath; but it is generally considered that, the purer the cinder the better is the puddled product. Hence comes the practice of ap-

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* [NOTE].—I am indebted to Mr. Newcomb, the Secretary of the Cleveland Iron Trade Foremen's Association, for his great care and skill in manipulating these tests, which may be thoroughly relied on.

plying an intense heat, by which means alone a sufficient liquefaction of the fettling is procured in order to do its work effectively. The consequence of this is that, although the iron may be thoroughly puddled when the heat is finished, the ball contains a cinder which, being comparatively infusible, sets readily on exposure to the cold atmosphere. I have before pointed out that shingling a puddled ball is essentially a process of welding, and here comes into play the fact that a ball will not shingle satisfactorily, especially if of large size, unless its contained cinder has a certain degree of liquidity. In order to procure a fine quality of iron a pure fettling is employed, and the heat is urged up to a point which rapidly destroys the furnace, while it often happens, that when the ball comes to the hammer, it drops in pieces in consequence of the setting of the cinder before it can be squeezed together.

It is true that the same untoward event will sometimes occur in ordinary puddling, but this is on a smaller scale, and the puddler usually takes care of his heat, which does not affect his furnace injuriously to any great extent; besides which, if it did, one furnace spoilt, he would ask for another, which he sometimes does. The destructive action of a high temperature on the revolving furnace is also accompanied by another evil—viz., an excessive and costly consumption of fettling, owing to the entire inner circumference being exposed to flame.

These are some of the difficulties of machine puddling, but I believe they are the only really formidable difficulties. The common process of puddling has been established by long usage, but we should not forget that its introduction also was beset with many troubles. We have only to glance over the abstracts of patents, from the period when sand bottoms were used up to the present, to note the variety of schemes which have been attempted to improve it. Among these inventions there is one class which I would here refer to, as it is kindred to the subject in hand—viz., that of chemical mixtures, commonly called physic. On examining this long series of specifications of inventions applicable to puddling, almost every element will be found named, and innumerable combinations, supposed by the patentees (as it is usually expressed) to purify the iron. Many of these nostrums are absurd enough; others are of problematical value. There are, however, doubtless, a great number which are more or less useful, but, with the exception of those which are intended to give the iron a steely nature, it may be questioned whether any of them act chemically on the metal to improve its quality beyond that which the fettling alone can effect without admixture. Their utility probably consists simply in this, that they assist in imparting that fluidity to the cinder which is so essential in giving soundness to the hammered bloom.

According to the instructions usually specified in these inventions, the proper time for adding the physic seems to be towards the end of the process, and there is good reason for this, because if it were applied early it would tend to scour the fettling, whereas, if applied late, it unites at once with the cinder which is already melted, a great part of which will be carried away by the puddled balls. It is a question whether the adoption of this method of working in revolving furnaces would not enable the excessive heat to be somewhat moderated. A few handfuls of sand thrown in just at the point when the iron is ready to ball up, always facilitates the shingling process, and tends to produce a solid bloom, without affecting the iron, so far as I am aware, injuriously.

In reference, therefore, to the horror usually expressed at the mention of sand in the puddling furnace, it ought to be distinctly understood that it is only objectionable in the process for the very same reason that it is beneficial in the ball, viz.: that it promotes fusibility. In the former case, it acts destructively on the lining, and in the latter, it facilitates the union of the particles of iron. If it be supposed that the metal itself suffers detriment from its presence, the two following experiments, communicated to me by Mr. Stead (of the firm of Pattinson and Stead), and suggested originally, I believe, by Mr. Williams, seem to affirm the contrary.

A small quantity of Cleveland pig, accompanied with some cinder, was poured in a liquid state into a hot crucible; a cover was then put on the crucible, and the whole was then shaken up violently for a quarter of a minute.

No. 1 experiment, the cinder consisted of mill tap alone. In No. 2, it consisted of eighty per cent. mill tap and twenty per cent. sand. The analysis of the metal after each operation was as follows:

	No. 1. Per Cent.	No. 2. Per Cent.
Carbon..............	2.40	3.00
Silicon..............	Trace.	Trace.
Phosphorus.........	0.13	0.04

It appears that, in eliminating the phosphorus, where the real difficulty lies, the best result was obtained where sand was present. It may here be noted that the wonderful short time in which this elimination was effected from pig containing not less than 1.25 per cent. of phosphorus, confirms the fact, if confirmation were needed, that agitation, not less than heat, is an essential of effective puddling. Hence it is again a matter worth consideration, whether more perfect mechanical agitation may not to some extent be a substitute for, and tend so far to moderate, the excessive and destructive heat.

Machine puddling in the revolving furnace has during the last few years been on its trial, and has had a hard struggle for the mastery, to which it is still destined to attain. I have endeavored to indicate where the great difficulties lie, and how they may be to some extent obviated by varying the mode of working, but it is quite possible still more to obviate them by improvement in construction. With regard to the practicability of producing finished iron without piling, I have only to refer to the results which Messrs. Hopkins, Gilkes, & Co. have obtained, to show that at all events, with certain classes of manufacture, this point may be considered established. Whether it will be found expedient to work in masses of extremely large dimensions, or not, is a question which only the construction of costly machinery can decide.

INFLUENCE OF FORM ON THE MAGNETISM OF SOFT IRON CYLINDERS.

From "The Engineer."

Though the magnetism of soft iron is a subject that has been largely investigated, it has not been sufficiently recognised that the various experimental researches upon it, not having, for the most part, been made in a homogeneous magnetic field, are not directly comparable with one another, and so the most of the laws laid down are valid only for the special arrangements of a particular experiment. Further—and this objection proves to be a most important one—the researches hitherto have not been sufficiently comprehensive, and more especially, they have not been made with iron masses whose dimensions varied within wide limits. Lastly, the remanent magnetism of soft iron has been little examined, owing to the small so-called coercive force generally attributed to this material. These reasons recently induced M. Christoph Ruths to make a thorough investigation of the magnetism produced in soft iron cylinders of the most various dimensions, by increasing magnetising force in a homogeneous field, and of the magnetism which remained after the removal of the induction. The results have been published in a monograph by the author, which has recently appeared in Dortmund. The following is an abstract of M. Ruths' valuable paper:—As material for the iron cylinder to be magnetised, commercial iron wire was taken, obtained from the same manufactory. Seven kinds, of different thickness, were examined, their radii being, on an average, 2.54, 1.67, 1.39, 0.81, 0.62, 0.44, and 0.31mm. Of these different thicknesses pieces were prepared, whose lengths in millimetres were 200, 190, 180, 170, 160, 150, 140, 120, 100, 80, 60, and 40; further iron cylinders were procured, the radii of which were 6.5, 5.65, 4.87, 4.02, 3.65, and 2.75mm. Altogether seventy bars were examined, whose dimensions varied from nearly cubical form to the most extended forms. Their specific gravity was on an average 7.794. All the cylinders were, before experiment, annealed three times. To produce the induction three separate iron wire spirals were used, the dimensions of which were so large, that for most of the rods a perfectly homogeneous field might be assumed. The iron bars were held by cork in the middle of the spirals, and currents of one to eight Bunsen elements were employed. The intensity of the inducing current was determined with a tangent compass; the magnetic moment with a Wiedemann mirror compass. The bars above referred to were nearly all examined with all three spirals, and on an average, in each series of experiments, there were ten separate experiments. For the special arrangement of the experiments, and the order of operations, the precautions taken, and the mode of calculating the various values, we must refer to the original; merely remarking, that all the results of experiment, the magnetising forces as well as the magnetic moments, are expressed as multiples of the horizontal components of the terrestrial magnetic force, which is assumed to alter but little in the course of the researches. We can only here give the principal results of the experiments. If we denote as induced magnetism the whole magnetism manifested externally by a bar during the action of a magnetising force, the experiments first show that the moment induced by any magnetising force in the unit of weight is a constant function of the dimension-relation a ($a =$ relation of length to thickness), and that to equal magnetism of unit weight of different bars magnetising forces correspond, which are likewise a constant function of a. In bars of the same dimension-relations, equal magnetisms of unit weight always correspond to equal magnetising forces. If we make the curve of magnetisation of soft iron according to the values obtained, we find that for the beginning the quotient of entire magnetism and the magnetising force does not begin with *o*, but with a determinate value which increases with the dimension-relation a; the value of this quotient then increases to a maximum, and the increase is completed the sooner, the greater the dimension-relation of the bar. The occurrence of the turning point also depends on a, and the magnetising force with which this turning point, *i. e.*, the maximum of the quotient, occurs, is a constant function of the dimension-relation. After reaching the turning point the curve of magnetisation goes at first with a strong, then with a very slight concavity to a maximum value. Whether the latter occurs with a finite or only with an infinitely great magnetising force, must be experimentally determined from further experiments. A study of the question whether and within what limit the induced magnetism may be put proportional to the magnetising force, leads to the result that for bars whose dimension-relation a is very small—say under 12—the quotient of magnetism by magnetising force is approximately constant. It is only for bars of a larger a, that Wiedemann's assertion holds good, that the magnetic movements increase more quickly than the intensities of the currents. Mr. Ruths next compared the foregoing results of experiments with the various theoretically established formulæ for the relation of the induced magnetism to the magnetising current, and found that not one of them corresponds to the ascertained relations. Nor did a simple relation of the induced magnetism to the dimensions of the bar (whether thickness, length, or volume,) appear from the observations. Specially interesting are the facts established by M. Ruths with regard to the remanent magnetising of soft iron after sudden interruption of a current repeatedly sent in the same direction. From the remanent magnetic moment of a bar, a function of the previously acting magnetising force, a curve of magnetisation was constructed, and such curves were obtained for 56 bars, the dimension-relation a of which varied between 20 and 300. All these curves show a constant course throughout. The remanent moments reached, in some bars, values which exceeded $\frac{4}{5}$ of the induced magnetism; and the relation of the remanent to the induced magnetism was greater, the greater the dimension-relation, or the longer the bar. The discussion of the tabulated values of the remanent magnetism shows, that this, with very small magnetising forces, is either *nil*, or has only an imperceptible value. With a certain magnetising force, however, which, for the bars examined, varied between 3 and 10, a marked amount of remanent magnetism occurred. After reaching this perceptible value, the remanent moments increase very quickly, and in a greater ratio than the magnetising force, and the induced moment. The curve of magnetisation soon reaches a turning point at which the remanent moment is about $\frac{1}{2}$ of the maximum; this turning point occurs approximately with the same magnetising force. In the further course of the curve, the relation of the remanent moment to the magnetising force reaches a maximum, and the remanent moment then approaches a maximum quickly, and much sooner than the induced magnetism. When this has been reached, there is in general no decrease from it. On the other hand, proportionality was in no case met with between remanent magnetism and magnetising force. A comparisom of the maximum values, according to the dimension relations, shows throughout a decrease of them, with decrease of the length of equally thick bars; with reference to moments of bars of equal length, but different thickness, there seem to be the most manifold varieties. If, however, the maximum remanent moments be arranged according to the dimension-relation a, a conformity to law at once appears. The maximum of remanent magnetism referred to unit weight of a bar is a constant function of the dimension-relation a. This function, with $a=$ about 7, cuts the axis of abscissæ; below this value it is negative, above it positive. Above $a=7$ it rises very quickly, turning its convex side to the axis of abscissæ; this relation then reaches a maximum, and the curve rises in less degree than before. The remanent magnetisms are therefore greater, the longer the bars. The remanent moments in unit weight reached the remarkable value 727, a value which is over 80 per cent. of the corresponding induced moment, and far exceeds all hitherto observed moments. We must not here dwell on the relation of the remanent magnetism to the lengths of the bars, their thickness, and the magnetising force, which are fully discussed by M. Ruths. It may merely be observed that the remanent magnetisms here referred to had quite a stationary character. In long bars, twenty-four hours after induction, the loss of force was only about 3 to 4 per cent., which may very well have been caused by shaking in the process of removing from the spiral. A general survey of the peculiarities of induced magnetism and remanent magnetism, as above indicated, leads to the conclusion that "in direct opposition to the prevailing opinion, according to which the laws of induced magnetism are considered directly transferable to remanent magnetism, the remanent magnetism of soft iron shows a very different behavior from the induced magnetism." This conclusion places remanent magnetism in quite a new light. M. Ruths is going to make it the starting point of new investigations. In an appendix the author considers the magnetism of steel, and shows that similarly, not only the induced, but also the permanent magnetism of steel depends on the dimensions of the bar.

A NEW FORM OF ELECTRIC LIGHT.

From "Engineering."

The recognized arrangement of the electric light is well known. The carbon points from which the light is obtained are arranged vertically—one above the other, and the distance at which it is required to regulate them in order to produce the greatest effect is governed by clockwork. MM. Jablochkoff and Denayrouze arrange their carbons side by side, separated by a slip of kaolin. It is the combustion of the latter, under the influence of a highly intensified electric current, that gives the light. Thus the system pursued by MM. Jablochkoff and Denayrouze differs considerably from that adopted in the ordinary arrangement of the electric light.

In proceeding to give a more detailed description of this arrangement it may perhaps be as well to trace its gradual progress. The arrangements referred to as tried at Chatham, consisted of two slips of carbon some 4 inches in length and rather more than a $\frac{1}{4}$ inch in thickness, separated and surrounded by a composition, the whole being moulded into the shape of an ordinary wax or tallow candle. At the experiment in the Louvre this composition was dispensed with; the carbon slips being 4 millimeters in thickness, and whereas in the former trial the carbons differed from each other to a slight extent in thickness, those of the Louvre were of the same size. Each slip of carbon was insulated by a slip of kaolin about 3 millimeters in thickness.

The carbons are held in small brass tubes insulated from each other, and having their lower portions left vacant so as to be readily fixed upon two small rods or thick wires which, as will be explained hereafter, are in connection with the secondary wires of an induction coil. The entire arrangement of the illuminator or candle is bound together at the point where the carbons are set in the brass tubes by a piece of felt or other convenient substance embedded in paste. A further modification consists of overlaying the kaolin with a conducting mixture by which the action of the current is assisted. The kaolin in its cold and solid state is an insulator offering great resistance, but which under the influence of the current becomes hot and melts, and in this liquid condition its resistance is so far reduced as to bring it within the category of a conductor. In order to accelerate its combustion a fine slip of carbon about the size of the lead of a cedar pencil is placed across the kaolin at the top of the carbons, at which point combustion consequently first takes place and continues as the kaolin consumes away. To insure an equal consumption of the carbons it is necessary that the currents of electricity passing through them should be of an opposite character—that is, a positive followed by a negative current. The power of the light obtained at the Louvre from eight of these electric candles was, it is stated, equal to 300 carcel gas burners. In the latter modification, in which the carbon slips are replaced by a conducting composition—the light from which is described as a luminous band—the power of the light can be varied from that equaling two to that of the power of fifteen gaslights by varying the size of the coils and other evident means. In either case the lights are soft, steady, and brilliantly white, although by mixing with the porcelain clay any coloring matter the light can be colored at pleasure. The consumption of the kaolin or porcelain is about one millimetre an hour.

The electrical arrangement is as follows : The electro-motor, is an ordinary magneto or Gramme machine. In connection with it is the line wire or wires required for providing the current at the point or points at which the lights are required. For every light an induction coil is used, the primary coil of which is in circuit with the line wire, while in the circuit of the secondary coil is placed the candle. On the electro-motor being set in motion the candles in circuit are lighted by the induced current set up from the primary coil. Each light requires a coil, but the coil need not be in the neighborhood of the light. The machine used at the Louvre was of three horse power, giving 450 revolutions per minute. M. Denayrouze remarks that it is possible to relight a candle seven seconds after it has been extinguised by breaking the circuit, and that a great number of mineral substances, and even some organic substances, may be employed to replace the kaolin.

It will be clear from what has been said that lights thus arranged may be put in circuit, and so lit at any time (so long as the motor is in motion) in precisely the same manner as the gaslights of our streets and rooms are brought into use, and that the employment of one and the non-employment of others can in no way interfere with other lights served by the same or by other main line wires. The whole system, indeed, assimilates perfectly with our gas supply. The electro-motor may be regarded as the gasometer, the main wires as the gas mains, and the candles as the burners. To turn the light on it is only necessary to connect the wires, which may be done by a small switch similar to the taps used for the gas branches or chandeliers. The invention is altogether one of considerable merit, and of no small moment. Should M. Jablochkoff succeed in his anticipations, which the experiments at the Louvre give every reason to believe will be the case, he will do much towards revolutionizing our present lighting arrangements.

THE TESTING OF PORTLAND CEMENT.

By ISAAC JOHN MANN.

Proceedings of the Institution of Civil Engineers.

In testing Portland cement it is usual to examine the color, weight, pulverization, and tensile strength. These properties are here enumerated in the inverse order of their importance, the three first being subservient to the last. It is hardly possible to compare accurately the results obtained by different investigators, as the experiments should be made under similar conditions as regards weight, fineness, and method of gauging, but hitherto this uniformity has not been observed.

COLOR.

Good Portland cement, in its dry ungauged condition, is of a uniform dull grey color; occasionally a slightly greenish hue is observable, sometimes replaced by a slight buff tint. A yellow or earthy color is almost invariably indicative of inferior quality. The color can be best observed by pressing a small quantity on a sheet of white paper with a clean smooth trowel, or a piece of sheet glass. But this test alone is insufficient as a guide, as inferior cement may possibly pass it, though good cement rarely, if ever, has the objectionable color referred to.

WEIGHT.

The recorded weight of cement, as at present obtained, depends very much on the manner of weighing. A method, frequently adopted, as giving the most uniform results, is as follows: dry cement, as received from the manufacturer, is allowed to flow from a small hopper-headed shoot into a counterpoised measure of known capacity—usually referred to the imperial bushel—any surplus in the measure being carefully struck off with a light straight-edge; it is then weighed. Instead of a shoot, a vessel shaped like a colander, with holes from $\frac{1}{2}$ to $\frac{3}{4}$ inch in diameter, has also been used. The object in each case is to prevent compression. Weighed after this manner, Portland cement varies from 98 lbs. to 130 lbs. per bushel—equivalent to from 78 lbs. to 102 lbs. per cubic foot. It is difficult to conceive why such a measure of capacity as the bushel (which contains 8 gallons, equivalent to 1.283 cubic foot) should have been selected, as it is not easy of comparison with the usual standards of cubical measurements, the cubic foot being manifestly preferable in every respect.

The weight of cement, ascertained as described or in any analogous manner, bears a very uncertain ratio to its strength, as may be seen from Table 1, which gives the averages of a large number of experiments on cement from different London manufacturers.

Table 1.—Relation between the Weight and Tensile Strength of Cement.

Age of Samples seven days; fineness from 6 per cent. to 10 per cent.; time occupied in setting thirty minutes to two hours.

Weight per cubic foot in lbs.	80	81	82	83	84	85	86	87*	88	89
Breaking weight per sq. inch in lbs.	373	353	418	397	399	405	383	463	392	408

* Including some samples or cement of exceptionally high tensile strength.

For convenience of comparison the following tables have been extracted from the Papers submitted to the Institution by Mr. Grant and Mr. Colson (see next page).

The fineness of the cement is not given, and it is apparently assumed that the weight of loose cement in air is an index of its specific gravity. The increase of strength is shown to be by no means uniform, for, to mention one instance only, cement weighing 110 lbs. per bushel ex-

MR. GRANT'S SUMMARY OF TESTS "SHOWING INCREASE OF STRENGTH WITH INCREASED SPECIFIC GRAVITY."

Age of Samples seven days.

Weight per bushel in lbs....	106	107	108	109	110	111	112	113	114	115	116	117	118	119	120	121	122	123	124	125	126	127	128	129	130
Breaking weight of 2.25 square inches in lbs..............	473	592	650	647	708	694	687	702	700	705	768	718	644	778	732	706	717	674	820	816	657	865	917	920	914

ceeded in strength that weighing 126 lbs.

The results of Mr. Colson's numerous experiments on this subject have been epitomised thus:—

Age of Samples seven days.

Weight per bushel in lbs......	112	113	114	115	116	117	118	119	120	121	122	123	124	125	126	127
Breaking weight of 2.25 square inches in lbs..	747	688	719	708	729	675	745	706	718	702	732	676	631	545	603	558

The cement experimented on appears to have been heavier than that which the Author has had an opportunity of testing. The same deduction, however, is derivable from each of the tables, viz., that there is no fixed relation between the weight of cement, as at present ascertained, and its tensile strength.

In dealing with the question of weight, it is necessary to consider the methods of weighing. These are unsatisfactory and open to objections; the ascertained weights bear no definite relation to the real density, and depend, *cæteris paribus*, on the degree of pulverization—finely-ground cement appearing to weigh less per cubic foot than the same cement when coarsely ground, so that, in many cases, difference of weight may simply mean difference in degree of pulverization. With the same cement also the results are not always similar, even when weighed by one person two or three times in succession. Although there does not appear to be much, if any, connection between the weight of cement, as heretofore obtained, and its tensile strength, it would probably be erroneous to assume that the strength is unaffected by the actual density or specific gravity.

It is, therefore, suggested that the present unscientific and comparatively useless operation of weighing should be discarded, and the specific gravity be taken instead. That this has not hitherto been done is probably due to the trouble and difficulty experienced in obtaining the specific gravity by the ordinary method. In the case of Portland cement a liquid must be used which does not chemically affect it. The specific gravity of this liquid has also to be taken and the results reduced to the standard of distilled water, involving tedious arithmetical calculations.

To obviate these difficulties, and to enable the operation to be performed with facility and expedition, the Author has devised an extremely simple gravimeter. It consists of a small glass vessel holding, when filled to a mark on the neck, a given quantity of liquid, and of a glass pipette furnished with a graduated stem and stop-cock, and containing, when filled to a mark on its upper extremity, a volume of liquid equal to that held by the first-mentioned vessel, *minus* the quantity displaced by 1,000 grains of the densest substance intended to be examined.

In using the gravimeter the pipette is filled to the mark with paraffin, turpentine, spirits of wine, or any other liquid which does not act on the cement, preferably paraffin; 1,000 grains of the cement are then introduced into the smaller vessel, which is placed under the pipette and filled to the mark. Before this is quite completed the vessel may be corked, and the contents shaken to remove any small air-bubbles that may be entangled in the cement. The height of the column of liquid remaining in the pipette determines the specific gravity, which can be at once read off on the graduated stem. It is manifest that the denser the substance operated upon, the less liquid will be displaced in the smaller vessel, and therefore the less will remain in the pipette, and *vice versa.* In reading the accompanying gravimeter, the second place of decimals is estimated. Any greater degree of delicacy may be obtained either by diminishing the diameter of the stem or by reducing the range.

The specific gravity of any solid substance coming within the range of the instrument can, of course, be taken in the same manner. The advantages claimed for this gravimeter are, that neither the density nor the temperature of the liquid used need be taken into account; one weighing is sufficient, and all arithmetical calculations are dispensed with; it is also inexpensive, and requires little skill in manipulation.

In order to test the accuracy of the instrument, a small piece of granite was reduced to powder and its specific gravity taken by the gravimeter; the specific gravity of an unpulverized piece was then ascertained by the ordinary method. Similar experiments were also made with a piece of limestone. The results were:

Granite:
{ Specific gravity by gravimeter.......2.62
{ " " ordinary method.2.63

Limestone:
{ Specific gravity by gravimeter.......2.70
{ " " ordinary method.2.71

Some of the results obtained in comparing the weight of cement of various manufacturers, as usually taken, with the actual density or specific gravity, are shown in Table 2.

TABLE 2.

Weight of 1 cubic foot of Cement, as ordinarily obtained.	Specific Gravity.
lbs.	
75.50	2.91
81.50	2.80
83.25	2.96
84.25	3.03
85.00	2.93
85.00	2.96
85.50	2.82
87.50	2.91
89.00	2.96

The weight therefore, as ordinarily found, bears no relation to the density. Taking the whole number of experiments (about fifty) the specific gravity varied from 2.77 to 3.03, the average being 2.91, showing that Portland cement is heavier than ordinary building stone. The specific gravity of a number of specimens of fine sifted cement gave an average of 2.9; that of the coarse particles of the same cements being 2.93. The density of a specimen of unground clinker was 2.55; the clinker contained numerous small air-holes, which accounts for the specific gravity being less than that of the ground cement. Some samples of gauged cement, the age of which was three months, had an average density of 2.21, the extremes being 2.07 and 2.45.

PULVERIZATION.

The degree of pulverization at present demanded in this country seems to depend upon the judgment of the engineer. In the author's experiments, the fineness of the cement was measured by a sieve with circular perforations, the perforations being 0.0231 inch in diameter. Coarsely-ground cement, when gauged neat, generally possesses greater tensile strength than finely ground cement. On this subject a series of experiments was made by the author, some of the results of which are given in Table 3 (next page).

The "fine sifted" cement was that which passed through a sieve with apertures 0.0231 inch in diameter, the various degrees of coarseness being

TABLE 3.—EFFECT OF COARSENESS ON THE STRENGTH OF CEMENT. SAMPLES GAUGED NEAT.

Breaking Strain per square inch after seven days' immersion.

Fine Sifted Cement.	Percentage of Coarse Particles.				
	5	10	15	20	25
lbs.	lbs.	lbs.	lbs.	lbs.	lbs.
355	..	435	..	..	471
439	..	..	..	..	..
476	441	493	477	..	357
313	288	324	323	..	404
403	..	466	406	369	..
471	..	505	..	..	..
333	331	369	412	..	..
443	458	465	..	..	..
396	..	400	..	..	..
392	..	392	386	..	..
257	..	..	..	286	..
445	430	..	437	..	..
..	410	445	458	..	..
371	..	319	435	453	..
394	402	404	429	426	..
486	..	558	560	541	..
462	449	463	463	467	..
183*	212	246	276	270	..
281	259	248	311	254	..

* Very slow-setting.

produced by adding the requisite amount of coarse particles, care being taken to mix them thoroughly with the fine. Each number represents the average breaking weight per square inch of from three to six samples; the age of the samples was seven days, and they were taken promiscuously from the cement of different London manufacturers, and of one French manufacturer.

The experiments of Mr. Colson on this subject confirm those of the author, for with samples one month old, the breaking weight of 2¼ square inches was, for fine screened cement, 939 lbs., and for coarse unscreened, 989 lbs. When the age of the samples was six months, the results were similar.

It appears singular that when the coarse particles are mixed with the fine particles the strength of the sample should be increased, as when gauged by themselves the coarse particles do not evince the slightest tendency to bond together or set, but remain loose and inert like so much sand. This result, however, would seem to be in accordance with what occurs in the case of limes. Admitting Rondelet's statement, as to the adhesion of mortar to stone exceeding its own tensile strength, to be true also of cement, then as the tensile strength of the unground particle *per se* may be assumed to exceed that of fine cement recently gauged, it follows that within certain limits the presence of the coarse particles, which may be conceived to act as minute stones, increases the strength of the aggregate above that of its matrix.

In the analogous case of hydraulic limes the strength is similarly increased by the admixture of clean sand. The coarse particles of cement, although probably acting somewhat in the same way as the sand, seem to present additional facilities for the adherence of the fine cement to them. This may be seen from the following experiments, in which the cement of four different manufacturers was used, and a series of samples made from each; the grains of sand were somewhat larger than the coarse particles of the cement. The number of experiments is limited, but the results are marked.

TABLE 4.—EFFECT OF COARSE PARTICLES OF CEMENT AS COMPARED WITH SAND.

Fine sifted Cement and Sand in equal parts by Weight.			Fine sifted Cement and coarse particles in equal parts by Weight.		
Area.	Breaking weight.	Breaki'g Weight per Sq. Inch.	Area.	Breaking weight.	Breaki'g Weight per Sq. Inch.
Sq. in.	lbs.	lbs.	Sq. in.	lbs.	lbs.
2.26	472	209	2.27	510	224
2.25	452	200	2.26	402	178
2.25	360	160	2.24	517	230
2.28	530	232	2.27	850	374
2.32	522	225	2.30	965	419
2.32	567	244	2.34	945	404
2.32	400	172	2.27	662	291
2.32	422	182	2.36	622	263
2.30	422	183	2.25	600	266
2.22	580	261	2.28	963	422
2.25	592	263	2.30	937	407
2.25	610	271	2.28	1,060	465
Average...		217	Average...		328

The author has frequently found, in the case of cement containing not more than 10 per cent. of coarse particles, that they may be replaced by sand without affecting the strength of the aggregate; beyond 10 per cent. the results were increasingly in favor of the coarse particles as compared with sand. In some instances, increasing the quantity of coarse cement from 10 to 100 per cent. only diminished the strength by about 12 per cent.

In order to ascertain the effect of coarsely-ground cement on mortar, experiments were made with cement, of various degrees of coarseness, and sand gauged in equal volumes; the mean results are given in Table 5. As before, the cement of different manufacturers was used, and each of the breaking weights given is the average of not less than three careful tests. From this table it would appear that, when diluted with an equal volume of sand, coarse cement does not give quite such favorable results as fine. From various considerations it seems probable that, in the proportion of 1 part of cement to 1 of sand, the limit is reached, if not exceeded, in which the coarse cement is as effective as the fine, and that beyond this limit the coarse particles act nearly the same as so much additional sand.

Table 5.—Effect of Coarseness on the Strength of Cement.

Samples gauged with Cement and Sand in equal volumes. Breaking strain after seven days' immersion per square inch.

Fine sifted + Sand.	Percentage of coarse Cement.		
	10 + Sand.	15 + Sand.	20 + Sand.
lbs.	lbs.	lbs.	lbs.
208	224	..	..
241	..	235	..
268	277	248	..
272	249	..	229
170	203	..	199
233	251	230	..
272	230	274	294
210	206	205	208
233	215	187	..
220	230	252	265
257	251	275	295
61*	150	98	103
175	187	179	176

* Twenty-four hours in setting.

In many instances the author has found that mortar consisting of 1 part of cement to 3 parts of sand,—the cement containing 25 per cent. of coarse particles,—possessed little more than one-half the tensile strength of mortar gauged in the same proportions with fine sifted cement, the age of the samples being four weeks.

Mr. Colson's experiments lead to the same conclusion, and show that, gauged in the proportion of 1 of cement to 1 of sand, there was only a slight difference in favor of the fine cement; but that when gauged in the proportion of 1 of cement to 2 of sand, the coarseness of the cement had the effect of diminishing the strength to the extent of about 14 per cent. There can be no doubt that the diminution in the efficacy of the coarse cement will be more apparent as the proportion of sand is increased.

In the investigation of this part of the subject, which may be termed the mechanical agency of the cement particles, it is of the highest importance to understand, if possible, by what means the cementing material binds substances together. The explanation given by the French chemist Macquer, of the analogous action of limes, is probably equally applicable to Portland cement. This is to the effect that the extreme fineness of the particles of lime which reduces it altogether to surfaces, gives it the faculty of adhering firmly to the surface of the sand, or stone, and with a force proportioned to the closeness of the contact. This explanation omits the effect produced by crystallization, which probably acts an important part, the setting of cement being attributable to the formation of hydrated silicates and aluminates, such crystals adhering strongly to foreign substances; and as the more minute the division of the particles, the more perfect will be the subsequent crystallization, an additional argument is presented in favor of finely-ground cement.

The extra cost of grinding cement, to the almost impalpable powder which theory would indicate, can hardly be compensated by the increased efficacy thereby obtained; but it is extremely desirable, and comparatively easy, to discover the limit at which extra grinding ceases to be economical. On the whole, the author believes that cement, of which not more than 10 per cent. is stopped by a sieve with perforations $\frac{1}{50}$ inch in diameter, probably approaches the present economical limit.

In connection with the subject of mechanical agency, the author has made some experiments on the effect produced by sands, of different degrees of granulation, on the strength of cement mortar. Some of the final results, as representing the average of a rather disconnected and tentative series of tests, are given in Table 6. In each case similar sand and cement were used, the various degrees of fineness being obtained by repeated sifting.

TABLE 6.—EFFECT PRODUCED BY FINENESS OF SAND ON STRENGTH OF MORTAR, 2 PARTS OF SAND TO 1 PART CEMENT BY MEASURE.

Age of Samples four weeks.

Sieves Nos. 1 and 2. Sand passing No. 1 but stopped by No. 2.			Sieves Nos. 2 and 3. Sand passing No. 2 but stopped by No. 3.			Sieves Nos. 3 and 4. Sand passing No. 3 but stopped by No. 4.			Sieves Nos. 4 and 5. Sand passing No. 4 but stopped by No. 5.			Sieves Nos. 5 & 6. Sand passing No. 5 but stopped by No. 6.		
Area.	Breaking Weight.	Breaking Weight per Square Inch.	Area.	Breaking Weight.	Breaking Weight per Square Inch.	Area.	Breaking Weight.	Breaking Weight per Square Inch.	Area.	Breaking Weight.	Breaking Weight per Square Inch.	Area.	Breaking Weight.	Breaking Weight per Square Inch.
Sq. ins	lbs.	lbs.	Sq. ins	lbs.	lbs.	Sq. ins	lbs.	lbs.	Sq. ins	lbs.	lbs.	Sq. ins	lbs.	lbs.
2.22	375	169	2.20	292	133	2.34	279	119	2.26	200	88	2.22	198	89
2.22	375	169	2.30	295	129	2.30	270	117	2.22	230	103	2.21	170	77
2.18	375	172	2.32	285	122	2.40	292	121	2.21	220	99	2.21	185	83
2.18	375	172	2.32	287	123	2.25	270	120	2.24	240	107	2.23	210	94
2.22	398	179	2.22	278	125	2.22	284	128	2.24	245	109	2.24	103	46
2.22	396	178	2.22	295	133	..	..	..	2.25	270	120	2.21	135	61
2.25	406	180	..	..	..	..	..	..	..	..	..	2.20	219	99
2.17	456	210	..	..	..	..	..	..	..	..	..	2.25	210	93
2.17	452	208	..	..	..	..	..	..	..	..	..	2.17	208	95
2.23	442	198	..	..	..	..	..	..	..	..	..	2.26	214	94
2.17	363	167	..	..	..	..	..	..	..	..	..	2.20	216	98
2.17	418	192	..	..	..	..	..	..	..	..	..	2.26	216	95
Average..		183	Average..		127	Average..		121	Average..		104	Average.		85

DETAILS OF SIEVES.

		Inch.	
No. 1	circular perforations	0.14	in diameter
“ 2	“ “	0.10	“
“ 3	“ “	0.07	“
“ 4	“ “	0.02	“
“ 5	wire gauze 2,900 meshes to a square inch.		
“ 6	“ 8,200 “ “		

The sand passing through No. 1 and rejected by No. 2 was considerably coarser than ordinary building sand. That passing No. 5 and stopped by No. 6 was of a degree of fineness frequently met with in practice, and known as 'woolly' or 'running' sand.

TENSILE STRENGTH.

As before observed, the tensile strength is the test to which all the others must be subservient. In practice cement has to resist both tension and compression; but its power to resist the latter is so closely allied to the resistance it offers to the former, as to render the test of tensile strength sufficient. In fact, it would appear that this test alone, if properly applied, not only to the cement in its neat condition, but also when diluted with sand, would answer all practical purposes. It is proposed, therefore, to treat this part of the subject in detail, though it may unavoidably lead to some repetition of what is already known.

METHOD OF MINING THE SAMPLE BRIQUETTE.

This determines to a great extent the breaking weight. Everything that conduces to uniformity should be observed, and the mould should be carefully filled with gauged cement, so as to leave no air-holes or other defects.

The following method, adopted by the

Author, gave good results[*]:—after being thoroughly mixed and incorporated by a light (plasterer's) trowel, the cement was packed closely into the mould, heaped a little higher than the edges, and pressure applied by means of a small board held in the hands of the operator; this may be done a second time, after adding a little dryly gauged cement. The pressure forces out any superfluous water, replaces it with cement, and prevents the occurrence of air-holes or other defects.

The application of pressure in the formation of the sample is not inconsistent, for in practice the cement, forming the joints of masonry or the matrix in concrete, is subject to much greater pressure by the weight of the superincumbent work. From a large number of experiments the Author found that the slight pressure referred to increased the strength of the sample more than twenty-five per cent. This result is satisfactory, as it shows that the pressure unavoidable in practice is probably beneficial. It is desirable to adopt every means to obtain the best results, and to develope as much as possible the strength of the cement under test. Manufacturers are liable to suffer some injustice from the want of proper care and skill in the making of test blocks, for two sets of samples of the same cement, presenting the same external appearance, can be made to give very different results, the strength of one being nearly double that of the other at the end of seven days.

PROPORTION OF WATER USED IN GAUGING.

The least possible quantity of water should be used in gauging, any excess having a direct tendency to reduce the strength of the sample. For this reason Portland cement should not be tested while too fresh. If warm, it should be spread out and allowed to cool for a few days, and then be made into test blocks. The cooling process—by which the particles of lime are slowly slaked by the absorption of water from the air—has frequently produced an improvement in the strength of seven-day samples of about forty per cent.

The following table gives some of the results obtained by using various proportions of water:

Table 7.—Cement gauged with various Proportions of Water.

Age of Sample seven days. Average breaking weight per square inch.

	5 oz. Water to 32 oz. Cement.	6 oz. Water to 32 oz. Cement.	7 oz. Water to 32 oz. Cement.	8 oz. Water to 32 oz. Cement.	9 oz. Water to 32 oz. Cement.	10 oz. Water to 32 oz. Cement.
	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.
	433	460	..	306	..	..
	416	435	..	296	..	..
	416	480	..	282	..	..
	344	398	382	363	223	184
	368	366	387	357	219	221
	329	416	353	339	211	197
	487	489	323	316	187	163
	474	447	420	302	234	165
	440	471	336	268	209	162
Averages ...	412	440	367	314	214	182

With 5 ounces of water to 32 ounces of cement the gauged cement was extremely dry, crumbling under the trowel, and could not be made to take a smooth surface; with the proportion of 6 ounces to 32 ounces it was moderately dry, and could be finished and smoothed off with the trowel; with 7 ounces to 32 ounces the mass was moderately wet; with eight ounces to 32 ounces the samples were wet and soft; with 9 ounces to 32 ounces they had the consistency of stiff grout; and with 10 ounces to 32 ounces a liquid was produced which could be poured from one vessel to another. In the last two cases the samples shrank consider-

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[*] Except in the case of very slow-setting cement, only so much was gauged at a time as sufficed to make one sample, the water for gauging being added at one operation.

ably, and must have lost some of the water by evaporation.

The time occupied in setting increased, in proportion to the quantity of water used, from fifteen minutes to about forty-eight hours. The effect of using an excessive quantity of water is manifest in seven-day samples, but it has been questioned whether the deterioration is permanent. Mr. Colson's experiments go to prove that the deterioration continues, the decrease of strength being more apparent in sample six months old than in others only seven days old. The best test blocks were made with from 6 ounces to 7 ounces of water to 32 ounces of cement; with this proportion the gauged mass presented a somewhat dry and granular appearance, but when placed in the mould and subjected to the small pressure before referred to, a little water exuded from under the mould, and the upper surface became soft and capable of being neatly smoothed off with the trowel.

SHAPE OF THE SAMPLE BRIQUETTE.

The form usually adopted (Fig. 1) appears capable of improvement. In the large majority of cases the line of fracture is not at the point of minimum section, but generally somewhat as shown by the dotted line in the left hand figure. This result has been attributed to a peculiar effect produced by change of form on the strength of materials. The Author, however, believes it to be chiefly due to irregular strains produced by the clips holding the sample, which clips, having a tendency to collapse, lead to crushing strains. That some irregular strain of this kind takes place seems probable from the fact, that weak samples ordinarily break at the point of minimum section, while samples of greater strength, requiring the application of heavy strains, generally break through the thicker portion, the latter result apparently arising when the strain on the clips is sufficient to cause collapse. Differences also frequently occur in the breaking weights of samples of similar make and from the same cement.

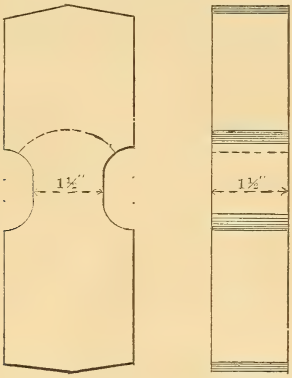

Fig. 1.

In order to avoid the use of clips, the Author had holes drilled in the samples and the narrow part reduced in area (Fig. 2); this reduction was made with a view to prevent fracture through the eye; they were then broken by steel pins passing through the eyes. In this way an increase in the breaking weight of more than twelve per cent. was obtained, the samples in every case but one breaking through the smallest section.

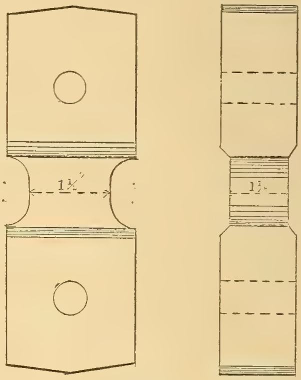

Fig. 2.

Some of these experiments are given in Table 8. The samples on each horizontal line were made of the same cement, at the same time, and were gauged with a similar quantity of water.

Subsequently samples were made of the shape shown in Fig. 3, with which very satisfactory results were obtained; and it is believed that this, or some similar, shape will be found superior to the form now generally in use.

In Table 9 some of the results of experiments made with the new-shaped sample are given. This table shows an increase of nearly twenty per cent. in favor of the proposed shape. In a few instances the samples broke through the eye, but this is attributable to the temporary character of the change in the

TABLE 8.—COMPARISON BETWEEN ORDINARY SHAPED SAMPLES BROKEN WITH CLIPS AND BROKEN WITH STEEL PINS PASSED THROUGH DRILLED HOLES.

Age seven days.

Ordinary Shape. Broken with Clips.			Ordinary Shape. Broken with Steel Pins through Drilled Eyes.		
Area.	Breaking Weight.	Breaking Weight per Square Inch.	Area.	Breaking Weight.	Breaking Weight per Square Inch.
Sq ins	lbs.	lbs.	Sq. ins.	lbs.	lbs.
2.32	790	340	1.800	830	461
2.32	875	377	1.650	755	457
2.36	845	368	1.800	663	368
2.32	884	380	1.830	832	454
2.34	951	406	1.830	834	455
2.30	967	420	1.800	773	429
2.30	910	395	1.545	695	449
2.32	910	385	1.635	800	489
2.36	999	423	1.635	825	504
2.25	999	444	1.570	776	494
2.32	1,087	468	1.680	924	550
2.30	1,026	446	1.650	645	391*
2.34	1,030	440	1.650	773	468
Average..		407	Average....		458

* Badly drilled.

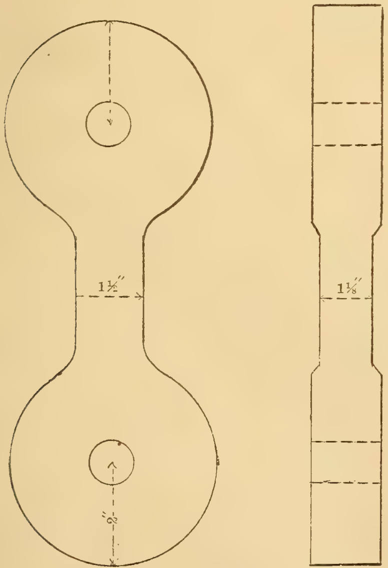

Fig. 3.

TABLE 9.—COMPARISON BETWEEN SAMPLES OF ORDINARY SHAPE BROKEN WITH CLIPS, AND OF AN IMPROVED SHAPE BROKEN WITH STEEL PINS PASSED THROUGH EYES MOULDED IN THE CIRCULAR ENDS OF THE SAMPLE.

Age seven days.

Broken with Clips. Ordinary Shape.			Broken with Steel Pins. Improved Shape.		
Area.	Breaking Weight.	Breaking Weight per Square Inch.	Area.	Breaking Weight.	Breaking Weight per Square Inch.
Sq. ins	lbs.	lbs.	Sq. ins.	lbs.	lbs.
2.27	840	370	2.36	830	351
2.36	790	335	2.36	997	422
2.34	750	320	2.37	820	346
2.24	520	232	1.77	460	261
2.27	645	283	2.30	915	398*
2.40	880	366	2.45	1,120	457*
2.30	655	284	2.36	890	377*
2.25	637	283	2.34	790	337
2.23	590	264	2.28	712	312
2.26	545	241	2.32	717	309
Average...		297	Average....		357

* Broke through eye.

testing-machine to adapt it to the altered samples, great care and accuracy being necessary, in order that the strain shall pass directly through the axis of the sample. The form suggested is similar to that generally used in testing the tensile strength of iron, and there does not appear to be any reason why what has been found most suitable for iron should not also apply to Portland cement. In this case also the samples on each horizontal line were under like conditions as to the cement, time, and quantity of water.

AGE AT WHICH THE SAMPLE BRIQUETTE SHOULD BE BROKEN.

The author has confined his observations almost exclusively to seven-day samples, as that period seems to be universally adopted; but it may be questioned whether this age gives a fair test in all cases. If the cement is moderately quick-setting, becoming sufficiently hard to be removed from the moulds in from half an hour to five hours after gauging, the seven-day test is probably a fair exponent of the quality; with such cement, taking the average of several hundred experiments, the breaking weight of the cement at seven days was nearly always in direct relation to the breaking weight at twenty-eight days. Warm cement can be cooled by exposure, until the time of setting comes within the limits mentioned. In the case, however, of very slow-setting cement, requiring from six to twenty-four hours to harden, the breaking weight at seven days does not seem to bear the same ratio to the ultimate strength as in the quicker setting cement, the latter gaining strength more rapidly, although it may ultimately be inferior to that of the former.

The longer the time between the gauging and breaking of the samples, the more reliable are the results; but whether the advantage thus gained is a compensation for the inconvenience arising from the lengthening of the time seems doubtful. Generally, the cement, as received from the manufacturer, sets with sufficient quickness to enable its quality to be determined by the seven-day test. The difference in the strength of seven and twenty-eight day samples was found, from numerous experiments, to be about 20 per cent.

It might be desirable to introduce along with the ordinary seven-day test one including a longer period, say twelve weeks, so that if the first should appear unsatisfactory, which might occur in the case of very slow-setting cement, the latter could be tried before the cement was finally rejected.

It has been sugested that cement mortar should be made the subject of test, as it is in this form that Portland cement is usually employed in practice. If, however, the properties of cement are understood, and its behaviour when diluted with sand, &c., a test of this description is unnecessary.

STANDARD OF TENSILE STRENGTH.

The standard test, specified at the commencement of the London Main Drainage works, was a breaking weight of 400 lbs. on an area of 2.25 square inches (equal to about 178 lbs. per square inch) after seven day's immersion. At present a tensile strength of 350 lbs. per square inch is frequently demanded and obtained, and it seems probable that the standard may be raised still higher, as the manufacture of the article improves and its properties become better understood.

In the author's experiments, in which the moulds and testing-machine were those in ordinary use, it was not uncommon to obtain a strength of from 400 lbs. to 500 lbs. per square inch after seven days's immersion, and in one or two instances it rose as high as 600 lbs. per square inch. The average of a large number of tests made within the last two or three years has been 380 lbs. after seven days, and 450 lbs. per square inch for twenty-eight-day samples. The fineness of the cement was such that about 7 per cent. by weight was stopped by a sieve the perforations in which were about $\frac{1}{43}$ inch in diameter. Having due regard to the effect of fine grinding on the strength of neat cement, and to the rates at which the strength of different samples increases in the early periods of their age, the author believes that a minimum breaking weight of from 350 to 370 lbs. per square inch is as much as can be reasonably expected from seven-day samples.

In conclusion, the author would observe that the experiments he has been enabled to record are but limited, considering the nature of the subject dealt with. They were, however, made with great care, under his own direct supervision, most of them being spread over the last three or four years.

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Forging Steel.—We would here merely direct attention to a small memoir that has recently appeared, entitled "Remarks on the Manufacture of Steel and the Mode of Working it," by D. Chernoff, assistant-manager of the well-known Abouchoff Steel Works, near St. Petersburg, in Russia, translated by Mr. W. Anderson, M.I.C.E., and published by W. Clowes and Sons, in London. It occupies itself principally with the forging of steel and the means of preventing a crystalline structure developing itself in the mass, which is done by a regulation of the temperature. We must, however, refer to the original for details. This paper has also been reproduced in the *Engineer* and in *Engineering*.

THE USE OF MACHINERY IN ITS RELATION TO ARTISTIC PRODUCTIONS.

By WILLIAM BRAGGE, F.S.A.

From "The Builder."

Last year Mr. Poynter, the President of the Art section of the Social Science Congress, held at Liverpool, in his address to the Congress, objected to the "Castellani" collection of Italian jewelry being sent to Birmingham on loan, because cheap and inferior copies would there be made mechanically, and in this objection Mr. Poynter embodied and expressed a very common idea,—that mechanical work cannot be artistic work.

How far this is true I shall endeavor briefly to show, and I may at once state that I shall claim for all mechanical appliances the right and privilege of assisting in our art manufactures. I am prepared at once to admit that the enormous industrial progress of this country during the present century has really done very little for art. Our manufacturers have, almost to the present time, given their whole attention to economy and rapidity of production, and the public taste has not been elevated by the style of the articles produced. But allowing that in past and even at the present time, the taste and skill of the artist have been lost,—sacrificed to the ingenuity and contrivances of the mechanican,—we surely need not conclude that art cannot become an ordinary adjunct to mechanical reproduction. We know well that in many manufactures carried on by us to-day our best models in design, as well as in material, are those bequeathed to us by workers who lived centuries ago, whose mechanical appliances were of simplest and rudest character; and it is with some sense of humiliation that we are bound to confess ourselves unable to equal not merely the works of three or four centuries ago of European civilisation, but even of those of the present day, produced by nations which we are conceited enough to call savage. Let us take a few examples of the arts and manufactures of old time and compare them with those of the present day. Have we improved in the art of printing since its invention, 400 years ago? or in the art of paper-making of 600 years ago? In quality, certainly not; we are bound to remember that to-day an ordinary edition of a newspaper is printed on both sides in quantity sufficient to cover an acre, in the same or less time than Caxton or Guttenberg required for the production of a single sheet. We produce paper now in continuous rolls of many hundreds of yards in length, and with a rapidity commensurate with the voracious requirements of the steam printing-press; but no paper now in general use (excepting bank-note paper) equals in quality that upon which our early books are printed. Most of the books printed of late are, I think, likely to be entirely lost within a century, simply from the natural decay of the (so-called) paper upon which they are printed. But we can to-day buy a newspaper for a penny or a book for a shilling, which in Caxton's time would have cost fifty or a hundred times as much. We can and do engrave dies and coin money for all the world, but we cannot produce anything comparable with the exquisite coins of ancient Greece. We can imitate in a feeble way the cameos and intaglios of the stone engravers of ancient Rome; but this is an art almost unpractised amongst us, and so far as we are concerned artistically, it is practically lost. In ornamental enamel we must go to China and Japan for our models, as Messrs. Elkington have wisely done, and then find out by painful experience how best to copy the humbler objects, before daring to imitate the more important. Our English enamelling of to-day is confined pretty closely to the decoration of mayor's chains, Masonic emblems, and the lining of iron pots and pans. The art of lacemaking, or ornamental needlework —in Mediæval times one of the most general occupations of women of every rank above the lowest,—is now almost entirely lost, and we must go to our museums to see the charming examples of taste in design and perfection in work which still remains for us to admire.

The hand-made lace will bear microscopic examination, consumed much time and patience in its production, and was consequently very costly. Now-a-days one can buy in Nottingham embroidered curtains enough to furnish a house for less money than a collar of ancient point lace would cost. I might indefinitely prolong this list, and point out to you the comparatively lost arts of damasquining, or inlaying iron and steel with the precious metals; of the special forging of sword-blades in Damascus and Toledo to produce the twist in the fiber, so to speak of the metal; of the remarkable skill in perforating steel and brass as a decoration of useful objects practised in Persia; of the marvels of design, color, and glaze of early Italian pottery or majolica; of the exquisite glass and mosaics of Venice; the gold and silversmith's work of Italy; the shawls of Cashmere, and textile fabrics generally of India; but further instances are not needed, and you will, I think, agree with that which is a commonly accepted opinion, that in all these, and in many other artistic, manufactures, we, the English, are now utterly unable to compete. I have not made in the foregoing remarks any reference to painting and sculpture, because these arts stand outside of the reach of mechanical appliances, and therefore the painter and sculptor of to-day are in this respect in the same position as were Apelles and Phidias, Titian and Michelangelo. And now we may proceed to consider what is the real or supposed antagonism between art as developed in individual workmanship, and as accompanied or assisted, by mechanical appliances. If we fairly compare the art of, say, the goldsmith or silversmith of three centuries ago with the art of to-day, we find that the workman then lived and worked under influences and incitements which have long ceased to exist. The Mediæval workman thought and cared only for the single object upon which he was employed. He threw his whole soul, his whole inventive faculty, his whole technical skill, into his darling work, and he knew that no meaner hand could rob him of the fruits of his patient labor and skill. He was not hurried with his work. He had no anxieties as to cost. His master, if he had one, was not perpetually crying out for the economies of labor and of material, no estimates had been given which could not be exceeded, and no arbitrary percentage of profit had been beforehand fixed. Is it extraordinary that under such favorable conditions the workman should produce his best? Certainly not, and we ought to be profoundly thankful that the results of such a state of things have been preserved to us for our special instruction. How does the working gold or silver smith of to-day stand in relation to his work as compared with his predecessor of three centuries ago? Truly in a miserable plight! Instead of being himself the creator of the design he has to carry out, it has probably been prepared for him in an office where the commercial economy of manufacture is more considered than the principles of art, where facility of production is held to be more important than elegance of form, and where the demands of the Demon of Fashion override the desires of good taste. All the patient labor of hammer and chisel, slowly and surely giving expression to the taste of the individual, are dispensed with now, and are replaced by a few blows of the stamping-machine! This stamping-machine or press has taken the place of hand labor, and being only a machine, it is perfectly indifferent whether the work it is called upon to do is in good or in bad taste. It is as willing to work for the demon of ugliness as for the spirits of beauty, and the work which it produces is simply a reflex of the mind, and taste, and quality of its employer. It is unfair to charge the mechanical appliances of manufacture with having caused a degradation of taste. The degraded taste existed when machines were first employed, and, unfortunately the process of refining and improving the artistic feeling of the public has been hindered by the cheap and ugly mechanical productions. If the modeler will only produce moulds worthy of admiration, the stamping-press is at his command to bring under the daily notice of every one the forms of beauty which he has himself designed. We cannot, if we would, go back to Mediæval habits of thought or modes of work, and those who to-day would abolish all mechanism in art-manufacture would injure instead of helping their cause. The patience and skill of the modeler and chaser who wishes to devote months or years to the production of a master-piece can still be given. He remains free to use his time and talents as he thinks best, and happily there is yet a demand for works of a high class in the production of which such men may be congenially employed. In this wealthy and art-loving country there is abundant occupation for the most skilful artists. Nothing that is really good fails to find a ready purchaser, and talent has to-day as fair a field before it as it ever had in days gone by. The gifts of corporate or public bodies to men who deserve honor,—the production of prizes for excellence in exhibitions, for racing, or boating, or yachting; the demand of the wealthy for specialties, designed for themselves, ensure for the conscientious workman full scope for his talents. And I am sorry to be compelled to think that this special work is more than enough to occupy all the competent workmen who can be found. In the production of such works machinery and mechanical reproduction have no part, they stand as individual works, each wrought out by a master mind, to fulfil a single purpose, and to satisfy a personal want. But can the requirements of the million be dealt with in this aristocratic manner? Certainly not. For their wants every element of economy must be utilised, the means of production must be assisted by all the resources of mechanical science. The absolute perfection of detail must of necessity be given up, but there is absolutely no reason why a high standard of excellence should not be preserved. Our jewelry, our gas-fittings, our table services, whether of silver, silver-plate, or other metals; our trays, coal-scuttles, fenders, and grates, and everything else which forms a part of our domestic surrounding, may as cheaply be made elegant as ugly. The use of machinery has only to be wisely and thoughtfully adapted, governed by artistic principles, and its result will be to give to the world in its cheapest form the useful and the beautiful. To the working out of this desirable end, I commend you, the students of the Sheffield School of Art.

THE PRESENT STATE OF INDUSTRIAL ART.*

By CHARLES L. EASTLAKE.

From "The Architect."

We have been in the habit for two or three generations past of dividing art into several sections, and of giving them grand names. There is High Art, for instance, and Fine Art, and Pictorial Art, and Textile Art, and Ceramic Art, and Industrial Art and what not, and of these some have been foolishly over-rated on sentimental grounds, and others have been foolishly under-rated on ignorant grounds; and the most curious fact is that all these fine names were invented or adopted during a period when taste in all art had sunk in this country to the lowest standard which perhaps it has ever reached since civilization began. I mean in the early part of the present century. Of course, in saying this, I except such noble instances of artistic power as Turner, Flaxman, and others whose genius shone like stars in the surrounding darkness, but the full excellence of even their work has only lately been appreciated as it should be, while the minor arts, as we are pleased to call them, were clean forgotten in those days or regarded as mechanical and unimportant. Amidst all the tall talk about the grand schools of painting and sculpture, amidst all the foggy theories and pompous dissertations which were imported from Germany about this and that style, and in spite of dilettante clubs and learned societies the arts of manufacture sank lower and lower in character of design, and as a matter of consequence in quality of execution. For you may take it as a fact that speaking generally the worst taste in manufacture is generally found in articles of flimsy workmanship, and the best taste in those of sound honest handicraft, all the world over.

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* A Lecture read before the Spitalfields School of Design on March 22, 1877.

The truth is, that while connoisseurs were theorizing about the old masters, while *virtuosi* were examining Greek vases and puzzling their heads to fix the precise period at which each was made, it never seems to have occurred to them that their time might have been better employed in encouraging the art of their own day.

Now don't let me be misunderstood on this point. No one has a greater reverence and admiration for ancient art than I have, and I only wish that all who hear me at this moment could spend their Saturday afternoons alternately at the National Gallery and the South Kensington Museum, both of which contain priceless treasures that cannot be examined too often or too carefully; but do remember this, that no centuries of time can ever add to the merit or detract from the excellence of those works. Those which are admirable now are to be admired, not because of their age or associations, however much our interest may be enhanced on that score—but on account of the skill displayed in their design and execution—just as they stood on the painter's easel, or were displayed in the potter's shop, or were carried away from the blacksmith's forge. There were degrees of excellence then as now, and of course some failures, and when we see and recognize them, whether in a picture frame or a cabinet, we should have the courage to say so, whatever their antiquity may be.

Now the connoisseurs of our grandfather's time talked a vast deal about the Classic school of landscape-painting, about composition and chiaroscuro, and Heaven knows what besides, but they let two of the greatest landscape painters the world has seen—William Turner and David Cox—work for years without finding out their merits, and I could mention others who, at the outset of their career, had to encounter similar difficulties.

The moral which we may draw from all this is, I fancy, plain enough. If a genuine love of art exists at anytime it will be found to extend forwards as well as backwards—in other words, it will encourage excellence in contemporary work and not content itself with hoarding up examples of a bygone age for the mere sake of their rarity, or what I fear is too frequently the case for the sake of their market value.

Further: I may say that a genuine love of art, whether in a nation or as an individual characteristic, should represent something more than the wealth of a museum or the pride of a collector. It should reach and invest with interest the home of every householder, however humble, throughout the land. One of the greatest historians of our time, Lord Macaulay, in the first pages of his greatest work, made scornful allusion to that absurd phrase—"the dignity of History," and pointed out that a history which did not include some notice of the domestic life of our ancestors would be no history at all. Now I think there is an equally absurd phrase, which is of recent origin and which is equally open to ridicule, and that is the "dignity of art." That expression, as commonly understood, means this—that art is a luxury which can only be enjoyed by the wealthy and highly cultivated; that it is the special property of the collector or the *savant;* that those who cannot afford to buy expensive pictures or rare specimens of bric-a-brac must be content with ugly houses and commonplace furniture. This melancholy and selfish creed is, I hope, dying out in our country; but depend upon this, that while a vestige of it remains, there will be little chance of improvement in really national taste. It is precisely because art has been looked on in this light, and talked of as something beyond the reach and appreciation of the people, that the people have grown indifferent to it, and have come to regard it as a thing of the past, the traditions of which are preserved by the world of fashion like armorial bearings or a footman's livery.

Now of course it is very difficult for us in these days to ascertain how far the working classes of any nation in by-gone times enjoyed the highest and most refined achievements of art; but one thing is certain, that down to the end of the seventeenth century the difference between what we now call "high art" and the ordinary arts of manufacture was only a difference of degree and quality—not one of style. The houses of the wealthy in those days were, of course, furnished with articles of costly material and elaborate workmanship, which were beyond the reach of humbler citizens, but the mode and manner of their execution was the same throughout the land, and the consistency of taste pervaded all the arts, from architecture down to the mere fashion of dress. How the change came about which we recognize in the present day; how the line came to be drawn between high art in some quarters, and no art at all in others, is a very complex question, which, to investigate thoroughly, would involve considerations beyond the scope of this lecture. It may, however, be well to point out that among the causes which have led to this result are the progress of education, and the rapid increase of wealth, which have taken place within the last century.

I am afraid this will sound rather startling to many of you until I explain my meaning, but there is really no cause for alarm, for the social progress of a nation is of infinitely more importance than the improvement of public taste; and if one has interfered with the other for awhile it only remains for the present generation to devise some means by which these two objects can be attained at one and the same time.

In former ages, as you know, the habits of the working-classes were far simpler and more domesticated than they are at the present day. Men, who in country towns and villages plied their trade as joiners, weavers, blacksmiths, or what not, were content to work as their fathers and grandfathers did before them, and generally brought up one or more of their sons to the same business. In this way the traditions of their craft, whether in regard to design, technical knowledge of material, or method of work were preserved, and as all these improved with experience, you may suppose that the result was most beneficial to every branch of industrial art. But by and by many changes came about. Railroads were laid down; schools were set up ; cheap newspapers were circulated; some of these honest folks began to get ambitious; they traveled; they read the papers; they sent their boys to school. All this cost money, and the consequence was they were obliged to do more work in the same time. Of course the work was hurried, and not so well done. The sons of these men, being better educated than their fathers, began to grow conceited, and look to down upon their fathers' work. Sometimes they went off to "better themselves," as the phrase is, and were replaced by journeymen, who being paid by the day, and having no interest in the business, naturally thought more of their wages than their work, and moved from place to place whenever they saw a prospect of higher pay, leaving the work to fresh and inexperienced hands. Nor was this all. As work went on increasing, for the sake of wage, population went on increasing too, and unfortunately at a far greater rate than money. Provisions, clothes, and many of the essentials of life became dearer. More work became necessary to pay for them, and at length masters themselves began to consider how they could execute the same amount of work with less trouble, or a greater amount of work at a cheaper rate.

The result of all this, as you may suppose, was to substitute an inferior class of handicraft and material. Cast iron took the place of wrought. In joinery, mouldings, instead of being worked in the solid, were run out by machinery and planted on. Common druggets were printed in imitation of woven carpets. Brickwork was "scamped," and plastered over with stucco or cement in imitation of stone. Deal was grained in imitation of oak or marble. Crockery, which had been formerly painted by hand, was decorated with printed patterns. Much of this work and many of these articles being of course cheaper than what had formerly been produced, suited the pockets of pretentious people, who like to make a show at a small cost; and the consequence was a gradual increase of demand for what I will venture to call cheap and nasty work, and a neglect of those honest trades which were once the pride of the British workman.

The traditions of some few, indeed, have survived, probably because no shams could possibly be practised in them without at once rendering the objects produced useless for their purpose. To this day I know no specimen of native manufacture more satisfactory in construction and more picturesque in appearance than a rustic cart. It is always solidly made of stout timber and well hammered iron. Such little graces of decoration as its purpose admits, in the way of turned woodwork, chamfered edges, or applied color, are simple and unaffected. It serves its object well and honestly, and has filled the page of many an artist's sketch book. And yet the maker of that cart may perhaps have never learned to read or write, and knows no more of industrial art than he does of the Differential Calculus. That is the kind of work which we should try and revive in this country; that is the kind of work which no school of design can improve.

Pray don't suppose that I am putting an extreme case, or that I wish to say a word in disparagement of this and many other excellent institutions which have become a necessity at the present time. All I desire to point out is that their object should be to start from the principles which guided our forefathers in design and construction. When we have mastered those principles and learned to work well and thoroughly in any one style of a bygone age, it will be time enough to think of inventing a new style, about which ignorant people often talk, and with as much reason as if they proposed to invent a new language.

Take, for instance, the noble decorative art of Japan as it has been applied for centuries past to the design of pottery, of lacquered work, and of textile fabrics. That is something like industrial art, if you will, and what is the secret of its excellence? It is traditional work handed down from generation to generation, with infinite varieties of form and color it is true, but with no change of style in the ordinary acceptation of the word.

Now, mark what has happened. The progress of civilisation goes on. Commercial enterprise and increased facilities of travel afford opportunities of intercommunication between Europe and the East. Japanese goods are brought to the English market, and specimens of British manufacture go to Japan. All very satisfactory in the interests of trade no doubt, but meanwhile the Japanese artist, after examining the work produced by this great and enlightened English nation, and perhaps getting a hint or two from our merchants, comes to the conclusion that he had better modify his taste to suit our notions of elegance, and the result for years past has been a gradual deterioration of Japanese art.

This is a melancholy state of things, for which it would be difficult, if not impossible, to devise a remedy, and if I dwell upon it, it is to show you the fallacy of supposing, as many excellent and well-meaning persons do, that the progress of good taste in manufacture must necessarily go hand in hand with what used to be called the march of intellect. The very fact that we have learnt so much, that we have brought before us so constantly at international exhibitions, and by means of published works, specimens of the art of other nations, puzzles the designer as to choice of style, and makes him borrow a hint from this and a trick from that school of taste. Conditions of form and schemes of color, which properly belong to one branch of manufacture, are too often imported into another, and we may congratulate ourselves if the result is not a hopeless jumble of ill-assorted materials, misapplied ornament, and inappropriate construction.

How can we escape from these difficulties? It is easy to say, "Go to the South Kensington Museum and study the lent examples of ancient art." But I cannot fancy a more bewildering task for the student—I am speaking of beginners of course—than to wander through that palace of countless treasures, including specimens—good, bad, and indifferent—of every age and nation, in the hope of improving his taste. It is true that he may join the schools and study types of ornament, but the study of ornament in the abstract, and without an adequate knowledge of the mode in which that ornament should be applied, may teach him to draw indeed, but will never teach him to design. I would rather say—make up your mind to study one branch of manufacture at one time and ascertain, as you may easily do, the best period and most notable country for that manufacture.

Choose the simplest objects first, and draw them (I assume that the student has learnt to draw) geometrically, giving an accurate profile of every vase or casket, mapping out the ornament on its surface, and making memoranda of any color decoration. In furniture and metal work examine carefully the construction of the objects, and plan it where possible, show every joint or rivet. Make full-sized sections of the mouldings, and figure every dimension on the spot. In examining old stained glass note the disposition of the lead lines, and in woven fabrics the direction of the threads, with as much care as you bestow on the figures or patterns themselves. Studies such as these will be of more value for future reference and of far more help towards mastering the proportions of design and technical details of manufacture than any perspective sketches however elaborate.

In selecting objects of ancient manufacture for this kind of study I would strongly advise you to choose those which in some form or other are still serviceable under the conditions of modern life. No doubt in many instances they may not be so attractive or interesting as others whose purpose has become obsolete, and the very quaintness of old fashioned articles, reminding us of the habits of our forefathers, will invest them with a certain charm which it is difficult to resist. But after all, if our object be to improve the taste of modern industrial art, it seems waste of time to perpetuate types of form which are alien to the necessities of our own day. By-and-by no doubt you will be able to glean hints for decorative design from such objects, which may be usefully transferred to those of everyday use; but, speaking generally, and especially to beginners, I would recommend you first to confine your studies to that class of articles which a shopkeeper would find saleable if reproduced for the modern household.

There are hundreds of such articles now exhibited in our museums, and it has always been a matter of astonishment to me that manufacturers who at a trifling cost might provide themselves with models or drawings of these objects for imitation, prefer to bring out year after year what they call "novelties," and which, while departing widely from ordinary types of form recognized by custom, are far from realizing the grace of ancient art. Some few years ago I lent the proprietor of a well-known china shop a pretty example of old Italian majolica for table use, which I thought might be reproduced at a moderate cost. The design was picturesque and good of its kind, and the china merchant was delighted with it. Well, this piece of majolica–it was a saltcellar—was sent down to the potteries to be copied, and, after waiting some weeks, I called to see whether the model had arrived. The china merchant said he was very sorry, but there was always a little delay about these matters. It had not been put in hand, but he would write about it. He did so, and I waited for several weeks more. At last I called again, but it was not ready. More letters passed, and at the end of six months the model arrived, very correctly imitated in form, but with a plain white glaze on the ware. The pretty Arabesque pattern on its surface had not been added. I had to wait three months more for that, and when the saltcellar came from the potteries I found that the local artist had tried to improve upon the original by shading up the ornament in a modern fashion, quite different from the original. Well, I said this would not do for me. I wanted the decoration copied line for line. Back to the potteries went the saltcellar, and I had to wait three months more. At length, after exactly a year had elapsed from the time when this piece of majolica left my house, I obtained a very faithful copy of it, and congratulated myself on having helped to revive a good design. But on inquiring the price I found that each copy would cost about four or five guineas. I had given as many shillings for the original in Italy. Now whether it was due to the costliness of the reproduction, or the caprice of the manufacturer, I do not know, but certain it is that although I have frequently visited the shop since, I have never seen this reproduction offered for sale, and I might just as well have never troubled myself about the matter. Observe that this was not a question of much difficulty, nor need it have been one of much cost. The French would readily have reproduced the article in their Gien ware, and at a third of the price. Indeed, every season I see in the same shop articles requiring quite as much care in production, with surface patterns far more elaborate, but they are novelties of nineteenth-century design, and, for some mysterious reason, are preferred both by the British manufacturer and the British salesman to any copy of ancient art.

I do not say that this infatuation extends to all branches of manufacture. In cabinet work, for instance, there has been a marked improvement of late years. The coarse carving and lumpy machine-made ornament which disgraced our furniture some twenty or thirty years ago, are being gradually replaced by delicate mouldings and graceful inlay. Hand polish is taking the place of heavy varnish. English oak, American walnut, and ebonized mahogany are extensively used, and often with great success, in the design of sideboards, mantelpieces, wardrobes, chairs, and tables. Many of these articles, although designed, and with good reason, to suit the requirements of modern life, recall some of the graces of ancient woodwork. There is indeed a tendency to prettify them unnecessarily, and especially when colored decoration is introduced, in panels, etc., the design is apt to be restless and fussy in character; but on the whole these examples of furniture are picturesque, and often exhibit evidence of an educated taste which was unknown in the last generation.

In curtain stuffs and printed goods there has been a still greater advance as to quality of design, and this in regard not only to the form of pattern but to the scheme of color adopted. Dyes and subtle combinations of tint are produced which would have been pronounced impossible a quarter of a century ago, and which, if produced, the great authorities on color would probably have scorned. For there were wonderful theories extant in those days about chromatic scales and the proper proportion of primaries to tertiaries, and complementary tints; and a general impression seemed to exist among the learned that the art of decorating in color was something which you could work out by arithmetic.

I devoutly hope that those far-fetched theories have been discarded by this time, together with all similar rules which attempt to apply a scientific test to the achievements of inventive art. If a designer truly loves color, his eye will always be in search of it, and by degrees he will learn almost instinctively why such and such combinations are harmonious and pleasing, while others are crude and unsatisfactory. In that and every other branch of design, it is well to bear this rule in mind, that whenever you have to judge between two distinct kinds of effect, one depending on extravagance of form and startling contrasts, and the other on delicacy of outline and sober hues, you will generally be right to err on the side of moderation. Very clever designers, by dint of long experience, learn the art of surprising agreebly, but all surprises are not agreeable; and just as every sensible man is won more by quiet argument than by vulgar rant, so most of us will, I think, prefer temperate to restless decoration. While on the subject of color, I cannot help saying that the more I study its effect and value, whether in pictorial or decorative art, the more convinced I am that its application will be found most harmonious in instances where one dominant hue is found to which all others are subordinate. You have all heard of that famous picture—Gainsborough's *Blue Boy*—which is the delight of every painter and connoisseur. Now why has this portrait such extraordinary attraction? Chiefly, I venture to think, because it has this quality of chromatic unity. You recognize it at once as a blue picture. I don't mean that it is all indigo, or cobalt, or French blue, or Prussian blue. It may pass from one to another of these shades, and include gray, white, and green. You may get warmer hues of pink and brown in the flesh tints and background, by way of delicate contrast, but the prevailing tone is definitely *blue.*

Well, I think that our rooms should be decorated on this principle, not in the upholsterer's sense of harmony by covering all his furniture with stuff cut from the same piece and by hanging up curtains to match, but by making one color dominant and ringing a variety of changes on it. In this way yellow might lead up to green, silver-gray up to purple, and Venetian red up to brown, but the subordinate tints in each case should have a certain affinity to the dominant color, and when you have settled all this you will find that any little bit of contrast introduced, provided it be unobtrusive and does not interfere with your scheme, will have a cheerful rather than a discordant effect. And this principle concerning the decoration of a room may be safely applied, I think, to all departments of design in which the element of color is a leading feature; as, for instance, in textile fabrics, paperhangings and the surface patterns of pottery and china. Wherever you find two or more colors introduced in such even proportions that you are puzzled to know which *rules*—so to speak—be sure the design is bad.

In the three branches of design which I have just mentioned a great change has taken place within the last few years, as no doubt many of you have noticed. I mean the substitution of natural forms of ornament chiefly borrowed from vegetable life, for the geometrical patterns and diapers which were once in vogue. This change is no doubt due to a reaction in architectural taste from mediæval types to those which prevailed during the seventeenth and eighteenth centuries. I have not time to go into this question of architectural taste now, and indeed it would be beyond the purpose and scope of my lecture, but it is quite certain that this change has taken place, and that it is gradually affecting the character of design in all branches of industrial art.

Now this will necessitate more than ever on the part of designers a study of natural forms in plants, animal life, and, indeed, of the human figure itself. In pursuing these studies I cannot remind you too emphatically that a literal imitation, or rather a literal representation, of natural forms has never in the best ages of art, and never will, constitute a good decorative design. To a certain extent, every person who can lay claim to a taste at all admits this principle. A marble bust, painted up like wax-work, a group of dogs, after Landseer, depicted on a hearthrug, or a shaded representation of a rosetree on a paperhanging, would at once be condemned as vulgar and ignorant by people who have had no artistic education at all. Well, then, the question arises how far, in the adaptation of natural forms to decorative purposes, you may go in the way of imitation consistently with good taste, and a very difficult question this is to answer in a definite way.

Perhaps the broadest and safest rule to follow in adapting natural forms to decorative purposes is to omit all expression of light and shade. I don't mean that you should be debarred from indicating, let us say the rotundity of buds or fruit, for instance, by a few dark threads in textile work, or a few "hatched" lines in the design of a wall paper, but there would be no attempt to relieve one object against another, or to give an effect of distance by the opposition of light and dark colors, and, above all, there should be no receding backgrounds or perspective in the pictorial sense of the word.

This rule is generally observed in the best curtain stuffs which are now designed, and to a great extent in the design of paperhangings also, but I have often observed it violated in the case of mural decoration by tiles and *plaques*, where I think the introduction of chiaroscuro and perspective is a great mistake. It is true that we find authority for it in some periods of Italian art, just as we find authority in the Middle Ages for painting the internal stone-work of our cathedrals, with patterns from floor to roof; but these are instances when, if ever, we may appeal to the critical philosophy of modern taste for our guidance, and most modern critics, who have given attention to the subject, agree on this point.

Among other branches of industrial art there are three which deserve especial mention, viz., stained glass, ornamental metal work, and the manufacture of pottery and porcelain—each of which has within the present generation been marked by extraordinary improvement either in design or manufacture, though not to an equal extent in both. I have purposely refrained from mentioning names in this Paper, but there are certain firms with whose work as glass-stainers you are all familiar, and who deserve the greatest credit for the high artistic excellence of their inventions. Personally, I think that academic accuracy in the delineation of the human figure is of small importance in a stained glass window, and for this reason: The very conditions of the art are not only removed from, but opposed to, pictorial treatment. In pictures, as Mr. Ruskin once deftly pointed out, the lights are opaque and the shadows transparent. In stained glass it is exactly the reverse. The highest light is the most transparent portion of the glass, and the darkest shadow is represented by that which is most obscure. In short, a stained glass window is less of a picture than a piece of tapestry. This being so, and inasmuch as in stained glass a judicious arrangement of color is the one essential for excellence, it seems unreasonable to hamper the design by the introduction of forms which prescribe any limit to that arrangement. One of the secrets of excellence in early glass was that the old designers made form entirely subservient to color. The quaint angular outlines of ill-drawn figures lent themselves more readily to a free association of cunningly opposed tints than if the artist had been restricted by considerations of anatomy and perspective.

Apart, however, from this question, and accepting the fact that modern designers do aim at correctness of form in stained glass, I think we must all admit that many works of the present day do realise great beauty of color, and I for one entirely approve the spare use of positive tints, which may be reckoned among its characteristics; for nothing can be more destructive of effect in internal decoration than a flood of crudely variegated hues pouring in from a window.

In the design of ornamental metal-work there seems to be no lack of inventive power, and though, being chiefly used for ecclesiastical purposes, it generally assumes a mediæval form, efforts have been recently made to produce articles of domestic use which shall partake of a character more in unison with the latest phase of architectural taste. In point of execution it would be the better for a little more refinement in hammered work. Sheet metal might be used in many cases where castings are now employed, and if this should result in substituting a happy freedom of outline for the mechanical accuracy which still seems to be the ignoble aim of the modern workman, it will be all the better for the interests of art.

The revival of majolica ware is an incident in the history of British manufacture of which the present generation may well be proud. In quality of material and skill of manipulation it leaves little to be desired. It is, however, far more expensive than it need be, and this is because the artists employed in its decoration insist in finishing their work too highly. Old majolica, intended for domestic purposes, was not treated in this way, but sketched upon in a free and easy fashion, and though we find elaborately painted plates and dishes in our museums, they may be regarded as exceptions to the general rule—articles to be hung on a wall, as we hang water-color drawings now, and not destined for ordinary use.

The French in reviving this manufacture have had both objects in view; and while their more costly productions are of the kind I have mentioned, they also supply to the public a cheaper class of goods printed in imitation of Italian faïence and old Rouen ware which, in point of design and price, we have not yet rivalled. English porcelain is perhaps the best in the world so far as quality of material is concerned, but the dinner and tea services which we see in our shop windows are still far from satisfactory in design, and the thick lustrous glaze with which they are covered is, to my mind, quite inconsistent with artistic excellence of effect.

I should like to have said something more on this subject as well as on the design of dinner plate and other fields for the exercise of "industrial art;" but I fear I have already exceeded the limits of the time allotted to me, and perhaps too the limits of your patience.

In conclusion I will only record my conviction that a time has arrived when the public of this country are becoming sincerely interested in the progress of "household taste," and that it will rest with designers educated in this and other schools of the same kind to determine by their efforts the direction which that taste should take and the success by which its development may be attended.

◆

A Shovel Manufactory.—The Ames Shovel Manufactory at North Easton, Massachusetts, gives employment to 500 persons, who turn out 500 dozen of shovels, spades and scoops daily, which will find their way all over the country as well as into foreign lands. The company's shops in North Easton are built of stone, and if placed in a line would extend for half a mile. In addition to the work done at North Easton, shovels are made at Canton, South Braintree and West Bridgewater, and are brought to North Easton for the finishing touch.

THE ST. GOTHARD TUNNEL.

By Prof. COLLADON.

Translated from "Revue Universelle des Mines."

The St. Gothard Tunnel has no comparable precedent, excepting that of Mount Cenis, finished in 1871, and the Hoosac Tunnel, completed in 1874.

The gallery of Mt. Cenis, 12,233 meters long, was undertaken by eminent engineers at the expense of the Government, and for which no necessary pains have been spared. It has taken *thirteen and one half years* for its completion.

The total length of the Hoosac tunnel is 7,634 meters, and the mean progress was less than that realized at Mt. Cenis.

The St. Gothard tunnel, constructed in harder rock, and 14,920 meters long, is a Swiss enterprise, and, according to contract, must be completed in eight years, or nine at the most.

On account of the excess of length and the shortness of the time allowed, this tunneling must be accomplished six times as fast as that of Mt. Cenis.

Can this gallery of St. Gothard be completed in eight or nine years is the main question which justly occupies the mind of the industrial world.

This question of *time* depends upon the mode of execution, and involves some technical principles upon which engineers are far from agreeing.

A tunnel having a double roadway, as that of Frejus or Gothard, requires an excavation of eight meters in breadth and six meters in height clear of the masonry.

We will not immediately consider this large section, but merely of a small gallery, having about $2^{m},40$ in height and $2^{m},60$ in breadth, and kept about 200 or 250 meters in advance.

This small gallery is bored by means of a machine worked by compressed air, which produces at the same time power and ventilation, according to the process proposed by M. Colladon, in 1852 for Mt. Cenis. The boring apparatus, called perforators, invented by Bartlett in 1855, and modified by the celebrated Sommeiller in 1857, have become much multiplied and improved; we can count, to-day, twenty or twenty-five different modifications of this apparatus.

The two methods, that of boring the small gallery at the top, and that of boring it at the bottom of the great section have their more or less exclusive partisans.

The Mt. Cenis tunnel was commenced at the bottom; the Hoosac tunnel adopted both systems; M. Favre prefers piercing St. Gothard at the top, furthermore he employs the machine drill both for the advance gallery and for the lower stages. The rapidity with which the work progresses indicates that it is a good method. The first gallery (*galerie d'avancement*) is bored to the height and breadth of about two and a-half meters, having a section of six or seven square meters. As the entire tunnel will be arched, it is necessary to excavate more space for the masonry in order that the roof of the gallery may be $6^{m},50$ or 7 meters above the bed of the proposed railway.

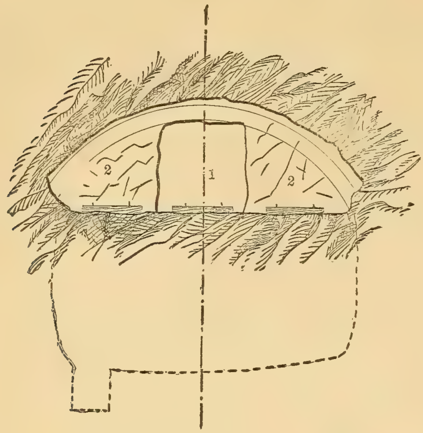
We know that tunneling by the use of powder or dynamite is very dangerous for the engineers and workmen. The excavations are therefore made in separate places and at distances necessary for the safety of the men at the apparatus.

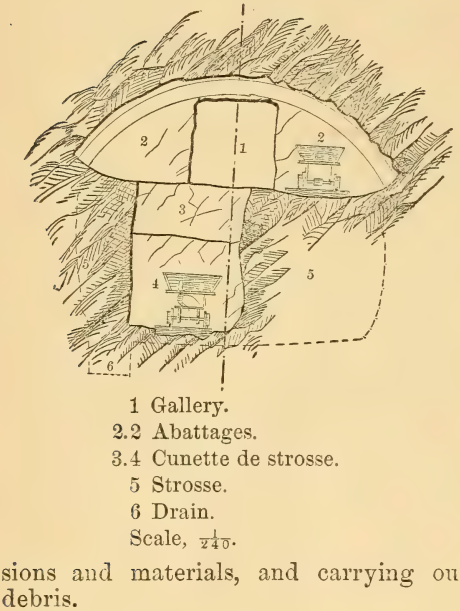
The end of the gallery is called the working place or heading. To the right and left of the gallery, and from 200 to 250 meters in the rear of the heading, are excavated two segments or side areas to the full width of the arch. These are called *abattages.*

200 or 300 meters behind the *abattages* is opened a ditch (*cunette du strosse*) nearly to the floor of the tunnel, that is to say four or five meters below the floor of the gallery; it is about three meters wide. In the rear of the heading of this ditch are excavated the lateral portions (*strosse*), which, when excavated, opens the entire section and the masonry is begun.

The *abattage* and the gallery have their own special little railway, another is built on the floor of the *cunette.*

Numerous cars are continually running on these roads, bringing in tools, provi-





1 Gallery.
2.2 Abattages.
3.4 Cunette de strosse.
5 Strosse.
6 Drain.
Scale, $\frac{1}{240}$.

sions and materials, and carrying out debris.

It is clear that the progress of the entire work depends on the progress of the gallery. At this heading the rock encased on all sides resists the effort of the explosion, and but few men or machines can be employed here, whilst in the enlarged portion of the tunnel the number of both may be very much increased.

The progress realized at the heading by M. Favre and his able engineers is very wonderful, especially if we compare it with that which has been done in rock of similar kind.

It is right to believe that this advancement progressed in the small gallery of Mt. Cenis to the end. For the years 1868, 1869, and 1870 the heading was advanced, 1320, 1431, 1635 meters, affording a maximum of 409 meters per quarter.

At Mount Hoosac, notwithstanding the employment of nitroglycerine and more rapidly acting perforators than those at Mt. Cenis, the total quarterly advancement for the latter years have been: 207 meters in 1870, 238 meters in 1871, 237 meters in 1873.

Here are the figures of the progress in the last five quarters at St. Gothard.

	Göschenen.	Airolo.	Total.
From July 1 to Oct. 1, 1874,	$321^{m},70$	$174^{m},10$	$495^{m},70$
From Oct. 1, 1874, to Jan. 1, 1875,	$283^{m},60$	$243^{m},30$	$526^{m},90$
From Jan. 1 to April 1, 1875,	$267^{m},90$	$289^{m},10$	$557^{m},00$
From April 1 to July 1, 1875,	$312^{m},10$	$344^{m}20$	$656^{m},30$
From July 1 to Oct. 1, 1875,	$360^{m},90$	$326^{m},20$	$687^{m},10$

The prophets had announced that M. Favre, hindered by the excesssive hardness of the rock and a greater quantity of water than was found at Frejus or at Mount Hoosac, would not be able to exceed an average of three meters per day at each heading, or, at the most 550 meters per quarter. In the last three quarters this maximum is changed to the minimum. The last two have given $3^{m},60$ and $3^{m},80$ of average advancement daily for each heading.

During this period there has been some time lost for examination, by direction of the Company, and by a temporary strike at Göschenen.

Notwithstanding these excellent results the ill-will is not destroyed. They predict anew that the tunnel will not be finished in eight years—the total bulk extracted being insufficient, etc., etc.

We will remark, at first, that not *eight* years, but *nine* years are allowed by the agreement, *with enormous penalty, it is true;* but this limit is only the more honorable in sight of rational men; and certainly when a contractor gains for a company fifteen millions he has a right to all the regard and to the extreme good will of those who represent that company, and of all men of this trade, especially in view of a success which is already a great honor to the Swiss.

It is evident that progress in works without precedent, and where difficulties seem to accumulate, ought to be purchased by trials, study and successive improvement. The whole progress cannot be accomplished at once, nor in a single year. The work at Mt. Cenis, where the expense was less and where each kilometer paid double that of Gothard, it was shown that the engineers and contractors of Frejus continued to improve in their machines and in their organizations of labor. M. Favre considers it to be his duty first of all to make progress at the heading. In three years he succeeded beyond all expectation—thanks to his activity, energy, ingenious spirit and enormous sacrifices. Now they contend that the enlargement should progress equally fast. It is hoped that, when the improvements and projects in progress shall have had time to be completed, experience will prove that the fault found with the able contractor has been premature and unjust.

The elements of progress in drilling very long tunnels depend, essentially, upon the use of machines and abundance of motive power.

The force is transmitted by compressed air which operates the perforators and at the same time ventilates the tunnel throughout. To obtain this power, waterfalls, motors, and compression apparatus are necessary. We will speak first of sources of motive power, afterwards of air-compressers, and finally of engines which this air puts in motion.

II. WATERFALLS AND MOTIVE POWER.

On the south side of the tunnel can be utilized the Tessin or the Tremola; in the last stream, which descends from Lake Sella, above the hospital, the water flows smoothly over a declivity of 20 to 100, an advantage which should make it desirable. From previous gaugings it is calculated that its minimum volume of water would be three or four hundred litres per second. To make the best possible use of this small volume it was necessary to obtain a maximum of fall, consequently the height of the upper reservoir above the turbines has been carried to 180 meters or 18 atmospheres. This source presented, apparently, insurmountable difficulties; this stream, flows through a gorge where large avalanches fill its bed nearly every minute.

M. Favre conceived an ingenious idea of bringing the water from a higher point, where the Tremola is accessible all the year by means of a canal of 1000 meters;—he directs the water into the bed of a secondary stream, the Chiasso, more remote from the fall of avalanches.

The depositing reservoir which ought to retain the floating matter and gravel is at the side of the Chiasso under shelter of a rock.

Clear water from this reservoir, placed 180 meters higher than the hydraulic motors, descends by a conduit $0^m,62$ in diameter, and 841 meters long, formed of very strong iron tubes, to the motors and compressers near the workshop.

The four turbines were furnished by Escher, Wyss & Co. of Zurich.

These turbines have vertical axles—they are $1^m,20$ in diameter, 100 floats, and make about 350 revolutions per minute.

They are cast in a single piece with their floats in bronze; under excessive pressure bronze lasts longer than iron, cast iron or steel. The shock of water under eighteen atmospheres affects the last three metals at the end of a few months' service.

Each of these turbines drives by a single gear a horizontal shaft, and all these shafts are placed on the same line.

The performance of this apparatus has been most satisfactory; it fails only in times of excessive cold or great droughts. This volume has decreased, during short intervals, to less than 100 litres per second. M. Favre decided in 1874 to obtain from the Tessin additional power.

This stream at its head at Airolo seems to defy all attempts to utilise it; its declivity is small, and it flows between rugged banks composed entirely of crumbling rocks, and to increase the danger, avalanches of snow and rocks fall every winter.

He, however, undertook this perilous work and has succeeded. The supply canal discharges a cubic meter per second: it is in a great measure suspended to the sides of almost perpendicular rocks, through two aqueducts raised twenty-five or thirty meters above the Albinasca and Tremola rivers.

The project of raising the water of the Tessin to the reservoir of the Tremola by means of a canal seven kilometers long was impossible, consequently, he determined to make a canal of three kilometers and a second reservoir, placed only ninety meters above the turbine wheels.

There are, then, at Airolo two supply canals, and two depositing reservoirs situated at two heights, of which one is double that of the other.

The corresponding difference of velocity for the two falls, being in the ratio of two to three, it was necessary to have recourse to two kinds of turbines. These important additions were accomplished in a remarkably rational and happy manner, by placing, under each shaft of the turbine wheels, a second turbine of different dimensions calculated for the fall of the Tessin.

These four new turbines are of the Girard system; each has its supply of water and special flood-gate. They were constructed and put in place by Escher, Wyss & Co.

This addition made the whole very easy to regulate, and assured a regular action throughout the year.

The water of the Tremola being freer from *debris* and gravel is always preferable, but as it has not the required volume, the water of the Tessin acts as complementary motive power; or if the conduit of eighteen atmospheres had a break or was interrupted in any way, the conduit of the Tessin would be able to supply its place.

The second work of canalization, so eminently remarkable for its energetic and judicious execution, has fully attained its aim. Its essential results are to regulate and at the same time increase the moving force. It now affords to the workshops of Airolo a power equal or superior to a thousand horse.

On the side of Goschenen the valley is also exposed to earth and snow avalanches, but here they are less frequent and less powerful, are of short duration, and present no other serious difficulty than that of transforming for a day or two the snow, which obstructs the wire lattice and the conduits, into mud, and occasions a delay that nothing can prevent.

The Reuss, below Andermatt, rarely decreases to less than a cubic meter per second; its slope is about 10 to 100, and permits of the construction of a useful fall of 85 meters by placing a dam about 926 meters up the river from the mouth of the tunnel.

130 meters downwards from the dam is a depositing reservoir of 100 cubic meters divided into five compartments and three chambers, serving to discharge the sand and gravel and retain the floating matter.

The last chamber affords exit to water by sheet-iron pipes being $0^{m},85$ in diameter, and 801 meters long, which descend to the building of the four turbines and distributes to them a total volume of about twelve hundred liters per second.

These four turbines are of the Girard system and have horizontal axles; their diameter is $2^{m},40$; their normal velocity, 160 revolutions.

III. AIR COMPRESSERS.

The quarterly report, No. 5., of the Federal Council, published in 1873, contains details of the air compressers used at Mt. Cenis, and compares their execution with those of the air-pumps adopted for Gothard. It revealed that, in the year 1852 M. Colladon had proposed, in a detailed memoir remitted to the Government, the use of compressed air as a substitute for cables in transmitting force into the tunnel. This memoir contained :

1st. The results of numerous experiments that had been made in 1850, 1851 and 1852, upon the resistance of air and gas in tubes of different diameters, and upon the essential modifications resulting from these experiments, upon the coefficients of resistance depending on the diameters.

2d. The calculations for application at the Mt. Cenis tunnel.

3d. Some practical details of compressed-air pumps, of the transmission of force, of its storing, of compressed air used for throwing water, and of the means of conducting the power at the end of the tunnel for setting in motion the drilling machines.

In this memoir, M. Colladon proposed to utilize the waterfalls by means of turbines; he indicated the possibility of cooling the compression pump by an envelope of water, or by an interior injection, and speaks also of liquid piston pumps.

These projects were presented, in 1852, for the boring of Mt. Cenis, but it was found that they were only partially applied, principally in the employment of compressed air as a substitute for cables; they are found now to accomplish wholly the work at St. Gothard.

MM. Sommeiller, Grandis and Grattoni, entertaining the idea that trains could be made to run on inclined planes by air motors, had, in 1853, taken out a patent for an air compressing ram. The experiments had been abandoned, when, in 1857, the Government decided to undertake the tunneling of Mt. Cenis and trusted the execution to MM. Sommeiller, Grandis and Grattoni, whose ram appeared, at that time, most advantageous for the compression of air.

Consequently, twenty compresser rams were ordered to be distributed near the two mouths of the tunnel. Their total cost exceeded two million francs. The practical results were not favorable, as the machines remained without employment at Modena and were not used for the space of three years at Bardonneche. For them were substituted double cylinder pumps containing two and a half cubic meters of water which formed liquid pistons for the compression of air. These pumps were, at that time, considered a great improvement.

An official report of three engineers, published in 1863, states that they gave, with the same hydraulic force, three times as much air as the rams, and at a third less cost.

It is easy to see that pumps with an alternating motion, whose piston is to move a great quantity of liquid, are not susceptible of rapid oscillations. This has been confirmed by experiments with all compression apparatus constructed on this plan. Beyond a certain limit of velocity any increase of power is not attended by useful results.

At Bardonneche, as well as at Modena, they had to limit the number of revolution of the shafts to eight. To compensate for this the pumps employed must have excessive dimensions.

At Gothard, as in all mountainous countries, the best hydraulic motors are rapidly revolving turbines connected with high falls.

For the compression of air at Goschenen and at Airolo, it has been necessary to apply similar engines to those used at Mt. Cenis; between the turbines and the pumps they had to interpose numerous powerful brake wheels to properly reduce the velocity, to prevent loss of work, chances of accident, frequent changing of apparatus and above all great excess of expense.

The employment of turbines necessitated that of rapidly moving compression pumps; but it was necessary, at the same time, to prevent heating the air which would have involved a very notable loss of useful effect.

Prof. Colladon had taken out a patent in 1871, for a new system of air-compression pumps, which, by a very rapid action, permitted of compression of the air or gas, and at the same time annulled the prejudicial effects of heating. A pump of this kind was established in 1871 for the railroad in Upper Italy; this pump, designed for the compression of illuminating gas under high pressures for lighting trains at night, worked constantly nearly a year at the mean velocity of about 200 strokes per minute.

This result was obtained by a double combination, which, simultaneously, cooled the outside and the moving parts of the pump; the cooling was accomplished by the injection of a very small quantity of spray. The piston and its rod, prolonged beyond the cylinder, are hollow; their interior is constantly cooled by a small stream of fresh water brought by a tube to the hollow part of the rod. This water circulates in the cavity of the piston, and goes out through the space between the injection tube and the sides of the rod.

For pumps of great volume the cooling is completed by small injectors which mixes the air with the spray.

Pumps of this kind were put on trial in the workshops of the Geneva Society in presence of the contractor M. Favre; he made known the possibility of obtaining great volumes of air, under pressures of eight or nine atmospheres without injurious heating.

The turbines of Airolo, each of 200 horse power, made 350 revolutions per minute. The consulting engineer proposed to employ pumps making eighty revolutions in the same time and worked by tangential wheels with interposition of only one gear. To equalize the resistance and dispense with the use of flywheels, the engineer coupled these pumps in groups of three, placed parallel on the same base, and worked by a shaft with three cranks. This plan was adopted by the Company. MM. Escher, Wyss & Co. had charge of transmission, and the Geneva Society of Construction had the supply of five groups of these compressers each, for the Airolo side.

These five groups are placed with moving turbines in a chamber 35 meters long and $8^m,50$ wide. Each turbine can work either one of the groups, or all of them together.

Four of these groups working together can supply, hourly, to the tunnel nearly a thousand cubic meters of air at the tension of seven or eight atmospheres, which, before expanding in the tunnel can transmit to the working machinery a force of several hundred horse power. This volume, in expanding, furnishes for ventilating the tunnel eight thousand cubic meters of air.

The disposition adopted for the air compressers at Goschenen differs from that of Airolo only in some secondary details. The pumps are disposed in like manner; they also form five groups, each composed of three compressers. The crank shafts have a mean velocity of sixty revolutions per minute. This difference of velocity compared with that of the apparatus at Airolo, is compensated by an enlarged volume of the pumps.

The compressers of Goschenen were furnished by MM. Roy & Co; they are constructed after the Colladon system and differ only in some details from the compressers that the Geneva company have provided for Airolo. In each of the stations the compressed air is collected into sheet iron cylinders serving as reservoirs. Thence it is carried by a continuous tube of $0^m,20$ diameter to the extremity of the cunette. This air is immediately conducted to the *abattages*, and to the extremity of the gallery by means of heavy iron tubes 0^m14, and 0^m10 in diameter. The outlets of air for working the perforators were established by means of rubber tubes 0^m05 in diameter.

Besides these supply cocks, there are at several points of the principal conduit air cocks for ventilating purposes.

The Colladon pumps working for the last two years at Gothard show, in an unquestionable manner, the possibility of compressing large volumes of air without hydraulic pistons to the tension of eight atmospheres, or more, by rapidly moving pumps, and of obtaining this compressed air at an elevation of temperature not exceeding 12° to 15° C.

On the Bardonneche side of Mt. Cenis, for supplying compressed air, they established seven bucket wheels, each of which was coupled to four large water-column cylinders. To put up these wheels and their pumps, they had to construct seven distinct buildings, each having a surface of 300 sq. meters.

These seven wheels and their twenty-eight compresser cylinders can furnish, per hour, 570 cubic meters of air under pressure of six effective atmospheres. This air by its expansion gives for ventilation, about four thousand cubic meters at atmospheric pressure.

At Gothard, four turbines driving twelve pumps of small capacity with great velocity, produce 1,000 cubic meters per hour at a tension of seven effective atmospheres; and this air in its expansion in the depths of the tunnel furnishes 8,000 cubic meters at the pressure of the atmosphere.

These turbines and their compressers are, to a great extent, in a single building which has a surface of only 350 square meters.

After all, the Colladon pumps of great velocity driven by four turbines give at Gothard twice as much power in compressed air as the apparatus used at Mt. Cenis, at about one third expense and occupying one fifth the space.

Eighty of the same formerly employed at Mt. Cenis are equivalent to four turbines and twelve pumps like those at Gothard.

IV. VENTILATION OF THE TUNNEL.

The average number of men working on one side of the tunnel at the same time, is four hundred.

Each one is generally provided with a lamp, and each lamp requires a renewal of air equal to that necessary for the man. An average of thirteen cubic meters of fresh air per hour is required for one workman and his lamp, or five thousand two hundred cubic meters per hour for 400 workmen and their lights.

The mean quantity of dynamite consumed in twenty four hours, at each mouth of the tunnel, is estimated at 300 kilogrammes, an average of twelve and a half kilogrammes per hour. It is proper for good ventilation to give one hundred cubic meters of air with each explosion of a kilogramme of dynamite, corresponding to an average of 1,250 cubic meters per hour.

It is required, then, to introduce at each side of the tunnel, six thousand four hundred and fifty cubic meters of air at one atmosphere, per hour. We have seen that, at Airolo as at Goschenen, the turbines drive four groups, and send into the tunnel the equivalent of eight thousand cubic meters under the atmospheric pressure.

This volume would be more than sufficient, if the fresh air expelled the same volume of partially vitiated air. This effect is produced in a satisfactory manner in the gallery and at the *abattages;* but as the excavations increase an eddy is produced and the foul air remains behind in the cavities or under the arch, while the fresh air partly escapes outward.

In order to remedy this imperfect expulsion, the contractor has decided to place at each end of the tunnel a powerful system of suction, extended to the extremities of the arch by a continuous tube $1^{m},20$ in diameter suspended under the intrados.

This apparatus is composed of two connecting bells suspended at the extremities of a beam, and which receive alternate ascending and descending motions by the working of two water-column machines. Each bell is plunged into an annular tub full of water; the central part of this tub is closed by a diaphragm supplied with valves, and the bottom of each bell is also provided with clack valves opening outward. At each ascension the air is drawn through the entire length of the tube suspended from the arch, and when the bell descends the air is discharged into the atmosphere.

The two bells are able, by ten double oscillations of the beam, to draw 500 cubic meters per minute, or 30,000 per hour.

This air, drawn to the amount of many hundred meters from the bottom of the tunnel, should be replaced; it will be, in part, by the 8000 meters furnished by the pumps, while 22,000 cubic meters per hour ought to arrive from the outside through the large section of the tunnel already finished.

With this powerful ventilation, which will be increased by the new pumps, the interior of the Gothard tunnel will, certainly, be better aired than is the majority of mining works.

V. BORING MACHINES.

The work at Gothard has given rise to new boring machines and to important improvements in the construction of this useful apparatus.

The first boring machine intended for piercing hard rock, by employing compressed air, was constructed in 1855, by the English engineer, Th. Bartlett, representative of M. Brassey, builder of the Victor Emmanuel Railroad. This remarkable machine was tested in March 1857, at Coscia, in presence of a nominated commission in view of the Mt. Cenis tunnel.

M. Sommeiller assisted at these experiments, and the rapid action of this machine induced him to have it patented, and it was employed, to the exclusion of all others, at piercing the tunnel of the Cottiennes Alps.

Then at the International treaty for the Gothard railroad, the Italian Government made, as a condition of its support, the re-purchase, by the Swiss Government, or by the executive Company, of all the old material which had been used in piercing at Frejus. This re-purchase was one of the obligations imposed upon the builder when he signed his contract. M. Favre, it is seen, bought one hundred of the Sommeiller boring machines for his own use.

This method is no longer in use; improvements and new ideas have arisen and have effected many changes; there are, to-day, more than twenty different machines for boring blast holes in hard rocks.

All of these machines have essential analogous parts and are generally composed:

1st. Of a principal cylinder for percussion.

2d. A percussion piston from which the shaft is prolonged and serves as a head-stock, because they fix, at its extremity, the chisel, the graver or the foil used for piercing the holes in the rocks.

3d. A slide-valve, or governator-cock, whose alternate motion guides the compressed air, alternately, before or behind the piston.

4th. Gearing for turning the piston, its shaft, head stock and piercing chisel, and to make the cylinders and these attachments advance against the working place during the progress of the drill.

5th. A support or frame, usually parallel tracks, on which the cylinder and these attachments are able to slide as the work advances.

This frame or support, intended to be placed on a carriage, should be able to incline in different ways according to the direction of the holes which are to be drilled.

The drill ought to have a rapid and powerful alternate motion; it ought, also, to turn on its axis to disengage its edge, during the perforations, and make a straight and regular hole. The piston and the shaft head-stock ought, evidently, to participate in the same motions. Finally, the cylinder and the principal attachments ought to advance, whether by hand or automatically, toward the work.

The miner, with a hand-drill realizes in an admirable manner these three simple indispensable motions; but the muscular force of a man is insufficient when the drilling is to be rapid. It is necessary then to have recourse to the use of machines, and to those of compressed air, especially when we wish to work in the depths of a tunnel.

Side by side with the accomplishment of the three above named motions there are other elements of comparison which guide the contractor in his choice of boring machines, such as: the proportion of compressed air for certain effects produced; the good execution of the apparatus; the choice of metals employed in its construction; the first cost; the cost of keeping it in repair; the more or less easy management by the workmen; the weight of the machine; the length and breadth and the depth of hole that it is able to make in one operation without changing the drill.

The Company of Gothard made trial of several models of boring machines, both at Geneva and at the tunnel. After these trials they were limited to the use of three or four models, each of which had its special advantages. The variety of these systems was no detriment to the rapidity of execution of the work, for the company required of the constructors, that each perforator should be adapted at once to the principal carriage on which they were to place a number to work together; they required also that the mode of operation be so easy and simple that miners be able to work after a very short apprenticeship.

The experiments show that this mode of manufacture is preferable to that used at Mt. Cenis.

Immediately after signing the contract, the contractor decided to purchase, in Belgium, two steam compressers and a provisional water-column, which were placed at the northern and southern extremities of the tunnel. He treated, at the same time, with the manufacturers, Dubois and Francois, for the delivery of a limited number of their boring machines.

These have points of resemblance with those of Mt. Cenis, but are different in several essential particulars.

Sommeiller's machine is composed, in imitation of that of Bartlett, of two distinct parts; one very small compressed-air motor with a fly-wheel, and a perforator properly so-called. It was by means of this small motor that Sommeiller was able to move the distributing valve and obtain the rotation of the piercing piston and the progression of the cylinder.

MM. Dubois and Francois' apparatus is more simple than Sommeiller's and uses less compressed air for the same effect. These manufacturers have supplanted the little auxiliary motor. The slide valve receives its alternate motion by the action of the compressed air upon two, small, unequal pistons, and by the cross-head which at each return opens a plug and determines the advancement of the valve.

The rotation of the piston and of the perforating tool is obtained by the alternate action upon a bent lever of two small pistons placed at the sides of the cylinder, and rising in turn by the impulsion of compressed air which rests upon the two faces of the large perforating piston.

The oscillatory motion of the lever causes the rotation of the head-stock, by means of a ratchet-wheel which governs the head-stock and of a catch whose motion is similar to that of the lever.

The regular advancement of the system, according as the aperture increases, is governed by a large screw parallel to the cylinder which is moved by hand. Soon after putting these machines in use, another new system obtained great success in England. After some trials made in Switzerland, M. Favre decided to order a certain number of these machines of the American inventor, Mr. MacKean.

This Anglo-American system of boring machines was entirely different from those we have tried to describe. The revolution of the piston, of its shaft and of the piercing instrument are obtained by the oscillation of the piston, by means of two screw-shaped cog-wheels.

One of these wheels is fixed on the piston rod, the other is fixed on a small, special shaft. This second shaft carries besides a ratchet-wheel.

One wheel participates in the oscillations of the piston; the pressure of this wheel against that of the second wheel tends to give to the latter and to its shaft an oscillatory rotary motion. But the ratchet-wheel and its catch will not permit the revolution of this second wheel, excepting in one direction. The result at each return of the piston is a partial revolution of the perforating piston and of the chisel.

The slide valve of the McKean machine is cylindrical, and the mechanism which moves it is more simple than in the Sommeiller and Dubois apparatus.

Mr. McKean has, for the advance of the cylinder and these accessories, employed a screw parallel to the motor cylinder. He has, also, utilized the alternate, rotary motion of the shaft of the slide valve for obtaining an automatic advance by means of a screw to which is adapted a ratchet-wheel.

The velocity of perforation obtained by this machine notably surpasses the machines previously described.

In the experiments made in Switzerland they obtained, with a pressure of from four to five atmospheres, a normal advance of $0^{m},10$ to $0^{m},12$ per minute in a very hard block of granite.

The whole apparatus is shorter and occupies less space than the machines of Sommeiller, or of Dubois and Francois. Its weight is also less which renders it easier of transportation and of putting into place. These machines fixed upon special small carriages have rendered useful service in the work of enlarging the tunnel.

The first McKean apparatus was illy adapted to the large carriages used at Gothard in the gallery of direction, at the *abattage* and at the cunette. In the year 1875 the inventor surmounted these difficulties, and the contractor of the tunnel decided to make a new order of sixty of these machines to work on the Airolo side.

A third system gives, also, good results at Gothard; it was invented in 1874 by M. Ferrono, the former head of the workshop at Modena.

M. Ferrono employs a separate little machine; abandoning Sommeiller's complicated mechanism which works the spider valve, he has replaced it by an eccentric to which the little motor transmits a direct rotary motion. The mechanism for the rotation of the drill on its axis are nearly the same as in the Sommeiller apparatus.

These machines employed for eighteen months at the heading at Goschenen are preferable to those of Dubois and Francois on account of the ease in working and the rapidity of advancement; they have, however, two objections: the length and weight increases by the addition of a second cylinder, and the outlay of air is considerably more than in the other machines, on account of employing a secondary motor.

M. Turrettini, the intelligent director of the workshops of the Geneva Society of Construction, has invented an entirely new boring machine.

This patent had its piston composed of two parts which separated a little before the shock of the chisel and gave more elasticity to the blow. This same shock of the drill determines the change of the distribution and the return of the head-stock; they thus avoid the serious difficulty which often presents itself in the generality of boring machines.

The revolution of the piston, also the motion of the valve, are obtained by combinations of which experience has shown the efficacy as well as the moderate expense of keeping it in repair.

Finally, the progressive, automatic advance of the perforating cylinder along the main support, and if necessary, its recoil, are obtained by an entirely new process. It is by the principle of utilizing the reaction of compressed air, that the engineer can obtain either of these effects by a single motion of the tap. A lever acted upon by this compressed air gives to the apparatus, in every position, the needed stability for resisting the shock.

The works for automatic advance of the perforating cylinder upon the main support, have been the main difficulty in the greater part of the apparatus invented since the boring of Mount Cenis.

Some give an advance disproportioned to the progress of the drill; others employ the delicate parts liable to injury.

The automatic motion, invented by M. Turrettini, leaves but little to desire; it exactly suits the progress of the chisel, and works without shock, and is remarkably simple.

If the apparatus, supplied with its graver, is moved upon these main supports to any distance from the face of the works, at the moment when the compressed air-cock is opened, the perforating cylinder rapidly advances until the chisel reaches the rock, and from that moment continues to advance equally with the drill.

This small machine is shorter and weighs less than those of Dubois and Francois, or Ferrono.

It also consumes less air for the same work. It is, doubtless, destined to win success in the future, since it was able on first trial to contend with the best models.

These new boring machines, put in motion at Gothard, in the summer of 1875, worked concurrently with the three systems previously described, and the good results of these trials decided the contractor to order, of the Geneva Society of Construction, thirty-two other boring machines of the Turrettini system to be delivered without delay. The holes have generally a depth of $1^{m},10$. The number of those that they have drilled at the face of the works in the gallery of advancement, where the surface equals six or six and a-half square meters, varies with the nature and hardness of the stone; it is generally comprised between sixteen and twenty-six.

When the holes are drilled the carriages are moved to a distance of sixty or eighty meters, the holes are charged with dynamite, and two or three successive explosions are made. The rubbish is removed by hand or in baskets and put into small wagons and conveyed beyond the *cunette*. Then, by means of a chute they empty the small wagons into larger ones, which are stationed at the bottom of the *cunette;* a compressed air locomotive then takes ten or twelve wagons full to a place for rubbish outside of the tunnel.

VI. COMPRESSED AIR LOCOMOTIVES.

Two compressed air locomotives at each end of the tunnel are occupied in transporting. The old one is formed of an ordinary locomotive of twelve horse power, supplied with compressed air by a cylindrical reservoir of sixteen cubic meters in volume, borne on two trucks drawn by the locomotive; this reservoir is supplied by an air-holder on the conduit principle of compressed air. The other locomotive was made at the Creusot; it has no tender and is composed of a reservoir of seven cubic meters able to resist 14 atmospheres. To this reservoir are fixed two motor cylinders which work at a mean pressure of five atmospheres. The distribution of compressed air is regulated by an automatic apparatus invented by M. Ribourt, engineer employed at Gothard, and formerly pupil at the *Ecole Centrale.* This work perfectly fulfills its purpose.

To obtain a regular supply of compressed air at 14 atmospheres, M. Favre instituted special reservoirs, and in 1875 ordered, of the Geneva Society of Construction, eight compressors of the Colladon system, each being able, withont sensible warming, to compress twelve cubic meters of atmospheric air per minute at the pressure of 14 atmospheres. Four of these machines work at Airolo and four at Göschenen. They are placed in the chamber where they are found uniting the four hydraulic motors and the five groups of compressers, and they are worked by the motor shafts of these four turbines.

CONCLUSION.

Before closing this abridged description of the numerous and powerful machines employed at Gothard, and of the work in progress, I should commend the intelligence, activity and energy of the eminent contractor as well as the zeal of his assistants.

When they reported at the end of 1872, that everything was against them, that there were difficulties that it was impossible to overcome or prevent, that duties accumulated and there was a combination of things to accomplish, we cannot but admire the present condition realized in less than three years.

The local and physical difficulties at Gothard have been exceptionally serious; utilizing the torrents, for example. The nearly perpendicular cliffs of rock, the exposure to falling stones and frequent avalanches render canalization on the south side nearly impossible.

To the difficulties of climate, locality, and the great mass of snow, are added, in the Airolo tunnel, incidents of greater importance: as those occasioned by the variable nature of the rock, the numerous breaks from which the sand and gravel flood the gallery, and especially by infiltration, of which the volume and the violence are extraordinary.

The streams escaping from the top and side of the south part of the tunnel, which has a descent of only one in a thousand, have transformed the lower part of the gallery and the *cunette* into a river at the bottom of which it is necessary to maintain a road and to work at the lower borings.

Two or three examples will show the magnitude of this obstacle.

At Mount Cenis the maximum of infiltrations at either mouth has not surpassed *one* liter per second. At Mount Hoosac, according to official reports, they considered eighteen liters per second a very great obstacle which notably interfered with the work.

In the first public report from the *Direction di l'Administration*, in speaking of infiltration in the south of the tunnel, which at that time had reached from fifteen to thirty liters per second, the honorable reporter called this flow: "A little torrent and a delivery of water of extraordinary proportions."

This little torrent became, some months later, a river gauging from two hundred to two hundred and thirty liters per second, eight hundred thousand liters per hour, in a gallery of less than seven square meters.

These physical obstacles are not all that have troubled the contractor and delayed the work. There have been others less known to the public, and quite foreign to the duties and the occupation of M. Favre.

He would have been ready to commence drilling the last of August, 1872.

The chief engineer of the company had illy prepared for the difficulties at the approaches. On the side of Airolo they were not ready until the last of September, and at Goschenen they were not entirely ready at the end of December. M. Favre was occupied with other things, but was constrained by the exigencies of the chief engineer to take the matter of completion in hand. To this task were added many others—he was delayed by obstacles still more foreign to his work. Diplomatic difficulties arose in November to occasion the delivery of material at Mt. Cenis, of which the purchase and expense was imposed upon M. Favre. For nearly two months he was kept in suspense without learning what was required or the time for delivery. This resulted in the loss of the autumn months, which were most favorable, and the last half of the winter was visited with more snow than had been known for half a century; thus the work of the contractor and the greater part of his transportations were delayed until May, 1873.

These united circumstances, aggravated by conditions already too severe, gave the contractor a right to claim special indulgence on the part of all interested.

The Italian Government was doubly honorable in dealing with the work at Frejus, to the undertaking itself and in exceptional ways towards those directing the execution—the engineers or contractors.

The Egyptian Government and the subscribers to the Suez Canal imitated this noble example toward M. de Lesseps.

It will be the same with the work at Gothard. The greatness of the task and the efforts exerted already will recommend the enterprise to the kindness of the public and especially to that of Government.

PHOSPHOR-BRONZE AND ITS APPLICATIONS.

By ALEXANDER DICK.

From "Journal of the Society of Arts."

According to antiquarian research, bronze formed the earliest alloy produced—some specimens are estimated to have an existence of from 4,000 to 5,000 years; it was not the alloy which we now call bronze, formed of copper and tin, but the result of the rough smelting of cupreous ores, and, of course, contained many impurities. It was used for casting domestic tools, weapons, and images; at a later period for bells, and still later for cannon.

The difficulty of the bronze founder, during the later periods referred to, appear to consist in the impossibility of obtaining pure copper and tin for his smelting, but even when these became articles of commerce, further difficulties were met with in producing sound and reliable castings, and it is of but a recent date that some light has been thrown on the nature and cause of these difficulties, as also on the way to avoid or remedy them.

In the year 1868, Messrs. Montefiore and Künzel, of Liége, in Belgium, engaged in a series of very exhaustive experiments with bronze and bronze castings.

These gentlemen observed that the tin in bronze continually decreases by oxidation during the process of smelting. They found that the oxide of tin partly goes into the slag and partly is dissolved in the molten metal.

	Tin.	Copper.
Bronze originally composed of..	10.10	+89.90
Contained after the 1st smelting.	9.82	+90.18
" " 2nd " .	9.40	+90.60
" " 3rd " .	9.16	+90.84
" " 4th " .	8.52	+91.48

The operators found great difficulty in determining analytically the proportions of the oxides, whether in combination with the tin or copper, or whether only mixed up in the metal; they found that "poling," a process by which the molten metal is stirred up with a wooden stick, will eliminate the oxide combined with copper, but has no influence on the oxide of tin; experiments, by passing hydrogen over heated bronze filings, proved equally ineffectual; the oxide combined with the copper was converted into water, and weighed as such, but the oxide of tin remained as in the "poling" process. It was in consequence of this difficulty that Dr. Künzel, for the first time, introduced phosphorus into the molten bronze; this had the desired effect, and the metal, which at first had a dull and covered appearance, suddenly became bright and of metallic surface. Thus, by introducing a small proportion of phosphorus, or phosphuret of tin or copper, of previously ascertained composition, it was found that the total amount of oxygen contained in the molten metal could be ascertained analytically with perfect certainty.

The absolute and elastic resistance of old bronze, that is, bronze containing oxides, is much smaller than that of bronze made with new metal; and, as will be seen by the following experiment, "poling" will improve old bronze, whilst phosphorus greatly improves bronze "poled."

Shavings of old bronze was melted and a bar thereof cast at 1595° C., the remaining liquid bronze was stirred with a wooden stick ("poled"), and a second bar cast at 1668° C. The then remaining metal was deoxidised with phosphorus and a bar cast at 1614° C. The three castings were thus made out of the same crucible and cast in the same manner in three moulds. The results were:—

	Resistance. absolute lbs. per sq. in.	elastic lbs. per sq. in.	Lengthening until rupture. Per cent.
Old bronze......	22,982 ..	17,020 ..	2.0
" poled...	24,922 .	17,709 ..	2.8
" deoxidised with phosphorus	33,916 ..	19,300 ..	6.8

Thus by the entire reduction (elimination of oxide), the old bronze had tripled its tenacity and considerably augmented its absolute resistance.

Having so far succeeded in eliminating the oxides from the bronze, the inventors showed that by the further addition of a small per centage of phosporus to the bronze alloy the qualities of the latter became more and more changed, the grain or fracture became finer, the color brighter, the elasticity and resistance to strain and compression considerably increased, and when melted it attained greater fluidity.

Similarly, as minute quantities of carbon alter the physical properties of iron and convert the latter into steel, so a minute quantity of phosphorus changes those of bronze and converts the same into phosphor-bronze. It may thus be said in a certain sense that what steel is to iron phosphor-bronze is to ordinary bronze.

Messrs. Montefiore and Kunzel next experimented with alloys of copper and nickel, and with manganese (binary alloys), also with (ternary) bronzes of copper, tin, and manganese, with copper, tin, and nickel, as well as with iron alloyed with copper and tin.

The manganese alloys they concluded to be entirely useless, as also those of nickel and of iron. They obtained great tensile strength and hardness with some of these compositions, but their ready oxidation at high temperatures made the qualities of the castings uncertain and impracticable.

Sodium eliminated the oxides contained in the molten bronze, but the slightest surplus of it produced an alloy which could not resist the atmospheric influences, and oxidised with great rapidity.

A minute addition of zinc to ordinary bronze augments the resistance to rupture by reducing involved oxides, but it softens the alloy and causes it to lose its elasticity.

Phosphorus was therefore found to be the only ingredient which will improve bronze by giving reliable results.

The action of phosphorus is twofold—firstly, it eliminates the oxides as stated above, and secondly, it makes the tin capable of adopting a crystalline structure—and, as two crystalline metals form a much more homogeneous alloy than two metals, of which one is not crystalline, phosphor-bronze must necessarily be more homogeneous than ordinary bronze. Homogeneity and absence of oxygen increase the elasticity and absolute resistance of the alloy.

Another great advantage of phosphor-bronze is that its hardness can be regulated by varying the proportions of phosphorus, which, in ordinary bronze, is done by increasing the proportion of tin, whereby the danger of segregation in the casting is greatly augmented. Ordinary bronze, after one or two smeltings, becomes thick-flowing, and putty-like, whilst phosphor-bronze remains perfectly liquid until the moment it sets (solidifies), if, therefore, it is cast just before the "setting" takes place, no segregation is possible.

Combinations of phosphorus with copper, with tin, or with other metals, have long been known by chemists, but Dr. Kunzel was the first to employ the same for the purposes above stated.

A number of phosphor-bronze alloys are now manufactured, varying in composition to suit the objects for which they are intended. The scope of their applications is of course very great. The harder alloys are used for casting bells, tools for gunpowder mills, &c.; other somewhat softer alloys are used for engineering purposes, and the still softer for rolling, drawing, and embossing, &c.

The following tables will show the results of tests made by Mr. Kirkaldy, of Southwark Street, with various phosphor-bronze alloys :—

CAST METAL.	Diminution of Section before Rupture.	Resistance in pounds per square inch.	
		Elastic.	Absolute.
	Per Cent.	Pounds.	Pounds.
Pure Copper	3.30	4.4000	6.975
Ordinary Gun Metal, containing 9 parts copper, 1 part tin	3.60	12.800	16.650
Phosphor-Bronze	8.40	23.800	52.625
" "	1.50	24.700	46.100
" "	33.40	16.100	44.448

	Drawn Metal.	Pulling Stress per square inch.		Twist in Five Inches.		Ultimate Extension.
		Wire as drawn.	Annealed.	Wire as drawn.	Annealed.	
		lbs.	lbs.			Per Cent.
Various Alloys.	Phosphor-Bronze...........	102.759	49.354	6.7	89	37.5
	" "	120.957	47.787	22.3	52	34.1
	" "	120.950	53.381	13.0	124	42.4
	" "	139.141	54.111	17.3	53	44.9
	" "	159.515	58.853	13.3	66	46.6
	" "	151.119	64.569	15.8	60	42.8
	Copper........................	63.122	37.002	86.7	96	34.1
	Steel..........................	120.976	74.637	42.4	79	10.9
	Iron, galvanised best charcoal E.	65.834	46.160	48.0	87	28.0

A series of interesting experiments with phosphor-bronze was made in Berlin by the Royal Academy of Industry, in order to ascertain the qualities and capacities of the metal whilst under heavy strain, and its resistance to often repeated strains. The first bar of phosphor-bronze was tried under a constant strain of 10 tons per square inch, and resisted 408,230 pulls ; a bar of ordinary bronze broke even before the strain of 10 tons per square inch had been attained. A second bar of phosphor-bronze was tried under a strain of 12½ tons per square inch, and withstood 147,850 pulls, and a third bar, under 7½ tons strain, broke only after 3,100,000 pulls. On the bending machine, phosphor-bronze, whilst under 9 tons strain per square inch, remained unbroken after 4,000,000 bends, whilst ordinary bronze broke after 150,000 bends.

Major Majendie tested phosphor-bronze as to its liability to emit sparks, when subject to friction, and attained very satisfactory results. The experiments were carried out at the royal gunpowder works at Waltham Abbey. A grindstone of 9 inches diameter was made to revolve very rapidly, so that any point on the grinding face would describe a distance of 2,000 feet per minute ; the metal was then pressed against the revolving stone, and the results proved that the harder descriptions of phosphor-bronze emit sparks less readily than the softer samples, and much less readily than ordinary gun-metal or copper.

For frictional purposes the Phosphor-Bronze Company, in London, produces a special alloy by fusing phosphor-bronze in certain proportions, together with another soft alloy of different degree of fusibility, so as to produce by cooling two given alloys. The shell is then formed of a very tough and hard phosphor-bronze and the interior of aforesaid soft alloy. The bearing surface may then be considered to consist of a large number of small bearings of soft metal, enclosed in casings of metal almost as hard as the arbor itself. The microscope reveals this disposition very plainly, and if one of these bearings be carefully submitted to heat, so as to cause the soft fusible metal to run off, the rest will remain in the form of a spongy mass. Bearings, slides, eccentrics, &c., of this peculiar alloy are now very largely in use, and the practical results show that it wears more than five times as long as gun-metal.

Phosphor-bronze is readily rolled or beaten out into sheets. In Russia it has been used as a material for cartridge sheathing, and specimens have stood 120 trials without tearing. Sheets of the alloy stand the action of sea-water much better than copper. In a comparative experiment made at Blankenbergh, lasting over a period of six months, between the best English copper and phosphor-bronze, the following results were arrived at :— (*See Table on following page*).

The loss in weight, therefore, due to the oxidising action of sea-water during the six months' trial, averaged for the English copper 3.058 per cent., while that of the phosphor-bronze was but 1.158 per cent., or about one-third.

Thickness of the Sheets = 0.236 inches.	Weight before Immersion.	Weight after Immersion.	Loss of Weight.	
	lbs.	lbs.	lbs.	Per Cent.
Sheet of Copper................	74.4	72.2	2.2	3.015
“ “	88.9	86.2	2.7	3.100
Sheet of Phosphor-Bronze......	69.5	68.75	0.75	1.123
“ “ “	114.3	112.97	1.03	1.195

Several governments have experimented on the use of the alloy for making cannon. Without any exception, the results showed a much greater resisting power over that possessed by ordinary bronze. The following instances of the results arrived at will be of general interest :—

In Belgium the ordinary bronze gun burst at the second shot, with a charge of 1 k. 250 gr. (2¾ lb.) of powder, and a cylindrical projectile weighing 8 k. 518 gr. (18¾ lb.) The phosphor-bronze gun supported this charge perfectly ; the normal charge was 500 gr. ($1\frac{1}{10}$ lb.) of powder, and 3 k. (6¾ lb.) of projectile.

In France the ordinary bronze gun burst at the second shot, with a charge of 1 k. 500 gr. (3½ lb.) of powder, and 16 k. (35¼ lb.) of projectile, while the phosphor-bronze gun was fired five times with this charge, and burst at the second shot with 1 k. 750 gr. (3⅞ lb.) of powder, and a projectile of 20 kil. (44 lb.), owing to the wedging of this in the barrel. The normal charge was 0 kil. 550 gr. (1⅜ lb.) powder and a bomb of 4 kil. (8⅘ lb.)

In Prussia it was shown in firing with the regulation charges, and diminishing, at each 50 shots, the exterior diameter of the chamber, that the phosphor-bronze cannon only changed their dimensions when the thickness of the metal was below that of the dimensions of a cannon of the same calibre made of steel.

The Phosphor-Bronze Company now supply most of the English Railways and nearly all the rolling mills, the Royal Arsenal, and many of the leading engineering firms. Amongst the steamship companies the Peninsular and Oriental line has nine steamers fitted with phosphor-bronze in parts of the machinery, the Temperley line, the Wilson line, State line, Messrs. McAndrew & Co., the Ducal line, Globe line, the North Cape Mail Co., Messrs. Pickernells, Messrs. MacIver & Sons, and numerous other firms, as also 25 ships in H. M. Navy are fitted with phosphor-bronze.

In conclusion I beg to draw attention to the specimens of phosphor-bronze exhibited here—they are separated into different groups, each group is headed by an ingot of the phosphor-bronze alloy used for the manufacture of the various articles belonging to that group.

In each group is shown an ingot of the phosphor-bronze alloy used in the manufacture of the respective articles contained in the group ; also test bars from Mr. David Kirkaldy, of Southwark. These tests were made with bars cast in sand, except those of Group D, which was cast in an iron mould and then turned down so that the area of the section corresponded to one square inch exactly. The resistance of each bar is marked upon it, and it is interesting to notice the fractures, elongations, &c., of these bars.

Group A.—Contains the soft and malleable alloys. They can be rolled or drawn, and, like iron or steel, thereby greatly increase their strength and elasticity. These alloys have the appearance of copper which, in fact, is their chief constituent. The tests made with phosphor-bronze wire show that if drawn hard, the tensile strength can surpass that of steel wire, while when annealed, it is much tougher than annealed copper wire, its elongation before rupture takes place being 46.6 per cent. against 34.1 per cent. of copper wire. Amongst the specimens here before us is a piece of phosphor-bronze wire drawn so fine that it takes 2,000 yards for the weight of one ounce. The wire is used for wire webs in paper mills, for pit and other ropes where acid waters occur. For telegraphic purposes a phosphor-bronze wire is made which possesses the same tensile strength as iron wire, and three times its conductivity. Copper wire, which has the greatest power of conducting electricity, is too soft to be suspended without the support of steel wire; it "bags" between the poles, as it cannot bear its own weight. Phosphor-bronze tubes of various sizes are for use in boilers, &c., and the finer sort for Bourdon's gauges. The sheathing can be made like the wire, either very tough and malleable or hard and springy.

Group B.—This alloy is of about the same hardness as ordinary bronze, but much superior to it in strength, elasticity, color, and fluidity when melted. It possesses a rich, gold color when polished, and the castings have the strength of wrought iron. It can be hammered, but will roll only with difficulty. It is used for steam fittings and harnesses. The Belgian cavalry have phosphor-bronze buckles, and the Garde Civique in Belgium phosphor-bronze gun-actions in use for their Comblain rifles.. Messrs. Chubb & Son, and other lockmakers, use it for keys. In engineering works it is used for a great variety of things. On account of its great fluidity in the melted state, this alloy is very suitable for ornamental castings; these leave the moulds so true that but little chasing is required to finish them.

Group C.—The hardness and grain of this alloy approaches that of cast steel; although it will stand a blow with the hammer, it will not improve its elastic and tensile strength by hammering. It is chiefly used for parts of machinery, for pumps, valves, pinions, piston rings, &c.; also for the three-cylinder engine of Messrs. Brotherhood for Whitehead's fish torpedoes. A plunger cast in this alloy remained without any mark of wear after 572 days, where a steel plunger wore out within six weeks. As worm wheels it lasted eighteen months against the same cast in gun-metal lasting but a few weeks. An eccentric strap of this phosphor-bronze alloy wore but $\frac{1}{4}$ inch in eighteen months, while a similar one in gun-metal only lasted three months. In her Majesty's Navy it is used for slide faces, and the Royal gunpowder-mills at Waltham-abbey, as also many private firms use it for tools.

Group D.—The alloy forming this group is less hard than that of group C.; its grain is very fine, but its main feature is its great tensile strength. The tests show over 26 tons of absolute resistance per square inch. It must be cast in metal moulds, and its chief use is for bolts and nuts, for spindles, pump rods, &c.

Group E.—Bearing metal. The nature of this peculiar alloy has been mentioned already; its applications for rubbing surfaces is of course very manifold, and its use undoubtedly the most extended of all the phosphorized alloys. It is much appreciated for bearings in rolling mills by railways for slides and axle-boxes, and for carriage and loco bearings, for eccentrics, &c., it has given the greatest satisfaction.

Considering the comparatively short time since phosphor-bronze was first known in England, its use is very large—its superior qualities gave it a ready introduction to engineers, and it now proves itself quite an article of necessity with which many branches of industry can dispense no more.

NOTE ON "RECENT EXPERIMENTS ON FLOW OF WATER IN RIVERS AND CANALS."

By Brevet Major-General HENRY L. ABBOT.

Written for Van Nostrand's Magazine.

My attention has been called to a translation, in the June number of your Eclectic Engineering Magazine of an article upon "The Flow of Water in Rivers and Canals," which appeared in *Der Civilingenieur*, from K. R. Bornemann, Kunstmeister at Freiburg, Saxony. In it appears the following sentence, (p. 536):

"Gordon's experiments, however, inspire a far greater confidence than those of Humphreys and Abbot, for Hagen has shown that it is extremely probable that their results were altered in part to establish the theory proposed. His velocity measurements are also free from the suspicion attaching to theirs, for his lower float was connected with the upper by a very small cord, while in the Mississippi Survey the surface of the cord for measurements at a depth of 100 feet was $1\frac{1}{2}$ times as great as that of the float itself."

In intimating that in his opinion our results were altered to establish a theory, Mr. Bornemann has exceeded the limit of professional courtesy. Our observations were published in great detail; and as the figures "establish the theory proposed," this language implies a dishonest tampering with the field notes—a suggestion which, whether made by Mr. Bornemann or Mr. Hagen, or by any other person is absolutely false and libelous.

As you have given currency to this statement in your Magazine, I have no doubt you will do the justice to insert this reply in your next issue.

I will add that the remark about the size of the cord used in the Mississippi observations, contained in Mr. Bornemann's article is based upon an error which occurred in the First Edition of the Mississippi Report, and which was officially corrected by Prof. Forshey who made most of the sub-surface velocity observations in question. His letter upon the subject will be found on p. 372 Part II of the report of the Chief of Engineers for 1875.

THE DECENNIAL PERIOD OF MAGNETIC VARIATIONS, AND OF SUN-SPOT FREQUENCY.

By JOHN ALLAN BROUN.

From "Nature."

A CENTURY and a half ago Graham discovered that the north end of a magnetic needle moved from morning till afternoon towards the west, returning thereafter to its most easterly position in the morning again. Van Swinden, who, half a century later, studied this phenomenon during several years, occupied himself greatly with the deviations from the diurnal law. One of these, the occurrence of the greatest westerly position before noon or after 4 P.M., he found to happen most frequently in 1776, the number of times increasing from 1772, and diminishing from the year of maximum till 1780. He then asked the question whether there was not a period of *eight years.* Van Swinden's results were greatly affected by imperfections of his instrument, and we can only consider that the excess of irregular days in 1776 was probably chiefly due to real causes.

Though several series of magnetic observations were made during the eighteeenth century, and two series early in this (those of Beaufoy and Arago), yet, as far as I can discover, Kaemtz seems (in 1836) to have been the first to remark that the mean value of the diurnal oscillation of the magnetic needle was not constant, but varied from year to year: this conclusion he founded on Cassini's observations, which gave the mean oscillation 9′.71 in 1784, and 15′.10 in 1787. The illustrious Gauss drew more distinct attention to the fact, for, in studying the observations made at Gottingen in the years 1834 to 1837, he pointed out that the mean diurnal oscillation for each month in the second year was greater than that for the corresponding month of the first year; and that a similar increase was to be found in the third year compared with the second. This increase Gauss did not think could go on long, and he predicted that by continuing the observations for several years, an oscillation in the mean value would present itself. It is not a little curious that in discussing the Gottingen observations for the next three years, Dr. Goldschmidt should have failed to remark that the maximum was attained in 1837, and that thereafter the mean diurnal oscillation was diminishing. This was reserved for Dr. Lamont, the distinguished astronomer of Munich, who, in the end of 1845, by adding the mean oscillations obtained from his own observations in 1842-1845, to those already found for the preceding years at Gottingen, was able to state that the minimum was then attained, but that a longer series of observations was required, in order to determine the law of the oscillation.

It was only in the end of 1851, when the maximum oscillation (which occurred in 1848-49) was decidedly past, and the mean oscillation had again begun to diminish in value, that Dr. Lamont published his conclusion that the diurnal oscillation of magnetic declination (as well as of magnetic force) obeyed a law whose mean duration was nearly 10⅓ years. For the determination of this mean he employed the epoch of maximum oscillations shown by Cassini's observations in 1787 (already noticed by Kaemtz), and he assumed that there were six periods from that date till 1849.

Schwabe had previously, from his persevering observations of the number of spots on the sun's surface, arrived at the conclusion that these obeyed a decennial law, so that the number was a maximum in 1828, 1837, and 1848, while it was a minimum in 1833 and 1843. The agreement of the epochs, 1843 and 1848, with those of minimum and maximum magnetic disturbance deduced by Sir E. Sabine from the observations made in the colonial observatories, was at once remarked by him, as well as that of Lamont's epochs with those of Schwabe.

This coincidence was also immediately afterwards, and quite independently, brought to public notice by Dr. Wolf, of Bern (now of Zurich), and M. Gautier, of Geneva. It is, however, with the important labors of the former of these philosophers that we are most concerned. Dr. Wolf began at once a systematic search for observations of sun-spots, and examined hundreds of volumes printed and in manuscript, dating from the first discovery of the existence of spots on the sun's surface. All the observations thus accumulated he has endeavoured to connect and to reduce to a common unit; and from the numbers thus obtained he has concluded that the sun-spot period, as well as that of the magnetic variations, occupies on the average 11⅑ years.

One great cause of the difference between the results of the Munich and Zurich astronomers is to be found in the interval 1787 to 1818. According to the former, three periods *ought* to have occurred in this interval; according to the latter, only one maximum happened, *in fact*, between the two of 1787 and 1818. Dr. Wolf has concluded, from the magnetic observations of Gilpin (1786-1806), that a minimum of the diurnal oscillation of the magnetic needle occurred in 1796, and a maximum in 1803, and these epochs he has supported by the observations of the numbers of sun-spots, as well as of those of the aurora borealis, a phenomenon known to be associated with magnetic disturbance, and to have the same epochs of frequency. On the other hand, Dr. Lamont has maintained that Gilpin's observations are without value, as his needle was supported on a steel pivot, and sometimes did not move freely; he has also objected to the observations of sun-spot frequency made during the time in question, that they were made rarely, without any common system, and by few observers, some having at times seen no spots when others saw many.

If we could assume with the astronomer of Munich that Gilpin's observations and those of sun-spot and auroral frequency made at the same time are worthless, all our knowledge of the epochs of magnetic oscillations since 1818, and of sun-spot frequency since 1826, would induce us to conclude that there were really three periods during the thirty-one years 1787—1818. If, however, any value can be given to the observations during that interval, it is not allowable to assume that the durations of the periods have always been the same, the more especially that we know the period has varied in length from eight to twelve years within the last half century. That some value is due to observations of three different phenomena has been allowed by most writers, and Dr. Wolf's period of 11⅑ years has, in consequence, been accepted by many of the most eminent men of science who have had occasion to allude to the subject.

Having had to study this question in connection with the results of observations made during twenty-three years at Trevandrum, I have examined with care the magnetic observations of the last and the present century, determined the exact times for which the yearly mean diurnal oscillation of the magnetic needle was a maximum or minimum, and have arrived at the following conclusions:

1st. That there are not sufficient grounds for rejecting the observations of Gilpin, which appear to be in general trustworthy as regards the change of mean position of the needle from year to year, and of the diurnal range from winter to summer.

2d. That these observations should, according to the mean law, show a maximum near 1797, and another should have occurred near 1807. I have found that they do indicate a maximum in the former year; and though another maximum appears in 1803, that there are grounds for believing the maximum may really have occurred after 1806, when Gilpin's series terminated.

It has to be stated, however, that the maximum shown by Gilpin's observations in 1797 is very small; that the whole interval between the preceding and following minimum is not six years; and that no such short period and small minimum have been observed during the last half century. Since, however, the shortness of the period and the smallness of the maximum are both confirmed by the observations known to us of the frequency of sun-spots and of the aurora borealis, I can only conclude, in conformity with the facts, that both these were real phenomena, which may yet be repeated and aid in the determination of the cause of the decennial period. The mean duration of the period at which I arrive is therefore almost exactly that which Dr. Lamont had previously obtained, or 10.45 years.

For this result the facts have been taken as they present themselves; since it would be difficult to conclude that the observers of all the three phenomena could have erred in the same way during nearly twenty years. In addition to this, after a careful study of Dr. Wolf's sun-spot numbers, I find it impossible to accept his period of 11⅑ years. How ill the facts satisfy this result may be shown by two comparisons in which the epochs accepted by the Zurich astronomer are employed.

Thus a maximum of the magnetic oscillation occurred in 1787 by the observations of Cassini and Gilpin; this epoch has been confirmed nearly by Dr. Wolf's sun-spot numbers, and by Prof. Loomis for the auroral frequency. We have then the last observed maximum 1870.9, about which there can be no doubt. In the interval between these two maxima there were, according to Dr. Wolf, only seven periods, consequently we have—

$$\frac{1870.9-1787.3}{7}=\frac{83.6}{7}=11.94 \text{ years},$$

a period which differs as much from his mean period as that does from Dr. Lamont's. If on the other hand we take one of Dr. Wolf's sun-spot epochs about eighty years before 1787, and employ the number of periods he has himself given for the interval, we find—

$$\frac{1787.3-1705.5}{8}=\frac{81.8}{8}=10.23 \text{ years}.$$

If, then, we commence with the epoch of 1787 and compare it with any epoch of maximum since, we shall always find for the mean duration at the least 11.9 years according to Dr. Wolf; and if we compare it with any of the epochs given by him upwards of eighty years before, we shall never find a greater mean than 10.75 years, and this result includes an interval of 172 years before 1787, with all the uncertainty of the earlier epochs. This great difference of more than one year in the mean duration, as derived from eighty-four years after 1787, and eighty-two to 172 years before, disappears to a great extent if we admit three periods between 1787 and 1818.

It has been already remarked that the duration of a period is not constant, but varies within certain limits. The question naturally presents itself—does this variation follow any law, or is it accidental, increasing one year and diminishing the next? The number of periods for which we have the epochs of maxima and minima of the diurnal oscillation of the magnetic needle accurately determined, is not sufficient for any very sure reply. At the same time the results I have obtained indicate a period of nearly forty-two years for the repetition of the variations in question; and if this conclusion is confirmed by next maximum, that should occur in the year 1879. It may also be pointed out that according to the law of forty-two years a maximum should have occurred in 1818—42 =1776. Now this year, according to Dr. Wolf, was a year of minimum. The variation of his sun-spot numbers for that period, it appears to me, is not sufficient to give his conclusion much weight; while, on the other hand, Van Swinden's result, which it is extremely probable was a consequence of the decennial law, gives 1776 for the year of maximum; and that it was so is further supported by the magnetic observations of Cotte, at Montmorency. The exceptional period about 1797 shows, however, that any definite conclusion from observations during the last sixty years may be impossible, since causes of variation exist with which we are insufficiently acquainted as yet.

When we compare the mean range of the diurnal oscillation of the needle for the year in which it is a maximum with that for the year of minimum at any station, we find that the ratio of the two is very nearly constant for places so widely separated as Toronto, Dublin, Trevandrum, and Hobarton. I have also found that the *law* of the diurnal movement is the same in the year for which the range is least, and in that for which it is greatest. This shows that it is the same cause which is acting, the variation being one of intensity only. Since few or no sun-spots are visible in the years of minimum range, we perceive that the sun-spots happen only when the intensity of the force producing the magnetic variations exceeds a given value. It also appears that considerable variations in the amount of magnetic disturbance may exist near the equator when there are few or no sun-spots; and, on the other hand, that the spotted surface of the sun may be a maximum, and no corresponding increase of the magnetic oscillations be visible. The latter are, however, exceptional cases, since increases of sun-spots and of magnetic movements occur frequently near the same time; the increase of the one, however, bears no constant proportion to that of the other.

It has been already stated that the ratio of the diurnal oscillation of the needle in the year of maximum to that in the year of minimum is very nearly constant for places very widely separated from each other; there are, however, slight variations in the ratio shown at some places; thus, although it is nearly the same at Toronto, Dublin, Trevandrum, and Hobarton (1.55), it is slightly greater for Munich and Lisbon (1.71). This is probably due to the action of disturbances which are known to obey local laws. I have also found for Trevandrum, nearly on the magnetic equator, that the disturbances, or the deviations of the magnetic needle from the mean position, do not show exactly the same epochs of maximum and minimum in the decennial period when different hours are considered. Thus, though the cause is cosmic, the actions appear to be influenced, though but slightly, by circumstances of locality.

When we seek for the cause of the decennial period, we are met at first by the three phenomena which obey this law: the magnetic variations, the sun-spots, and the aurora borealis. The connection between the first and third is so marked, that if a magnetic disturbance commences during the day in a high latitude, it is quite certain that the aurora will be seen as soon as the disappearance of sunlight permits. This is a fact I have verified during several years' observations in the south of Scotland. Both these phenomena are results of electrical motions. It did not seem improbable then that the solar spots might be connected with disturbances of electrical equilibrium, and that these might be due to the different electrical states of the sun and of the planets.

We do not know, however, of any planet with a period of ten and a half years, nor of any combination of planetary positions which would produce such a period. My own researches have failed in connecting the variations of the sun's spotted surface with the time of revolution of any planet by a law which holds for different decennial periods. This fact, however, does not disprove a planetary action. We are unacquainted with the nature of the medium through which the electrical actions producing the magnetic variations are conveyed. Physicists seek to reduce the phenomena of nature to the fewest possible factors: many then have been induced to believe that electrical and magnetical actions are conveyed by the same ethereal medium which we believe transmits heat and light. The facts do not appear to be easily explained by such a hypothesis; thus I have found that certain electrical actions of the sun producing marked diminutions of the earth's magnetic force happen exactly at successive intervals of twenty-six days; when one point of meridian of the sun returns to the same position relatively to the earth; this action, similar to that of a beam of light reflected from a revolving mirror, which illuminates a particular point only at the same part of its revolution, has no resemblance to that of light and heat, which are propagated equally in all directions.

If, then, we can suppose, that the electrical medium is disposed unsymmetrically around the sun, that the disposition and extension varies, it is obvious that the supposed planetary actions would also vary, and might be quite different for different parts of their orbits, in different decennial periods. This suggestion may explain why I have not been able to find a law remaining the same in the different periods; and it is not opposed to the conclusions of Messrs. De la Rue, Stewart, and Loewy, who have found very remarkable relations between certain positions of the planets and the amount of the sun's spotted surface during a single decennial period.

Any hypothesis which seeks to explain the mode of production of the sun-spots (by cyclones or otherwise) must also explain why the causes become insufficient for their production every ten and a half years.

M. Faye, the distinguished French astronomer, considers that the prime cause of sun-spots is to be found in the excess of heat radiated; so that the spots are the symptoms of a dying sun; that we have in fact here a phenomenon like the flickering of an expiring lamp which may have a periodical character. Such a hypothesis will scarcely satisfy the demands of science, but we must evidently wait for more facts before any satisfactory theory can be proposed.

DAMP HOUSES.

From "The Architect."

At the close, as we are now fain to hope, of a winter, which has not only been the wettest within the memory of man, but which, according to all accounts, has swum over the land in almost double the average depth of rainfall, there may perhaps be a good many people with whom the question of domestic damp is one of considerable melancholy interest. With one it is the rain that has "come through the ceilings." With another it is the same rain that has "driven right through the walls." Here the water has risen in the cellars, or in the "kitchens," until trim waiting maids have had to navigate them in a tub. There the mere "damp" has proved almost more unmanageable than any amount of palpable wet, by creeping up the walls of the rooms, or making the carpets and floorcloth as mouldy as old cheese, or actually procuring the importation, from some mysterious source under the floors, of fungi by the square yard, or by the pound, or by the bunch of mushrooms bursting playfully into the light through the crevices and crannies of the skirtings. If, therefore, we submit a few observations of a practical character upon some of these miserable manifestations of water in the wrong place, it is not at all unlikely that some of the sufferers may take heart of grace for some other wet winter, by reason of the acquirement of a little elementary knowledge upon the unpleasant but solemn subject.

When a damp spot appears in a bedroom ceiling after a shower of rain, the cause is generally too simple for any elaborate explanation. There is a slate or a tile cracked or displaced; and it must be repaired, and perhaps the whole roof examined. But at any rate we may take occasion to allude to the well-known complaint that when the slater mends one slate he breaks three. There is a very intelligible fact at the bottom of this. A roof slate is the most brittle of all building materials—except of course glass. It has to be fixed in its place with two nails. If it should be fixed in any degree awry—and it is ten to one that it is—sooner or later the slate becomes cracked. This may be by frost, or even by wind; but it is obviously much the most likely to occur by the passage of a man's weight over the surface; and hence the mending of one slate may cost the breaking of three dozen as easily as of three, if the slater be only sufficiently painstaking in his examination.

Where snow drives in through the crevices of the slating, this is only to be remedied by pointing or stopping up all those crevices from within—a thing which of course ought to have been done at the first in any circumstances wherein it must require to be done at the last.

The case of wet coming through the solid wall is a very common one. The rain is dashed against the outer surface by the wind; the water clings to the surface; the wind is simply so much pressure per square foot brought to bear upon this water; and if the porousness of the wall material—stone or brick—happens to be such as to allow this pressure to force the water through its whole thickness, the wet must of course appear inside. A south-west aspect in an exposed situation in the country, with a sandstone wall, or with a pretty red-brick facing, is eminently favorable for this transmission of the wet; and indeed a strong wind will carry it sometimes straight through as much as eighteen inches in thickness of solid brickwork, or a good deal more of solid stonework—one of the most experienced of masons going so far as to say that he knew no stone in the British Islands (granite not being overlooked) which would hesitate to carry the damp through a thickness of three feet if fairly put to the test. Constructive precautions against this kind of damp-plague are very well understood and constantly put in practice. To form a hollow wall is the commonest plan. To line the inside of a stone wall, whether hollow or not, with hard brick is a thing often done. To avoid in every possible way the carrying of header stones through the whole thickness of the wall is a self-evident rule. To batten and lath the inside, instead of plastering directly on the stone or brick, is most effectual. But it must be borne in mind that this battening—which in fact leaves an inch or so between the wall and the plastering—only conceals the damp after all; for even if it be nothing more serious than a case of the mere absorption of passive moisture by soft stone, whatever amount of wet actually comes through to the inner surface is obviously left there behind the lathing, to evaporate as it best can.

To remedy the defects of a wall of this class is now an easy task. The ends of header-stones may have to be cut away and patched; or the entire surface may have to be re-plastered on battening and lath; or some kind of paint or paper or the like which is impervious to water may be applied to the surface—although it is difficult to see how anything of this kind can be really effective except in cases of a very slight moisture, affecting for instance the wall-paper and no more. It may be fairly alleged, however, that the application of certain washes or paints externally may prove much more serviceable. In this instance the first access of the damp may possibly be prevented, whereas in the other it is only its exit that is attempted to be stopped.

Where the basement rooms become flooded, this is manifestly due to some one of half-a-dozen causes of an altogether different character which it would be impossible here to discuss. One of these causes, however, may be alluded to as being exceptional in its nature and in its effects, namely, the rising of underground water at almost unexpected times. This occurs generally in marly or gravelly soils where there is a substratum of clay. This surface-bed is connected, perhaps at a considerable distance, with similar soil at a higher level, the clay substratum extending from the one locality to the other. The result is, after rain has fallen heavily on the remote higher level, that it finds its way along the back of the under-clay to the lower level and then breaks out in the form of land springs. If this be, as it probably will be, an incurable evil (unless one is prepared to make the basement a waterproof tank), there may at least be some sort of dismal satisfaction in understanding the reason of it.

The ascent of damp from the ground through the substance of walls is far too common both in town and country. The process of it is easily described. The foundation of the wall being built of some soft quality of brick or stone, the water will rise directly through this material by capillary attraction if encouraged. Consequently when the soil comes to be wet below, the warmth of the house within is quite sufficient to encourage the water so to rise. The common preventative is to introduce in the substance of the wall during the course of building a layer of some damp-proof material, such as slates or asphalte. This keeps back the ascent of the moisture and leaves the foundation alone saturated. But it is plain that if this simple measure be not carried out at the first it is generally almost impossible, or quite so, to adopt it as a remedy after the defect appears; and all that can be said is that, unless the expense can be encountered for inserting a layer of slates by the laborious process of cutting out the stone or brickwork foot by foot, there is perhaps nothing else to be done but to try the makeshift of applying impervious paint, or metallic cement, or whatever other nostrum may offer.

When damp is found to ascend into the house from the whole surface of the soil under it, that is to say, probably through the floors of the basement, this is not difficult to understand. The soil being charged, as matter of course, with atmospheric air, when the temperature without is lower than within—as in the night-time—the cool external air necessarily forces its way into the warm house by every means of access, and, amongst the rest, through the subsoil. If, therefore, this subsoil be damp, the consequence may be that a mouldy smell will manifest itself within the house by reason of the ascent of air from the ground, and it is more than probable that this smell will also indicate the presence of foul gas as well as mere water. The remedy for all this is to take up the floor and make an impervious bed or surface underneath as a covering for the soil. Concrete is no doubt the most handy material for this purpose, but it ought to be good or it may fail to stop either the moisture or the gas.

Our last subject shall be that which is commonly called dry-rot. This is a species, or rather any one of several species, of fungus, which, when established on any surface of wood, runs with rapidity over the whole, and at the same time throws its filaments into the substance. A piece of wood thus attacked by dry-rot soon becomes coated over with either a delicate fibrous tissue or a coarse leather-like skin, and is consumed throughout by what is very expressly called by the name it bears—rot. The plant itself happens to be especially fostered by a combination of damp and warmth, and still more signally when these combined conditions exist without the presence of ventilation. Logs of American pine, for instance, when taken from the hold of a vessel, are not unfrequently found to be affected more or less with dry-rot, and it is well known that the most serious standard example is when the fungus attacks the timber of a ship for want of air. As a theoretical principle, the provision of ventilation effectually prevents the disease from being initiated; and as a general rule, it is even asserted that the subsequent admission of air will check its progress, and indeed cure it so far as it happens to be curable. The fungus, in a word, dies in fresh air. When, however, it has come to any head before discovery, it is scarcely necessary to say that the floor must be taken up, and the skirtings and other parts removed, and every infected portion cut away carefully for the substitution of a sound wood. It is also desirable to apply a wash of some such liquid as diluted carbolic acid. Beyond all other things, a circulation of air must be set up by the insertion of air-bricks or whatever else under the floor, so that, whether the air will kill the fungus or not, it may at least prevent it from spreading.

No doubt there are other forms of the domestic damp disease which one and another reader may be able to recall to recollection from wretched experience, but probably these are the chief. At any rate the man is always to be pitied who lives in a damp house—unless he be some hardy amphibious person who can afford to say that he disregards such trifles.

Into the details of what diseases of the body—and of the mind—an average Englishman becomes liable by dwelling in damp we do not desire to enter; the discomfort of the thing or even the objectionable appearance of it, is enough for our purpose, and we are certainly offering good advice to every one who has this to complain of when we say that he ought to get at the root of it as soon as he can, and, in so far as it is possible, make an end of it.

CO-ORDINATE SURVEYING.

By HENRY F. WALLING, C. E.

Transactions of American Society of Civil Engineers.

Relation of Surveying to Geodesy.—Surveying is commonly defined as the art of measuring, laying out and dividing land. The similar but more comprehensive art of geodesy applies to the earth itself, as indicated by the etymology of the word, which is derived from γη, *the earth*, and δαιω, *I divide*. Its objects are, the determination of the form and dimensions of the earth and of its different portions; the continents and islands with their lakes, rivers, mountains, civil divisions, etc. Its processes, like those of astronomy, upon which, indeed, it is to a considerable extent dependent, require instruments of a high degree of precision and mathematical computations of great refinement.

General geodetic Co-ordinate System.—In the operations of the great geodetic surveys of the world, including for our own country those of the United States Coast Survey, positions are finally determined by referring them to coordinate planes. The plane of the earth's equator forms one of these co-ordinate planes and that of an assumed standard meridian another. If we should assume that the earth is spherical in form, with continental elevations, the position of any geodetical point could be fixed by determining the direction in space of its radius vector, as referred to the two coordinate planes, and the length of this radius vector, the origin being at the centre of the earth. The angle with the equatorial plane made on either side by the radius vector would be the latitude; the angle made by its projection upon the equatorial plane with the meridional plane, measured in either direction around a semicircle would be the longitude, and the length of the radius vector, or rather its excess over that to the level of the sea would be the altitude of the place determined. Owing, however, to the spheroidal form of the earth, latitudes as observed and established do not exactly represent the co-ordinate angles here described. The actual latitude of a place is the angle made by a normal to the earth's surface at that point with the equatorial plane. The geocentric angle could easily be computed from the latitude if the earth were an ellipsoid of revolution of known eccentricity.

Irregular Form of the Earth.—But it has been found that this is not the true form of the earth. The equator, and probably all parallels of latitude, taken at the level of the sea, vary more or less from true circles, indicating a want of homogeneity in the materials which make up the earth's volume. Longitudes are measured by noting the earth's rotation angles on the undoubted assumption that its angular velocity is strictly uniform. By noting the difference in time between the passage of a normal to the earth's surface at any particular point, and of another normal at any point on the standard meridian, across a celestial meridian, we obtain an angle which is called the longitude of the former point, the longitude of the standard meridian being zero. Irregularities in the form of the earth of course change the direction of normals to its surface, and the co-ordinate system of Latitudes and Longitudes, used in Geodesy and Navigation, is correspondingly irregular.

Its Determination a difficult Problem.—In consequence of these irregularities, the problem of determining the exact form of the earth is an exceedingly difficult one. Instruments of the highest degree of precision must be used by skilled observers, and the combined results of a vast number of careful observations over widely extended areas must be subjected to the most refined mathematical investigation before its full solution can even be approximately obtained.

PROGRESS MADE IN ITS SOLUTION.—Its investigation has been going on, however, for one or two centuries, and very fair progress has been made. The great national surveys of the world have been conducted by men eminently qualified for the task, and the results of these surveys, so far as completed, seem to approach quite near to the attainable limits of accuracy.

UNITED STATES COAST SURVEY.—This is particularly the case with our own Coast Survey, which is probably unsurpassed, if not unequalled in precision and general accuracy. Our country, however, has thus far failed to realize some very important advantages which might be derived from these Coast Survey operations, although they have been sufficiently carried out to render them available over considerable portions of the country.

OBJECT OF THIS ESSAY.—It is the object of this paper to point out a simple method by which the high degree of precision which accompanies the Coast Survey work may be made available in the ordinary operations of land surveyors and civil engineers in those districts over which the Coast Survey triangulations have been carried, and at the same time to call attention to the importance of an extension of these triangulations over the entire country, either by the general or by State governments. One of the disadvantages of our peculiar confederate form of popular government is an apparent inability or indisposition to undertake works of acknowledged and eminent utility, unless they are popular with the masses or with those who control the machinery of elections. It is certainly the experience of foreign countries, including several less wealthy and prosperous than our own, that these surveys are many times more valuable than their cost, in the aid they afford to the carrying out of internal improvements, to the equalization of taxes, and to many of the necessary operations of general and municipal governments, as well as of private individuals. But in this country where legislation usually follows, instead of leading, the expression of general public opinion, such works are likely to await the slow and gradual cultivation of the popular mind to a proper appreciation of their great utility.

CONGRESSIONAL LEGISLATION.—In the meantime, it must be admitted that Congress has enacted a very liberal and wise law by which the Coast Survey is authorized to extend its triangulation over any State in which scientific surveys have been provided for by the State Legislature. Moreover the Superintendent of the Coast Survey evinces a disposition to construe this act with great liberality. It is understood that he will, where thorough topographical surveys are undertaken by State authority, cause the subsidiary triangulation to be carried to any reasonable degree of minuteness with the same refined accuracy which has characterized the work already done by the officers of the survey. Indeed, with their elaborate determination of local irregularities in the form of the earth, and their well organized corps of skilled observers and computers, they have an immense advantage, which could hardly be soon attained by any new organization, for extending the triangulation over whole States and carrying it to the secondary and tertiary stages.

TRIANGULATION OF MASSACHUSETTS.—The State of Massachusetts is entitled to the credit of being the first of the United States to recognize the importance of having her territory carefully surveyed and to commence upon such a work. More than forty years ago, a triangulation was commenced which was completed in 1842. This triangulation will compare favorably with the Coast Survey work and with the other geodetic surveys of the world. But a surprising indifference to its value, or potential utility, seems to have prevailed, and up to the present time, no further use has been made of it than to adopt it for the basis of such State and county maps as have from time to time been published. These maps give only the horizontal locations of objects obtained from such imperfect surveys as could be paid for by the sale of maps, published entirely by private enterprise, no assistance being given by the State.

STATE SCIENTIFIC SURVEYS.—At the present time, the subject of scientific surveys is being agitated in the States of Massachusetts, Rhode Island, Connecticut and New York. The latter State appropriated $20,000, in 1876, for preliminary organization, to effect which it appointed a skilled director. The other States mentioned have organized commissions to investigate and report on the subject.

REFORM NEEDED IN LAND SURVEYING.—Before proceeding to describe the proposed combination of surveying with geodesy, a brief consideration may be permitted of the urgent need of reform in the prevalent methods of land surveying and of writing descriptions in conveyances of land, based, as many of these descriptions necessarily are, upon imperfect surveys, and frequently upon no surveys at all. Lawyers and land surveyors are perhaps most familiar with the short-comings of these conveyances in failing properly to describe the property conveyed. A large share of the litigation of the country arises from this cause. Indeed the laxity in this respect is something almost incredible. Scarcely one deed of conveyance in a hundred will be found to contain such a description of the land conveyed as would fix its location with certainty, if the fences, walls or other inclosures should become obliterated, a contingency which is quite likely to arise.

CONFLAGRATION OF DETROIT.—An occurrence of this kind on a rather extensive scale took place in 1805, when the city of Detroit was devastated by fire, and so thoroughly destroyed, that it was found quite impossible to ascertain the former boundaries of estates. The restoration was entrusted to the Governor of the State and a council of judges, who could find no better way out of the difficulty than by re-laying out the city on an entirely new plan, dividing the lots among the former owners as equitably as possible.

Notwithstanding this experience, the lines of streets and lots in Detroit are now so uncertain that disputes and litigation in regard to them are of continual occurrence. The same is true in most of the cities and large towns of the United States, especially in suburban districts and growing villages where land is rapidly increasing in value.

FAULTY DESCRIPTIONS IN LAND CONVEYANCES.—If we examine the descriptions given in land conveyances, we shall find that they usually fail to fix either the location of the property by references to permanent land marks, or even the position of its boundary lines relative to each other. Frequently the tract conveyed will be bounded in the deed by the several tracts adjoining, of which the only description given is to state the names of the supposed owners. In many deeds, all dimensions are omitted and only an indefinite approximation to the quantity of land conveyed is given, the statement being that it contains about so many acres, "be the same more or less." Where good permanent division fences, walls, hedges, ditches, streams, shore lines, bound-stones, stakes, &c., mark the boundaries, and are properly described, such descriptions may answer the purpose so long as the boundaries remain unchanged, although such indefiniteness as to quantity would hardly be tolerated in the sale of other kinds of property.

OBLITERATION OF MONUMENTS.—But physical monuments are continually becoming obliterated even when well defined at first. It is said to be an old custom in some parts of the country, to take children once in every year to important boundary corners, where monuments have been erected, to the location and surroundings of which the careful attention of the children is directed. If on a subsequent visit, their memory is found to be at fault, it is refreshed and deepened by combining with it that of a sound flogging.

UNRELIABILITY OF SURVEYS.—Even where surveys have been made, they are in many cases so unreliable that the recollection of old residents in the vicinity, considerately stimulated, perhaps in their youth in the way described, is more reliable in determining the proper location of lost boundaries, than the retracing of old surveys. This is not surprising when the modes of surveying and the character of the instruments used are taken into consideration.

Outside of cities and larger towns, the instruments usually employed in surveying are the chain and compass. The method is to perambulate the boundary line of the tract to be surveyed, taking the magnetic bearing with the compass, and measuring with the chain the lengths of each side of the polygon forming the boundary.

Imperfection of the Compass.—Now, provided the bearings thus taken were precisely measured angles from fixed parallel meridians, whose directions could always be easily ascertained, when a re-survey should be needed, no better method of noting directions could be desired. But this is far from being true. The magnetic force to which the direction assumed by the needle is due, is quite irregular in its action, changing its direction continually, backwards and forwards, even during the different hours of the day, while larger oscillations extending sometimes to many degrees of arc take place in irregular cycles, perhaps one or two centuries in duration. And this is not all. In regions containing metallic deposits, especially magnetic iron ore, very irregular and powerful local disturbances of the magnetic force arise, causing the needle to take widely different directions, even at points in close proximity to each other, thus destroying parallelism of action, and rendering the compass quite useless for ascertaining true directions.

Beside the uncertainty of the magnetic meridian, there is an incapacity of precision in the use of the compass for measuring angles. The needle must swing clear of the graduated limb, and cannot be suspended for field use, with very great delicacy. In practice it is usually impossible to read a magnetic bearing with greater precision than to the nearest ten minutes of arc.

Theodolite and Surveyor's Transit.—The theodolite and the surveyor's transit are instruments of far greater precision and are generally used in cities, and for more valuable farm lands, also for engineering works, roads, railroads, &c. Those in ordinary use, measure angles to single minutes.

In land surveying, the common method of using these instruments is to measure the angles formed by the sides of the bounding polygon with each other, and sometimes, for verification, with one or more diagonals. The compass needle, usually attached, affords an approximate means of ascertaining azimuths, but with no more precision than with the ordinary compass. It is accordingly, quite as difficult to retrace obliterated boundaries with these instruments as with the compass, unless one or more well-defined lines remain for reference.

Solar Compass.—Burt's solar compass now used in the surveys of the public lands at the West, for running out the parallel and range lines is a great improvement upon the magnetic compass in the accuracy of its azimuths if not in the precision of reading minute angles. The direction of the sun, with proper adjustment of the instrument for the latitude of the place and declination of the sun and hour of the day, affords of course, a reliable means of obtaining the true azimuth of an observed line with as much precision as the mechanical construction of the instrument permits. The use of the solar compass, however, is limited to sunshining or slightly cloudy days, the middle portions of which, moreover, are unfavorable to accurate observations, and at best, the precision of its angular measurements is much inferior to that attainable with the transit. Nevertheless, for the preliminary surveys of wild lands, where no trigonometrical survey has been made, and where rapidity and economy are required, as in the government surveys of western lands, it is a very convenient and useful instrument.

Measurement of Distances.—The direct measurement of distances is attended with even more difficulty than that of the determination of directions. It is accomplished by the repeated application of the chain, tape or measuring rod, as nearly as possible, upon the line to be measured. Passing over inaccuracies in the length of the measuring standard arising from imperfections of construction, inequalities of temperature, changes of length by stretching, kinking, &c., the difficulties of making the direct applications are frequently quite serious. It often happens that access to all parts of the line to be measured is difficult, if not impossible. In fact, the boundaries of improved properties are generally indicated by walls, fences, hedges, ditches, etc., which occupy a considerable width upon the ground, being partly upon either side of the dividing line, upon which it therefore becomes impossible to apply the measuring standard. In such cases, it is usual to measure the opposite sides of an imaginary parallelogram, equal offsets being made at the ends of the line, and the measurements effected between the offset points. The offset angles are quite frequently estimated by the eye, and brought as nearly as possible to right angles. Even if these angles are instrumentally measured, the operation becomes complicated, thus increasing the liability to error. Impassable obstacles along boundary lines are of frequent occurrence, and various expedients, more or less complicated, are resorted to for ascertaining the distances through them. The difficulties of measuring accurately over uneven ground, requiring a careful and laborious use of the plumb line, the avoidance of sagging or unequal stretching when the chain is used, the exact marking on the ground of the end of the measuring standard, &c., are familiar to all land surveyors.

Measurements of Angles.—It is easy to see that measurements of angles with good instruments can be performed with far greater ease and precision than the measurements of distances by the ordinary methods. Either mode of measurement is merely a determination of *ratios*. Thus, when we measure the length of a line by direct applications of a standard length upon the ground, we simply ascertain the ratio between the length of the measured line and that of the chain. So, if we measure the three angles of a triangle, we know by a simple computation, the ratio of its three sides to each other. The percentage of error in the ratios, as found by either method, is much less in the measurement of angles with good instruments; for although the distances which are compared together in this measurement, namely those marked in degrees along the limb of the instrument used, are many times smaller than those compared together in the measurement of distances upon the ground, the nicety of the mechanism and the ease of the verification by repetition admit a precision quite unattainable in actual ground measurements, except by slow and laborious processes.

Trigonometrical Survey.—In the trigonometrical survey, this superiority of angle measurement is practically recognized. Convenient points are selected, at suitable distances from each other, where the angles between imaginary lines, joining adjacent points, can be measured. The whole area to be surveyed is cut up by these lines into a network of triangles, and the ratios of the sides of these triangles to each other are determined by measuring the angles between them. Then, when we ascertain the length of any one side of any one triangle, we can compute all the sides of every triangle, or the distances from point to point throughout the whole survey.

Degree of Accuracy Attained.—The accuracy of the result, accordingly, depends upon the precision with which this one side or base line is measured, as well as upon the accuracy of the angle measurements. It is usual to verify the whole of the work by measuring another base line in a distant part of the network of triangles. For example, in the Coast Survey work, a base line was measured at Fire Island, on the south side of Long Island, in 1834; another in Attleborough, and Sharon, Massachusetts, in 1844; and another near the village of Epping, in Columbia, Washington county, Maine, in 1857. The distance between the Fire Island and Massachusetts bases, along the axis of triangulation, was 230 miles, and between the Massachusetts and Epping bases 295 miles. The length of the Fire Island base, 8715.942 metres, or 5.415 miles, as actually measured, varies from the length, as computed from either of the other two bases, by less than 0.07 metres, or 2.75 inches; and the probable error of any computed line between these two bases is shown by careful analysis not to exceed $\frac{1}{288000}$ of its entire length. This proportion of error to distance amounts to 0.22 inches in a mile, or a little less than 2 feet in 100 miles.

This degree of accuracy indicates the wonderful skill which has been attained in the construction and use of instruments both for the measurement of base lines and of angles. Thus, if we compare the actual distance upon the limb of a theodolite, 30 inches in diameter, which corresponds to an error of $\frac{1}{288000}$, which we will suppose to be all thrown into one of the three angles of a single well-conditioned or nearly equilateral triangle, we shall find it equivalent to about $\frac{1}{20000}$ of an inch,* being about 0.71 seconds of arc.

The percentage of error here developed is so small that it would not practically vitiate the measurement of lands even in the most valuable localities of great cities.

Superiority of the Trigonometrical Method.—The degree of precision commonly attained in direct measurements of distances by ordinary methods falls very far short of this, and even of that attainable in angle measurements with the ordinary surveyor's theodolite or transit.

It follows that accuracy, even in common surveying, would be promoted by using the method of triangulation for ascertaining ratios, whenever practicable or convenient, in preference to the common methods of traverse surveying. In the survey of a field, or tract of land, for example, triangulation from one or two judiciously selected and carefully measured bases would give the positions of the corners and other objects with greater accuracy and far less labor than the usual routine of perambulating the outside boundaries, which is now taught in treatises on surveying, and generally practiced by surveyors.

Advantages of Combining Surveying with Geodesy.—With the facilities afforded by the Coast Survey triangulations, when carried to the tertiary stage, it is not difficult to perceive that a general method of determining the positions of points by co-ordinates might be established, and that while determinations thus made would be far more satisfactory and definite than those obtained by the ordinary methods of surveying, they would involve no more labor. Under such a system, the field work might consist of such a combination of triangulation and traverse surveying as would be found most convenient under the special circumstances.

To carry out such a system, the trigonometrical stations should be located so near together that two or more of them would be available for any subsequent local survey. If not otherwise visible, there should be convenient arrangments for the temporary erection of signals upon stations to indicate their positions, making them visible from adjacent stations.

Transformation of Co-ordinates.—While for geographical purposes, the great co-ordinate planes already described are the most suitable for reference, simplicity and convenience would be promoted by transforming this general co-ordinate system into numerous plane rectangular systems in limited local areas. Accordingly, instead of defining the position of a point by giving its terrestrial latitude and longitude, we would give its "latitude and departure," or its co-ordinates, from the zero point or origin of co-ordinates for the containing area.

For this purpose the local areas, into which the earth's surface is sub-divided, should be made sufficiently small to reduce the error which would arise from considering each separate area, or plane surface, to an inconsiderable amount. Six miles square is not far from the average area of townships in the northern, middle, western, and most of the southern States. The bulge of curvature, or the versed sine of half the arc of 6 miles would be about six feet. This would make the proportional difference between the straight and the curved distance, or between the length of the cord and of the arc, much less than the percentage of probable error inseparable from measurements of the utmost attainable precision in actual practice. Township boundaries, therefore, would seem to afford the most convenient divisions between separate co-ordinate systems. If, however, as is the case in a few of the southern States, no smaller subdivisions than counties exist, these are not usually so large that the use of a single co-ordinate system over its areas would involve any important error.

Direction of Axes.—The most appropriate and convenient directions for the axes would doubtless be found in meridians and perpendiculars to them,

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* More exactly 0.000052 inches, for we have radius = 57,3°, nearly, in terms of arc; or making radius = unity, $1^\circ = \frac{10}{573}$, $1'' = \frac{1}{206280}$ and $\frac{1}{288000} = 0.71''$ nearly. Conversely in terms of distance, if radius = 15 inches, $\frac{15}{288000}$ = 0.000052 inches.

since azimuths reckoned from the meridional axis would then conform very nearly to the true astronomical azimuth. Owing to the convergence of meridians, there would be a small variation from the true azimuth increasing with the distance from the axis, but no practical error need arise from this cause. The true azimuth, if needed, is easily computed by the formula,

$$\text{tan. } \tfrac{1}{2}\, C = \text{sin. } L \text{ tan. } \tfrac{1}{2}\, P,$$

in which L is the middle latitude between the origin and the point where the convergence is to be computed, P the difference of longitude between the same points, and C the convergence sought. In passing from one district to another, however, a certain degree of complication arises, and it becomes necessary to take the convergence into account. We may, without practical error, consider any two adjacent districts as lying in a plane produced by developing a conical surface tangent to the earth on the middle parallel of latitude between the origin of the two districts. On this plane, the convergence of the meridional axes of small districts will conform to the above formula, and the small angle of convergence measures the change of direction between the co-ordinate systems of the two districts.

PASSING FROM ONE DISTRICT TO ANOTHER.—In the passage from one district to another, four different cases of transformation of co-ordinates arise, namely:

1st. When the origin of the new system is east and north of the origin of the old system.

It will, of course, be necessary while assigning positive values to latitudes and departure in one direction to give negative values in the opposite direction; thus if north and east are to be reckoned as positive, south and west must be reckoned as negative.

In this first case, the old co-ordinates of the new origin are, accordingly, positive while the new co-ordinates of the old origin are negative. Or, if we call the old co-ordinates a and b, and the new ones a' and b', a and b are here positive and a' and b' negative.

2d. When the new origin is west and north of the old, or a negative, b positive, a' positive and b' negative.

3d. When the new origin is west and south of the old, or a and b negative, and a' and b' positive.

4th. When the new origin is east and south of the old, or a positive, b negative, a' negative and b' positive.

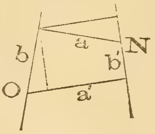
For ascertaining the new co-ordinates of the old origin, the equations—

$$-a' = a \text{ cos. } \phi + b \text{ sin. } \phi,$$
$$-b' = b \text{ cos. } \phi - a \text{ sin. } \phi,$$

would prove correct in all these cases, provided proper positive and negative values were given to the different terms in the equations. It is necessary to remember, however, that ϕ, the angle of change in direction, between the old and new axes, is positive according to trigonometrical usage when reckoned from zero around towards the left, and negative in the opposite direction, while according to the geodetic method of estimating azimuths, positive angles are reckoned around to the right.

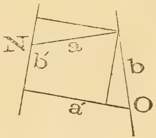
To avoid the confusion which might arise from these different methods of estimating angles, or from assigning a negative value to ϕ, equations are given below for each of the four cases. Their correctness will be apparent on simple inspection of the accompanying figures, in which O and N are the old and new origins respectively:

Fig. 1. Case First.



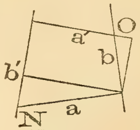
$$a' = a \text{ cos. } \phi + b \text{ sin. } \phi,$$
$$b' = b \text{ cos. } \phi - a \text{ sin. } \phi.$$

Fig. 2. Case Second.



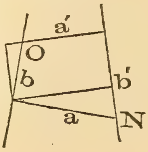
$$a' = a \text{ cos. } \phi + b \text{ sin. } \phi,$$
$$b' = b \text{ cos. } \phi - a \text{ sin. } \phi.$$

Fig. 3. Case Third.



$$a' = a \text{ cos. } \phi - b \text{ sin. } \phi,$$
$$b' = b \text{ cos. } \phi + a \text{ sin. } \phi.$$

Fig. 4. Case Fourth.



$$a' = a \text{ cos. } \phi - b \text{ sin. } \phi,$$
$$b' = b \text{ cos. } \phi + a \text{ sin. } \phi,$$

In these equations the negative sign is omitted before a' and b', but we must remember that they are always estimated in directions opposite to those of a and b.

Since ψ is a very small angle, *cos.* ψ approximates closely to unity, and it appears by these equations that a' is greater than a, and b' is less than b when the new origin is farther *north* than the old, while a' is less than a, and b' greater than b when the new origin is farther *south* than the old.

Having found the new ordinates of the old origin, that is of O referred to N, the ordinates of any point referred to the new origin may be computed from its old ordinates by the equations:

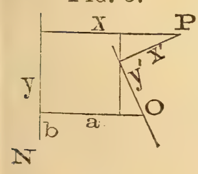
$$x = x' \cos.\psi - y' \sin.\psi + a$$
$$y = x' \sin.\psi + y' \cos.\psi + b;$$

in which x =departure of any point an referred to N,
x'=departure of the same point as referred to O,
y =difference in latitude of any point as referred to N,
y'=difference in latitude of the same point as referred to O,
a=departure of the origin of O as referred to N,
b=difference in latitude of the origin of O as referred to N,
ψ=the angle of convergence of the meridional axes of N and O.

These are the formulæ for passing from one system of rectangular co-ordinates to another in the same plane.

In these equations, we may consider north and east to be the positive directions, as before, the opposite directions being negative. Also *sin.* ψ is positive when the change of axial direction is towards the left, and negative when in the opposite direction; that is, positive when N is farther east than O, and negative when farther west. *Cos.* ψ will always be positive, since ψ is always either in the first or fourth quadrants.

FIG. 5.



In Fig. 5, the point P, of which x and y are the new co-ordinates, is east and north of the axis passing through O, making x and y greater than a and b respectively, and all the terms of both equations have, accordingly, positive values. But if x, x' or a be estimated towards the west, its sign must be reversed when particular values are substituted in the equations; likewise, if y, y' or b, have a southern direction, its sign must be reversed.

CO-ORDINATES IN A SINGLE DISTRICT.—It is hardly necessary to say that when the azimuth and distance are given from a point whose co-ordinates are known to any other point, the co-ordinates of the latter are found by multiplying the given distance by the sine and co-sine of the azimuth, the first product giving the departure and the other the difference of latitude.

VERIFICATIONS.—Before finally establishing the co-ordinates of a survey its accuracy should be tested in the most rigid manner, both as regards the instrumental observations and the computations. The computations are easily verified by working to the same point from different directions. Some methods of verifying the field work are indicated in the accompanying plates and explanations. Others will suggest themselves to the surveyor under different circumstances.

ILLUSTRATIONS OF NOTATION.—Plates I and II, exhibit the sort of notation which may be employed under the system proposed. Instead of magnetic bearings or angles written between lines, azimuths are given, which are estimated around to the right from zero at due north to 360° or due north again. From these azimuths, angles around to the right are easily found by subtracting the first azimuth from the second, adding 360° to the latter if zero comes between.

The co-ordinates are here given in feet, but metres, chains or any other standard units may be used in the same way. The letters N. E. S. W. indicate the directions for the origin, north, east, south or west. Points upon the division lines between two adjacent districts have their co-ordinates give in both. For railroad surveys, it would be found convenient in plotting to have the co-ordinates of tangent points and centers of curves given, even though the latter should not appear upon the plan. Other convenient details of notation will suggest themselves to engineers, and indeed the plates are only intended to present to

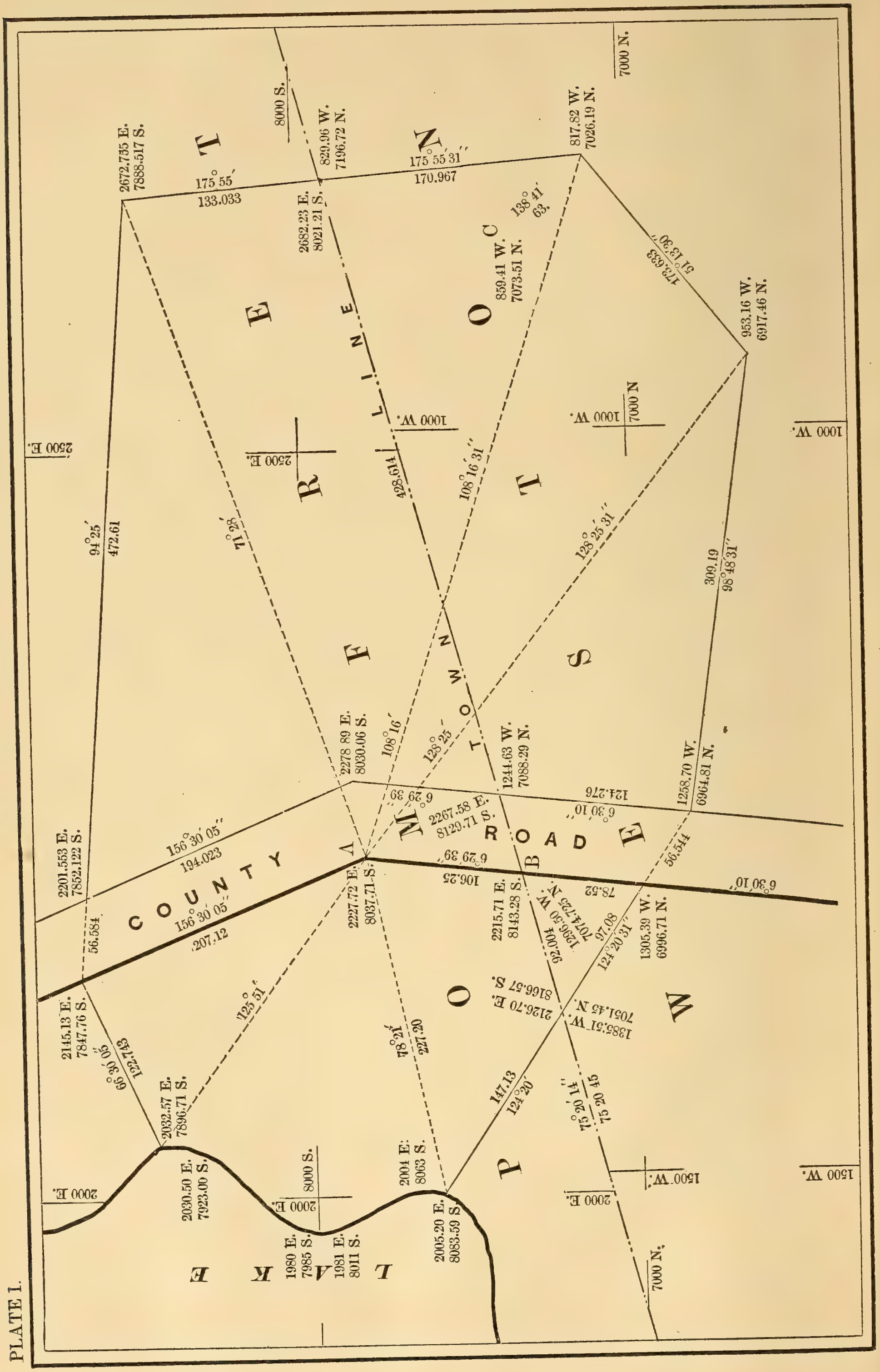

TOWN OF RENTON
TOWN OF POWSET
TOWN LINE
COUNTY ROAD
LAKE
8000 S.
7000 N.
1000 W.
1500 W.
2000 E.
2500 E.
2672.735 E.
7888.517 S.
175° 55′
133.033
829.96 W.
7196.72 N.
2682.23 E.
8021.21 S.
175° 55′ 31″
170.967
817.82 W.
7026.19 N.
138° 41′
63.
859.41 W.
7073.51 N.
51° 13′ 30″
173.633
953.16 W.
6917.46 N.
428.614
108° 16′ 31″
128° 25′ 31″
94° 25′
472.61
71° 28′
309.19
98° 48′ 31″
2278.89 E.
8030.06 S.
108° 16′
128° 25′
1244.63 W.
7088.29 N.
1258.70 W.
6964.81 N.
124.276
6° 30′ 10″
6° 29′ 39″
2267.58 E.
8129.71 S.
2201.553 E.
7852.122 S.
156° 30′ 05″
194.023
156° 30′ 05″
207.12
56.584
2227.72 E.
8037.71 S.
106.25
2215.71 E.
8143.28 S.
78.52
56.544
1305.39 W.
6996.71 N.
97.08
124° 20′ 31″
92.004
1296.30 W.
7074.25 N.
2126.70 E.
8166.57 S.
1385.51 W.
7051.45 N.
2145.13 E.
7847.76 S.
66° 30′ 05″
122.743
125° 51′
78° 21′
227.20
147.13
124° 20′
75° 20′ 14″
75 20 45
2032.57 E.
7896.71 S.
2030.50 E.
7923.00 S.
1980 E.
7985 S.
1981 E.
8011 S.
2004 E.
8063 S.
2005.20 E.
8083.59 S.

PLATE 1.

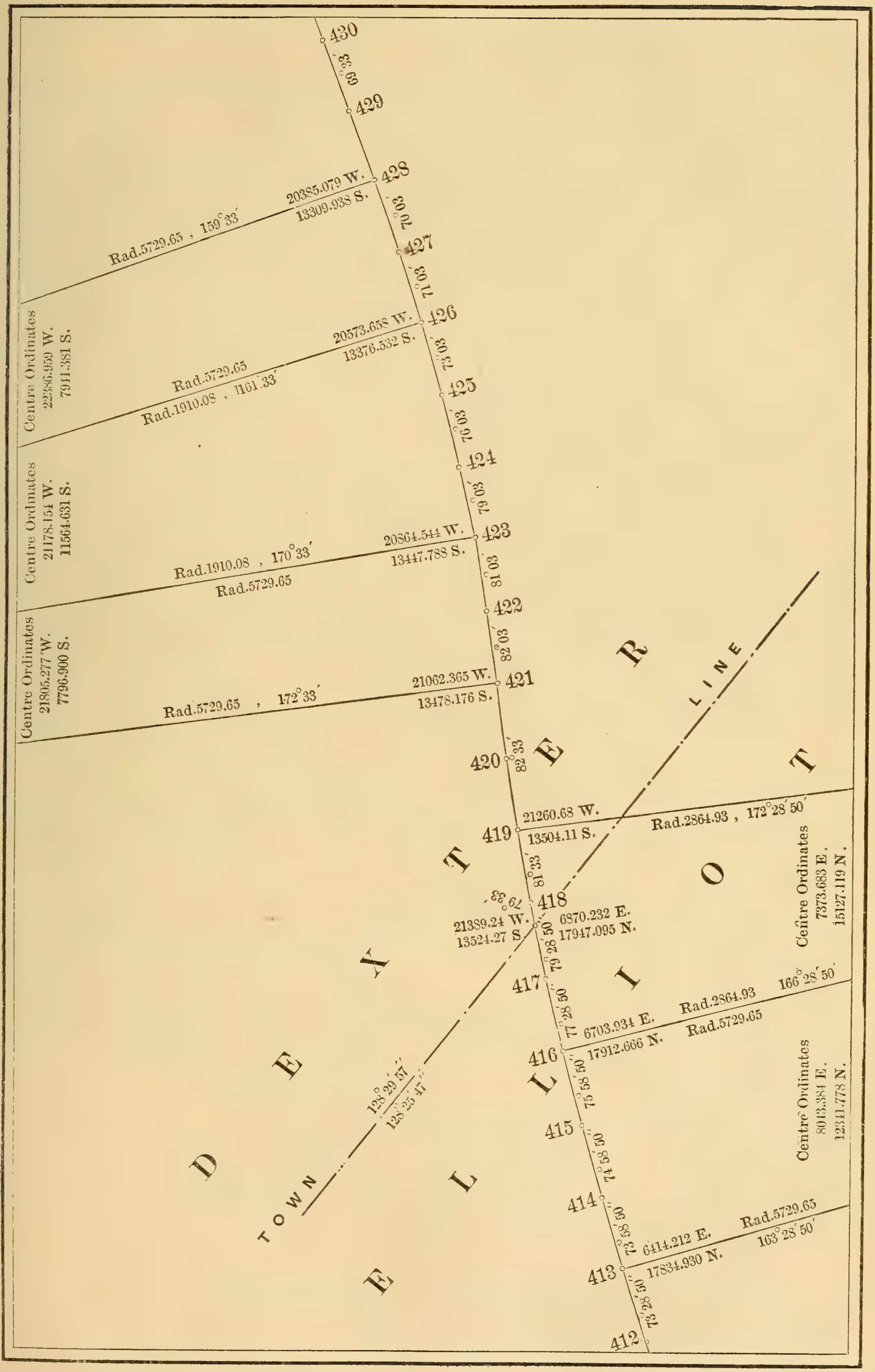

430
69°33′
429
428
20385.079 W.
13309.938 S.
Rad.5729.65 , 159°33′
70°03′
427
71°03′
426
20573.658 W.
13376.532 S.
Rad.5729.65
Rad.1910.08 , 161°33′
Centre Ordinates
22386.959 W.
7911.381 S.
73°03′
425
76°03′
424
79°03′
423
20864.544 W.
13447.788 S.
Rad.1910.08 , 170°33′
Rad.5729.65
Centre Ordinates
21178.154 W.
11564.631 S.
81°03′
422
82°03′
421
21062.365 W.
13478.176 S.
Rad.5729.65 , 172°33′
Centre Ordinates
21805.277 W.
7796.900 S.
420
82°33′
419
21260.68 W.
13504.11 S.
Rad.2864.93 , 172°28′50″
Centre Ordinates
7373.683 E.
15127.119 N.
81°33′
79°33′
418
21389.24 W.
13524.27 S.
6870.232 E.
17947.095 N.
79°28′50″
417
77°28′50″
416
6703.934 E.
17912.666 N.
Rad.2864.93
Rad.5729.65
166°28′50″
Centre Ordinates
8013.384 E.
12311.778 N.
75°58′50″
415
74°58′50″
414
73°58′50″
413
6414.212 E.
17834.930 N.
Rad.5729.65
163°28′50″
73°28′50″
412
123°29′57″
125°25′47″
TOWN LINE
DEXTER
ELLIOT

PLATE II.

the eye the general features of the method proposed. A great variety of cases will arise in practice, many of them requiring special treatment.

COMPUTATION OF AREAS.—Areas are computed under this system with special facility and certainty, the method being the common one of double latitudes and departures. This method is prescribed by law in the State of Ohio, for calculating areas of farming lands and for testing the accuracy of surveys made with the surveyor's compass.

DESCRIPTIONS FOR CONVEYANCES.—For definite and accurate descriptions of land in conveyances, it does not seem possible to devise a more precise and certain method than that of co-ordinates from geodetically determined reference points of origin. The present loose and indefinite descriptions in conveyances, upon which the tenure of a large part of the real estate of the country now depends, are disgracefully uncertain and frequently lead to excessive expenses of unnecessary litigation, and sometimes to costly errors of misplaced constructions.

CONVENIENCE IN CONSTRUCTING MAPS.—In the construction of maps and plans, co-ordinate determinations will be found especially convenient. After completing the survey of any portion of a district it is easy to place it in its proper position upon the map of the entire district, with the certainty that other portions subsequently surveyed will fit into their proper places, without the perplexity and the distortions frequently accompanying the attempts to unite two or more independent surveys made under the methods in common use.

SUMMARY OF ADVANTAGES.—A few of the advantages which may be expected to follow the general adoption of the co-ordinate method of surveying may be summed up as follows:

First.—The attainment of the highest practicable degree of accuracy, as well in smaller local surveys as in more extended operations. The units of measurement which form the basis of the United States Coast Survey have been most carefully compared with those of the entire civilized world, and with the dimensions of the earth itself, and are verified to a degree of precision beyond which the present attainments of scientific skill have not passed.

Second.—Extreme simplicity of notation with ease and convenience of field work and computation.

Third.—Facility in graphic representation.

Fourth.—Absolute certainty of locations in descriptions for conveyances, and consequent removal of a fruitful cause of litigation and trouble.

ALTITUDES.—No change is proposed in the existing methods of determining the third ordinate or altitude. The most convenient mode of fixing this ordinate pre-supposes that the form of the earth's surfaces, or of that surface which would be presented if the irregular surface of the land were razed to the level of the sea, has been accurately determined by geodetic operations, so that in ordinary surveying we have only to ascertain the heights of our points of survey above this imaginary level. This is done by using the spirit level, by measuring vertical angles and by barometrical observations. The first method admits the greatest degree of precision under ordinary circumstances, and is almost exclusively used for engineering purposes.

Plate I, represents a tract of land lying partly in Pomfret and partly in Weston, two adjacent towns. One side is bounded by a lake, and a road passes through the tract. Several of its corners are visible from the point A. The point B, where the town line crosses the west side of the road, is one of the stations fixed by the preliminary trigonometrical survey. All the co-ordinates upon this plan have been determined by computing the latitudes and departures, directly or indirectly, from this point. The convergence of the axial meridians in the two towns is 31″, and the co-ordinates of the origin in Weston referred to Pomfret are,

$$a=3513.27 \text{ E, and } b=15217.81 \text{ S.}$$

From these, we can compute the ordinates of the Pomfret origin referred to Weston, by the formulæ for Case Fourth:

$$a'=a \cos.\ \psi - b \sin.\ \psi,$$
$$b'=b \cos.\ \psi + a \sin.\ \psi,$$

substituting values

$(\cos.\ 31''=1,\ \psi \log.\ \sin.\ 31''=6.1769365)$

$a'=3513.27-15217.81 \sin.\ 31''=3510.983$

$b'=15217.81+3513.27 \sin. 31''=15218.338$

Reversing the directions,

$$a=3510.983\ W.\quad b=15218.338\ N.$$

Equations for passing from Pomfret into Western. The general formulæ are:

$$x=x' \text{ cos. } \psi-y' \text{ sin. } \psi+a.$$
$$y=x' \text{ sin. } \psi+y \text{ cos. } \psi+b.$$

In this case $\psi=+\ 31''$; (in passing eastward from Pomfret to Weston the axes swing around to the *left*, and the angle of change is *positive*, according to trigonometrical usage); $a=3510.983$, and $b=15218.328$. Substituting values, the equations become:

$$x=x'-y \text{ sin. } 31''-3510.983$$
$$y=x' \text{ sin. } 31''+y'+15218.338.$$

Equations for passing from Weston to Pomfret.—In this case, $\psi=-31''$ (reckoned to the right), $a=3513.27$ and $b=15217.81$; substituting values:

$$x=x'+y' \text{ sin. } 31''+3513.27$$
$$y=-x' \text{ sin. } 31''+y'-15217.81.$$

Double pairs of co-ordinates are given along the town line, and either pair may be computed from the other by using these equations. The accuracy both of the equations and the computations are verified by reversing the method.

Verification of the Survey.—At the point C, where three trigonometrical stations can be seen, azimuths were taken to each and the co-ordinates of C computed by the "three point problem." They are 859.36 W, and 7073.59 N, from the origin of Weston. The azimuth from C, to the south-east corner of the tract is 138° 41′, and the distance 63 feet, giving the co-ordinates of C, 859.41 W., 7073.51 N. The degree of accuracy here indicated would probably be sufficient under ordinary circumstances.

Plate II.—In running railway surveys, every opportunity should be taken to connect with the trigonometrical stations which become accessible near the line, so as to verify its direction and the position of the stakes or stations. The methods of doing this by triangulation and otherwise are simple, and will readily suggest themselves to the engineer. This plate illustrates the passage of a railroad survey across a town line, passing from one system of ordinates to another.

The ordinates of the origin in Dexter referred to Elliot, are:

28243.13 E. 31497.21 N.

and the convergence is 4′ 10″.

By the equations given for Case First we find the ordinates of Elliot referred to Dexter to be

$$(\text{cos. } 4'\ 10''=1, \text{ log. sin. } 4'\ 10''=7.0819376.)$$

$$a=28243.13+31497.21 \text{ sin. } 4'\ 10''=28281.134$$

$$b=31497.21-28243.13 \text{ sin. } 4'\ 10''=31463.103$$

or reversing directions, 28281.134 W., and 31463.045 S.

Equations for passing from Dexter to Elliot.—The general formulæ are

$$x=x' \text{ cos. } \psi-y \text{ sin. } \psi+a.$$
$$y=x' \text{ sin. } \psi+y' \text{ cos. } \psi+b.$$

In this case $\psi=4'\ 10''$, $a=-28281.134$ and $b=-31463.045$; and the equations become:

$$x=x'-y'(-\text{ sin. } 4'\ 10'')-28281.134$$
$$\text{or } x=x'+y' \text{ sin. } 4'\ 10''-28281.134,$$
$$\text{and } y=-x' \text{ sin. } 4'\ 10''+y'-31463.045.$$

Equations for passing from Elliot to Dexter.—Here $\psi=4'\ 10''$, $a=28243.13$, $\psi=31497.21$, and the equations become:

$$x=x'-y \text{ sin. } 4'\ 10''+28243.13$$
$$y=x' \text{ sin. } 4'\ 10''+y+31497.21.$$

THE "FUTURE" OF STEEL.*

From "Iron."

UNTIL not so very long ago all construction, whether of houses, bridges or ships, depended in point of fact upon the use of stone, brick, or timber, metal of any kind was but little used, except, perhaps, lead, which was to some small extent utilised for roofing, or if they took it for sheathing. In 1758 copper was in use for sheathing, and afterwards was applied in that way throughout the British Navy, but iron was not, either in the way

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* A Lecture before the Royal Institution.

of construction or applied to house building, except merely so far as door fastenings and so on, although for more than 500 years it had been used for the mansions of the dead in Sussex, where, to use an eloquent but inaccurate expression, "cast-iron tombstones" were to be found. In 1779 there was the great Coalbrookdale Bridge, and in 1819 Southwark Bridge, which still adorned London. Wrought iron was first used for boats in Staffordshire, and in 1821 a boat was built at Horsley Ironworks, and steamed over to France—this was another instance that substantive inventions were very often made by persons and in localities where they would not naturally be looked for, showing that they were the product of powerful minds unfettered by the traditions of any industry. But by 1830, iron steam-ship building had gone to its appropriate position in a large seaport. In 1830 Messrs. Laird had built their first barge, and from that time there followed a number of vessels—parents of the large fleet which has been built at these works—thus from the beginnings at the Horsley Ironworks and Birkenhead sprung the enormous fleet of wrought-iron ships which are now to be found throughout the world, and, whatever the ownership may be, are generally the product of British naval architectural industry.

But while cast iron and wrought iron were thus used in structures of one kind or another, steel was not so applied: it was a luxury, and cost a shilling a pound or more. It was used for swords, needles, knives, and so on, and for the purposes to which it was put people did not mind paying for it, because they could afford to pay for a certainty—in fact it became a proverb, "As true as steel." He had already said that it would be necessary for him to wander away from the title of his lecture, and now he had to ask his hearers to bear with him while he said something of that metal which made up more than 99 per cent. of the steel which was used—the metal called iron.

Iron, he might say, in its compounds, existed in three forms: cast iron, wrought iron and steel. Cast iron was divisible into three classes, ordinary, chilled, and hardened qualities; there were the ordinary and hardened wrought irons, and steel could be made of such fineness as to have the brightness of glass. Of cast irons, pig-iron was made by a mixture of the ore of iron with some kind of flux, and placing the whole in a blast-furnace—there were blast-furnaces in the Cleveland district which had attained the enormous dimensions of 100 feet in height by 30 feet in diameter, at the part above the boshes, and contained over 40,000 cubic feet of space. Through this enormous tower of material a powerful blowing engine sent a blast which, in the best furnaces and with the aid of the hot-blast stove, reached the temperature of 1250° Fah. Having described the process of smelting inside the furnace, and the great heat attainable by the aid of Dr. Siemens' regenerative appliances, the lecturer showed how the ore became deoxidised, and, by the aid of carbon, ran down to the hearth, when tapping ensued, and the hot metal ran into the pig-bed and became pig-iron. He then exhibited a sample of what is technically known as "sponge of iron." Pig-irons were arranged from number one to number eight, number one containing the largest amount of carbon, the quantity decreasing till the highest number was reached, this being white iron used for malleable purposes. Carbon, in the lower qualities of the iron, was very perceptible, and became less so in the higher numbers, but chemists would tell them that if they wanted total combination they must go to chilled iron. He exhibited a Palliser chilled shot split in two, which showed the extent to which the chilling process had penetrated, and also a bar of iron chilled or hardened at one end and not at the other; the explanation of the process was that the molten metal was suddenly congealed in the metallic moulds, where it was formed, the process taking place so suddenly that the carbon had not time to separate. Malleable iron was made in boxes which could be kept heated for several hours, and where decarbonisation took place producing a metal which was not so brittle as the ordinary cast iron. Some of them could remember that in the exhibition of 1862 there was exhibited a pair of Norwich gates, shown by Messrs. Barnard, Bishop and Company as a sample of art work, and afterwards presented to the Prince of Wales and are now at Sandringham. This iron could be made with the same ease as other castings; it was rendered malleable, and thus they began with the cheapness of the casting process and ended with the gracefulness of form. With regard to wrought iron, this was made, as they all knew, by taking pig-iron of the higher numbers, 5, 6, 7 or 8, and putting it into a puddling furnace and then stirring about with a rabble or rake. The sides of the furnace were lined with oxide of iron which, acting on the carbon of the pig, formed carbonic oxide of iron, which rose on the top of the mass and looked like little blue flames. This went on till the iron was decarbonised, and then, being made into balls of about one cwt. each, they were taken to the shingling hammer, which was supposed to expel the oxide from between the particles before the mass was rolled into puddled bar. He was afraid the workmen could not stand such an employment for many years, and attempts had been made for a long time past to get rid of that great labor and to apply mechanical motion to the working of the rabble. These efforts had succeeded and the process was in operation, but not largely. But other attempts had been projected, which appeared to be very successful, to change the form of the furnace altogether, and by making that part circular where the iron lay, and so as to operate like a barrel churn, dispense with either mechanical or hand rabbling. Dr. Siemens' rotary puddling furnace was an instance of the latter method, and after describing it the lecturer said the combination after puddling represented carbon, phosphorus, sulphur, manganese and silicon. This wrought iron, with its ½ to 2-10ths of one per cent. of carbon, was what was relied on for kedges, eighty-ton guns, and the like, for years, and very properly so when a better could not be got. It possessed toughness, and, to a certain extent, cheapness, and could be worked from almost any temperature, and would weld, and was altogether a very desirable material. But he had just a word to say as to welding—unless it was of the very best, and they did not know whether it was or not, it was, after all, a very treacherous thing. He had some old wrought iron rails on the table, and in outward appearance they had a dangerous proximity to Landore steel, but they were broken in pieces. The man who made them undoubtedly thought they were welded properly, but, as a matter of fact, they were merely bundles of fagots. He had also a piece of boiler-plate from a first-rate maker's that had been placed in a boiler which went to work, then a blister appeared because that particnlar plate had not been properly welded, and this caused the boiler to be unsafe. These were some of the difficulties which had to be met with in dealing with wrought iron.

When steel was first made it was manufactured in a sort of petty way. One of the diagrams showed the process by cementation which made blistered steel, and until 1750 a welding process produced an objectionable material called shear steel. After this, Huntsman went to work, and finding the material was fusible, the metal was fused in pots or crucibles holding half cwt. each, and then poured into pots, and until 1851 this was the steel at 1s. per lb. The quality containing from 1.0 to 1.2 per cent. of carbon was the hardest steel, used for files, etc., and the mildest form, containing from ½ to 6-10ths. In 1851 Krupp startled all the makers by making one ingot of 4,500 lb. weight, for the greatest thing that any Sheffield maker had then done was to put two pots together. But Krupp took a lot of pots and, having drilled his men, simply poured the whole in one mass. But in 1862 there was exhibited a mass of twenty tons weight, and this was found perfectly sound from end to end when cut open.

A French chemist, Chenot, next made an ingenious attempt to obtain steel direct from the ore. After the ore became deoxidised by his process, it was ground into powder and then mixed with carbonic material. The whole powder was put into a machine which pressed it, and then it was fused, the earthy matter left rising to the top. The remainder was then pressed into a mould, and the best steel he (the lecturer) had ever seen was the result. This was a good original idea, but it was not cheap, and nothing came of it in England or, he believed, elsewhere. In 1850 Riepe attempted to make steel in a cheaper way. In making iron the material inside the furnace had to come down to a condition lower than that required to produce steel, so it was argued that the right point might be hit upon during the process of making iron. But Riepe did not succeed in hitting the right moment, and he was sorry to say that as a consequence of these experiments a good deal of harm was done. The product was not successful, and it could be understood how and why, when it was recollected that the steel manufacturer was careful even to half-tenths of carbon being present in his steel. The steel thus produced, however, was thought well of, and it was largely used at the beginning; girders were made with it, and the Board of Trade officials inspected them—and they said they were not so good as wrought iron. So steel got a bad name, which it has not lost even to this day. After Riepe came Bessemer, who practically converted steel from being a luxury into that which will be used eventually instead of wrought iron everywhere. Describing the Bessemer converter and its functions, the lecturer went on to say that steel thus made did not, however, regain the character lost under Riepe's process. The exact effects of every preparation could not be ascertained, and some tests showed that the metal would bear 20, 30 or 40 tons of pressure. It was said at last that the metal could not be depended upon, and it would be a poor satisfaction to some injured passenger if the manufacturer of a rail said the accident should not have happened according to his averages; the passenger would at once reply that he cared nothing for the averages but looked to the minimum. He would quote the following words uttered by Mr. Barnaby in 1875: "I should be very doubtful of a ship built of it unless I could see every plate worked." Now, he thought Mr. Barnaby judged very fairly considering the condition of things at that time, but he did not remember that many able men were then turning their attention to steel manufacture. Among those able men was one of the managers of the Royal Institution, Dr. Siemens, who had probably done more to restore the good character of steel than any other man in England. He has described by the aid of diagrams the principle of the Siemens regenerative furnace. By this process about thirteen charges of ten or twelve tons each could be worked in one of these furnaces per week with a minimum of labor, consisting of shoveling in the material every three hours, while there was the invaluable opportunity of taking samples of the mass, thus reducing its ultimate character to an absolute certainty. Thus, while it might be said that by the Bessemer process it was a difficult thing to make good steel, by the Siemens process, it really required a good deal of trouble to make bad steel. In this way the character of the metal having been made a certainty, and manufacturers having improved their steel, Mr. Barnaby's remarks last year were to the effect that it was a "splendid material, such as shipbuilders might use with confidence. Steel makers," added Mr. Barnaby, "might not have taken his remarks of the previous year kindly, but he would ask them whether there was not a good deal better class of metal now than they had then?" Steel was now used by the Admiralty with confidence. They demanded that a sample eight inches long should before ultimate fracture extend 20 per cent. of its own length, and after heating and quenching in water should bear bending round to a radius of $\frac{3}{4}$ of its own thickness. Of 14,000 samples not one had failed, and it was seen that steel would do more than that. (Here, as an illustration, a steel bar was torn asunder in a small testing machine.)

He thought he had shown them that steel had now recovered its character, and that they could again say, with all confidence, "As true as steel." Let this once be the conviction in the minds of constructors and they would use it. They were prompted to do so, he was glad to say; and owing to the assistance of Sir John Hawkshaw, and the committee of the British Association at Bradford, the Board of Trade has been so plied with arguments, that he believed they were on the point of yielding, so that steel would be recognised at its proper value. Holland had recognised it, and it was a pity that the country which was the home of the metal should be behind in this respect. He believed steel would supersede iron in almost everything; for instance, they conld not go on making bridges with large spans by merely adding to the material in order to add to their strength, for the weight would tell against the structure, and therefore when such were required, the only question was whether they would be allowed to use steel. There was no doubt, he thought, the steel and wrought iron overlapped each other, but steel was really to be the metal of the future, for eventually iron would only be used in two forms—cast iron, for purposes where great weight was required, and wrought iron for the purposes of ornamentation, and then they would have steel. Riepe, by a single moulding process, had enabled them to make spectacular steel, which could be run and moulded with the ease of ordinary castings, and in his (the lecturer's) opinion, the future of steel was that it would occupy every province hitherto occupied by wrought iron, except the welding and forge work of ordinary blacksmiths. Mr. Bramwell, who stated that his preliminary observations had only left him three minutes to devote to the title of his lecture, then concluded his remarks.

PROCESSES AND PRODUCTS OF IRON MANUFACTURE.*

By Dr. C. W. SIEMENS, F. R. S.

From "The Engineer."

Having thus dwelt—too long I fear, for your patience—upon the subject of fuel, I now approach the question as to the processes by which we can best accomplish our purpose of converting the crude iron ore into such materials as leave our smelting works and forges. The subject of blast furnace economy has already been so fully discussed by you, during the term of office of your past President, Mr. I. Lowthian Bell, M.P., F.R.S., who has done so much himself to throw light upon the complicated chemical reactions which occur in the blast furnace, that I may be permitted, on the present occasion, to pass over this question, and to call your attention more particularly to those processes by which iron is made to attain its highest qualities, both as regards power of resistance and ductility. Iron and steel were known to the ancients, and are referred to in their works, but we have no account of the processes employed in their manufacture until, comparatively speaking, recent times. Aristotle describes steel as purified iron, and says that it is obtained by re-melting iron several times, and treating it with various fluxes. We are hence led to suppose that in Aristotle's time steel was made by careful selection and treatment of steely iron, which latter was produced by something analogous to the Catalan process. A method referred to by ancient authors is to bury iron in damp ground for some time, and then to heat and hammer it. Another process, described first in Biringuccio's "Pyrotechnology," one of the earliest works on metallurgy, and later in Agricola's "De Re Metallica," both published in the sixteenth century, is to retain malleable iron for some hours in a bath of fused cast iron, when it becomes converted into steel. Reaumur, in 1722, produced steel by melting three parts of cast iron with one part of wrought iron (probably in a small crucible) in a common forge, but he failed to produce steel in this manner upon a working scale. A similar method of producing steel to that proposed by Reaumur, has been employed in India for ages, the celebrated Wootz steel being the result of partial or entire fusion of steely iron and carbonaceous matter, in small crucibles arranged in a primitive air furnace, followed by a lengthy exposure of the ingots to heated air in order to effect a partial decarburisation. In 1750, Hasenfratz refers in his "Siderotechnic," to three processes for producing steel—melting broken fragments of steel with suitable fluxes, fusing malleable iron with carbonaceous matter, and so treating cast iron, probably with oxides, as to obtain cast steel directly from it. The credit of producing cast steel upon a working scale is

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*Abstract of Inaugural address of Dr. Siemens before the Iron and Steel Institute.

due to Huntsman, who was the first to accomplish its entire fusion in crucibles, placed amongst the coke of an air furnace, pouring the fluid metal produced into metallic moulds. This process is still carried on largely at Sheffield for the production of steels of special qualities, such as tool steel, tire steel, castings and forgings, and a ton of cast steel in ingot is produced with the expediture of from 2½ to 3 tons of Durham coke, according to the degree of mildness of the metal produced. At Pittsburgh, where pot-melting is employed on a considerable scale, plumbago pots are invariably used of nearly double the capacity of the clay pots used at Sheffield ; eighteen or twenty-four of these pots, each containing about a hundred weight of metal, are placed in a furnace, each pot lasting twenty-four hours, and yielding five charges during that interval. The fuel consumed amounts to one ton of small slag per ton of steel melted, and is delivered to the works at the surprisingly low price of thirty cents per ton. With these important advantages in his favor, the American steel melter should be able, one would think, to meet without protection his Sheffield competitor in the open market. As regards Bessemer steel, great advances have been made in recent times in cheapening production. At Creusot, and other Continental works, a system of direct working, or of transferring the pig metal in the molten condition from the blast furnace to the Bessemer converter has been introduced, and the same method has been recently adopted at several of the leading English works. By this method of working the fuel usually employed in remelting the pig metal in the cupola—say 2½ cwt. per ton—is clearly saved ; and other advantages are realized, but on the other hand, the Bessemer converter is made dependent upon the working of the blast furnace both as regards time and the quality of the resulting metal. At Barrow, and other large works, where a number of blast furnaces supply a number of Bessemer converters, and pig metal for the open market in addition, this mode of working appears to be practically free from the objection above stated, and a hot ladle, with its engine, may be kept steadily at work transferring the pig metal from one blast furnace or another, to the converters. But it still remains to be seen whether any practical advantage can be realized by this method of working at smaller works, where a change in the working of the blast furnace from Bessemer to forge pigs, would cause a serious interruption in the working of Bessemer plant.

In America the effort of the iron-master has been directed—chiefly under the guidance of Mr. A. L. Holley—towards a saving of labor, by increasing to an almost incredible extent the number of blows per diem from each converter. Thus I was informed that at the North Chicago Steel Works as many as seventy-three blows had been obtained in one pit in twenty-four hours, although I have reason to doubt whether this rate of working could be maintained for any length of time. The Americans have not adopted, so far as I could ascertain, the direct process of working, but are content to remelt their pig metal in large cupolas in immediate proximity to the converters; the capacity of the converters has latterly been much increased, and the degree of heat engendered by a blast of increased power, has been augmented to such an extent that a considerable amount of scrap metal can be remelted within the fluid bath before discharging the same into the ingot moulds. Whilst the Bessemer process has been making rapid strides, a rival process has gradually grown up by its side, which I cannot pass over without remark. I allude to the open hearth steel process, with which my name and the joint names of Siemens and Martin are associated. The conception of this process is really as old as that of cast steel itself. The ancient Indian steel, the Wootz, was the result of a fusion of a mixture of malleable and cast iron. Reaumur, as already stated, proposed to melt wrought iron and pig metal together, for the production of steel, as early as 1722; and G. B. Heath—to whom we owe the important discovery that by the addition of manganese to cast steel its malleability is greatly increased—endeavoured to realize the conception of producing steel in large masses upon the open hearth of the furnace in the year 1839, and he again has been followed in these endeavors by Gentle Brown, Richards, and others in the same direction. When, in

1856, I first seriously gave my attention, in conjunction with my brother, Frederick Siemens, to the construction of a regenerative gas furnace, I perceived that this furnace would be admirably adapted to the production of steel upon the open hearth, and I remember proposing it for such a purpose to Mr. Abraham Darby, of Ebbw Vale, in 1861. Ever since that time I have been engaged in the realization of this idea, which has been retarded, however, by those untoward circumstances which ever intervene between a mere conception and its practical realization. Although two of my earlier licencees, Mr. Chas. Attwood, of Tow Law, and the Fourchambault Company, in France, with whom was my late esteemed friend, Mons. Lechatelier, Inspecteur-General des Mines, succeeded, in 1865 and 1866, in producing steel upon the upon hearth, they did not persevere sufficiently to attain commercial results; and it was not until after I had established experimental steel works at Birmingham that I was enabled to combat in detail the various difficulties, which at one time looked well-nigh insuperable. Whilst thus engaged, Messrs. Pierre and Emile Martin, of Cereuil—who had obtained licenses for furnaces to melt steel both in pots and on the open hearth—succeeded, after a short period of experimenting, in introducing into the market open-hearth steel of excellent quality.

Whilst Messrs. Martin thus gave their attention to the production of steel by the dissolution of wrought iron and steel scrap in a bath of pig metal, my own efforts were more especially directed to the production of steel by the use of pig metal and iron ores, either in the raw state or in a more or less reduced condition, which latter process is the one mostly employed in this country. One of the advantages that may be claimed for the open-hearth system consists in its not being dependent upon a limited time for its results. The heat of the furnace is such that the fluid bath of metal, after being reduced to the lowest point of carbonisation, may be maintained in that condition for any reasonable length of time, during which samples may be taken and tested, and such additions, either of pig metal, of wrought scrap, spongy metal, or ore, may be made to it as to adjust it to the desired quality. Spiegeleisen, or ferro-manganese, is then added in the solid condition, in the requisite proportion, and the result is a bath of metal, the precise chemical condition of which is known, and which has the advantage, if properly managed, of being what is technically called "dead melted," which circumstance renders it applicable for certain purposes for which pot steel has hitherto been mostly employed. The purpose for which the open-hearth process is more especially applicable has reference to the conversion of scrap steel, and iron of every description into steel or ingot metal, and it is now used, indeed, to a large extent, for the conversion into steel of old iron rails. The wearing qualities of these converted rails have been under test since 1867, when the Great Western Railway Company had some old Dowlais iron rails converted into steel at my experimental steel works at Birmingham, which was rolled into rails by Sir John Brown and Co., and which have been down ever since that time at Paddington, subjected to great wear and tear. The manufacture of steel, both by the Bessemer and the open-hearth process, is much facilitated by the use of ferro-manganese. This material was introduced into the market in 1868, by Mr. Henderson, of Glasgow. It was produced successfully by charging carbonate or oxide of manganese and manganiferous iron ore intimately mixed with carbonaceous matter upon the open-hearth of a Siemens furnace with a carbonaceous lining; but the demand for this material was not sufficient to render the manufacture profitable at that time, and it was not until the year 1875 that it was re-introduced into the market by the Terrenoire Company. Manganese, when added in a proportion of .5 per cent., or more, to steel or ingot metal containing only from .15 to .20 per cent. of carbon, has the effect of removing red-shortness, and of making it extremely malleable both in the heated and cold conditions.

In using spiegeleisen containing only from 10 to 15 per cent. of metallic manganese, it is impossible to supply the amount necessary to produce this malleability without adding, at the same time, such a percentage of carbon as would produce a hard metal. The use of ferro-manganese enables us to overcome this difficulty, and greatly facilitates the production of a metal so malleable and with so little carbon, as to remain practically unaffected in its temper when plunged red-hot into water. Another result produced by the use of manganese without carbon, upon mild steel or ingot metal, is to neutralize the objectionable effect of phosphorus, so long as the latter does not exceed the limit of .25 per cent. This metal, in which phosphorus may be said to take the place of carbon, presents a large specular fracture, and is, contrary to what might have been expected, extremely ductile when cold. Iron when in the fluid condition can be alloyed with other metals, and some of the compounds thus formed are known to possess very remarkable properties. Thus, iron combined with 3 per cent. of tungsten and .8 per cent. of carbon, yields a metal which can be worked like ordinary steel, but which, when hardened, retains magnetism to a very remarkable degree. A further addition of tungsten produces an exceedingly hard metal (introduced into the market by Mr. Mushet) which cannot be forged, but which when cast into bars, and ground so as to form a shap edge, produces cutting tools capable of great endurance. An admixture of chromium has for many years past been known to produce steel of great hardness and strength, but it is only quite recently that it has been brought into practical use in America by Mr. Julius Baur, and has been taken up in this country by Sir John Brown, and Co., of Sheffield, who claim for it very remarkable properties as regards strength, malleability, and fredom from corrosion. The formation of compounds such as these is a matter of great interest in connection with the future development of the applications of steel, and is one of those subjects which I venture to suggest might be much advanced by an organized research, under the auspices of a committee of the Iron and Steel Institute. The value of the material known as mild steel or ingot metal consists in its extreme ductility under all possible conditions. Its ultimate strength is much inferior to that of ordinary steel, and rarely exceeds 28 tons per square inch; its limit of elasticity is reached at 15 tons per square inch, whilst the limit of elasticity of a harder steel may reach from 25 to 30 tons per square inch, and that of hard drawn steel wire from 45 to 50 tons.

But in estimating the relative value of these different materials by the amount of work that has to be expended in causing rupture, it will be found that the mild steel has the advantage over its competitors. When subjected to blows or sudden strains, such as are produced by the explosion of gun-cotton or dynamite, extra mild steel differs in its behaviour from that of BB iron and ordinary steel, by yielding to an extraordinary extent without fracturing, and it is in consequence of this non-liability to rupture that it may be loaded to a point much nearer to its limit of elasticity than would be safe with any other material. Attention has been recently directed in various quarters to remedy a defect appertaining to steel, that of piping and showing honeycombed appearance in the ingot. It is well known that if such steel is hammered and rolled, the open spaces contained in it are elongated, and seemingly closed up, but in reality continue to form severances within the metallic mass, to the prejudice of the uniform strength of the finished forging. In casting steel containing more than .5 per cent. of carbon, the defect of honeycombing can easily be avoided if care is taken to have the metal "dead melted" before casting it into the mould; and that of piping in continuing the inflow of fluid metal for a sufficient length of time while it is setting. But in dealing with mild steel containing only say .2 per cent. of carbon, the difficulty of making a sound casting is greatly increased. Much may be done, however, by careful manipulation of the fluid metal, and by the judicious addition to it of manganese or other oxidisable metal, such as silicon or lead, by which occluded oxygen is removed. Sir Joseph Whitworth, who, as you well know, has given much attention to this subject, has overcome the evil mechanically by subjecting the steel, while setting in the mould, to great hydraulic compression. He has thus succeeded in producing, in large masses, mild steel of extremely uniform strength, and the only doubt which could possibly be raised against the advisability of producing fluid steel for ordinary applications by this method is on the question of expense. The subject of producing sound steel castings is one which we shall have an opportunity to discuss in reference to a paper which will be presented by M. Gautier.

The employment of steel for general engineering purposes dates only from the year 1851, when Krupp, of Essen, astonished the world by his exhibits of a steel ingot weighing 2,500 lbs., and of his first steel gun, and introduced a comparatively mild description of pot steel for steel tires, axles, and crank shafts. For the production of these he constructed his celebrated monster hammer, with a falling weight of 45 tons, which at that time far surpassed in magnitude and power our boldest conception, and is now only being exceeded by a still more powerful hammer in course of erection at the Essen Works. Krupp's steel was, however, not cheap steel, and it is to our past President, Mr. Henry Bessemer, that we are indebted for the production of steel at such a reduced cost as to make it available for railway bars and structural purposes, in substitution for iron, since which event the applications of this superior material show a most extraordinary rate of increase. Not only do we travel upon steel tires, running over steel rails, but at least one of our leading railway companies, the London and North Western, has, under the able management of Mr. F. W. Webb, constructed as many as 748 locomotive engines, including boiler, frame, and working parts, entirely of that material, excepting only the fire-boxes, which are still made of copper. In France, also, much attention has been given to the introduction of steel for machinery purposes, and there, as well as in the United States, Germany, and Holland, that material is used largely in the construction of bridges and other engineering works. In this country, the application of steel for structural purposes has occupied the attention of some of our leading civil engineers for many years, and Sir John Hawkshaw, when called upon to construct a railway bridge at Hungerford, in 1859, proposed the use of steel in order to lighten the structure. He was prevented, however, from carrying his idea into effect by the rules of the Board of Trade, which provide that any kind of wrought material shall not be weighted either in compression or extension to more than five tons per square inch. Repeated efforts have been made since that time to induce the Board of Trade to adopt a new rule, in which the superior strength of steel should be recognized, and in order to facilitate their action a committee was formed, consisting of Mr. William Henry Barlow, Captain Galton, and others, who carried out—with the pecuniary aid of leading steel manufacturers—a series of valuable experiments, showing the limit of elasticity and ultimate strength of various steels, which results are published separately in the "Experiments on the Mechanical and other Properties of Steel, by a Committee of Civil Engineers." At the instance of Mr. Barlow, the British Association appointed a further committee to promote the object of obtaining for steel its proper recognition, and this has led finally to the appointment under the sanction of the Board of Trade of three gentlemen, viz., Sir John Hawkshaw, F.R.S., and Mr. William Henry Barlow, F.R.S., who were nominated by the Council of the Institution of Civil Engineers, and of Colonel Yolland, F.R.S., of the Board of Trade. These gentlemen have agreed upon a report recommending the use of steel as a building material, subject to a limit of strength greatly in excess of the limit assigned to wrought iron, and it is to be hoped that the Board of Trade, by adopting that report, will remove the serious drawback which has too long stood in the way of the application of steel for structural purposes, and which has rendered the construction of large works, such as the projected bridge over the Firth of Forth, practically impossible. As regards the construction of ships of extra mild steel, the English Admiralty, following the example set by France, has, under the advice of Mr. Barnaby, the Chief Constructor, taken the lead of the commercial navy of the country, and several corvettes have recently been constructed entirely of that material, at the Government yard at Pembroke, and upon the Clyde.

The constructors of merchant shipping have been hitherto restricted by rules laid down by Lloyd's Registry, which makes no distinction between common iron and steel in determining the classification of a vessel. It is to be hoped that the important engineering and shipbuilding interests of the country will soon be released from regulations which may have been well adapted to the use of an inferior material such as common iron, but fail entirely to meet the requirements of the present day. In ship-building, the use of a material superior in toughness and in strength produces the double advantage of greater safety to life and property, and of an increase of carrying capacity to the full amount of weight saved in the construction of the ship. It should be borne in mind that this additional weight of merchandise is carried without increasing the working expenses and power required to propel the ship, and may just suffice to strike the balance between working a vessel designed for long voyages at a fair profit or a loss.

In constructing the masts and yards of vessels of the stronger material, the weight saved is a matter of still greater importance, which I am glad to say now engages earnest attention. In the United States, a committee, composed of both military and civil engineers, have been engaged for some time upon the subject of determining experimentally the structural value of iron and steel, with the advantage of substantial support from the United States Government, who, after a previous grant of 75,000 dollars, have, I observe, granted a further sum of 40,000 dollars in aid of the experimental inquiries instituted by the committee. The council of the Iron and Steel Institute are not unmindful of the importance of this subject, and have invited those gentlemen of this and other countries, who have given most attention to the production and application of steel, to aid us in our forthcoming discussion with the results of their experience. In the course of this discussion the distinctive limits between steel and iron will necessarily engage your attention. Considering the extraordinary change of physical condition which iron undergoes when alloyed with small percentages of carbon, manganese, phosphorus, tungsten, chromium, and other substances, and considering, further, that it is never quite free from some admixture, the question of nomenclature is one naturally surrounded with difficulty, but it is becoming one of considerable practical importance when rules are to be laid down regulating the permissible strength of different grades of these materials. Dr. Percy has, in his "Metallurgy of Iron and Steel," defined steel as iron containing a small percentage of carbon, the alloy having the property of taking a temper; and this definition is substantially equivalent to those found in the works of Karsten, Wedding, Gruner, and Tunner. On the other hand, Messrs. Jordan, Greiner, Gautier, Phillipart, Holley, and others define as steel all alloys of iron which have been cast in malleable masses, whilst Sir Joseph Whitworth considers that steel should be defined mechanically by a coefficient representing the sum of its strength and ductility. With the object of settling this question of nomenclature, an International Committee was appointed at Philadelphia, by the Institution of American Mining Engineers. The committee consisted of the following gentlemen:—Mr. I. Lowthian Bell, M.P.; Dr. Hermann Wedding; Professor Tunner; Professor Akermann; M. Gruner; and Messrs. A. L. Holley and T. Egleston, and they resolved that the following should be recommended:

(1) That all malleable compounds of iron, with its ordinary ingredients, which are aggregated from pasty masses, or from piles, or from any form of iron not in a fluid state, and which will not sensibly harden and temper, and which generally resemble what is called wrought iron, shall be called weld iron (German, *Schweisseisen;* French, *fer soude*). (2) That such compounds when they will from any cause harden and temper, and which resemble what is now called "puddled steel," shall be called weld steel (German, *Schweiss-stahl;* French, *acier soude*). (3) That all compounds of iron, with its ordinary ingredients, which have been cast from a fluid state into malleable masses, and which will not sensibly harden by being quenched in water while at a red heat, shall be called ingot iron (German, *Flusseisen;* French, *fer fondu*). (4) That all such compounds, when they shall from any cause so harden, shall be called ingot steel (German, *Fluss stahl;* French, *acier fondu*)." The nomenclature here proposed is entitled to careful consideration from the eminence for both theoretical

and practical knowledge of the gentlemen composing the committee; but I apprehend that for common use the distinctions desired to be drawn are too manifold. Moreover, the lines of demarkation laid down run through materials very similar, if not identical, in their application, where a distinction in name would be extremely difficult to maintain and awkward to draw. Take, for instance, railway bars from ingot metal, which are usually specified to bear a given dead load without deflecting beyond certain limits, and to resist certain impact without rupture. The materials answering to these requirements contain from .2 to .6 per cent. of carbon, depending in a great measure upon the mode of production, and upon the amount of admixture of phosphorus, sulphur, silicon, and manganese. But inasmuch as the quality of tempering depends chiefly upon carbon, part of the rails delivered under such specification might have been classified as ingot iron, and part as ingot steel. The committee omits to define the degree of hardening which it considers necessary to bring a material within the denomination of ingot steel. It is well known, however, that the temper depends upon the exact temperature to which the metal is heated before being plunged into the refrigerating medium, and also upon the temperature and conductivity of the latter, and that ingot metal with even .2 per cent. of carbon, when plunged hot into cold water, takes a certain amount of temper. The question of the amount of import duties payable in foreign countries upon metal occupying a position near the proposed boundary line, would also lead to considerable inconvenience. Difficulties such as these have hitherto prevented the adoption of any of the proposed nomenclatures, and have decided engineers and manufacturers in the meantime, to include, under the general denominations of cast steel, all compounds consisting chiefly of iron, which have been produced through fusion, and are malleable. Such a general definition does not exclude from the denomination of steel, materials that may not have been produced by fusion, and which may be capable of tempering, such as shear steel, blister steel, and puddled steel, nor does it interfere with distinctions between cast steels produced by different methods, such as pot steel, Bessemer steel, or steel by fusion on the open hearth. The forthcoming discussion will, I hope, lead to some general agreement regarding this question of nomenclature.

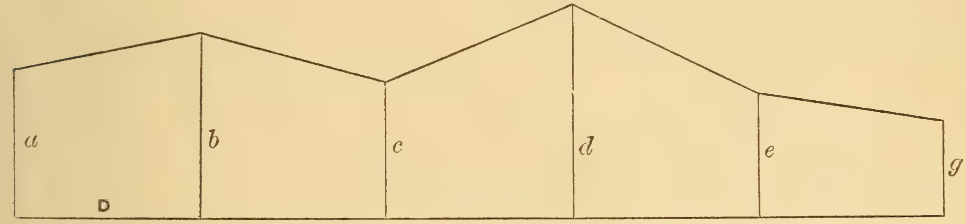
While steel is gradually supplanting wrought iron in many of its applications, efforts are being made to maintain for the latter material an independent position, for cheapness and facility of manipulation, by improving the puddling process. Mechanical puddling, like many other important inventions, has taken a long time for its development, and has engaged the attention of many minds, but I will only here mention the names of Tooth, Yates, and Mr. Menelaus, our past president, who have pioneered the road; and of Danks, Spencer, Crampton, and others who have followed more recently in the same direction. It is chiefly owing, however, to the persevering endeavors of Mr. Heath, and of Messrs. Hopkins, Gilkes, and Co., that the mechanical puddling of pig metal has been accomplished with a considerable amount of success. All these efforts have had reference to puddling in a chamber rotating upon a horizontal axis, but numerous attempts have also been made to accomplish mechanical puddling by the introduction into stationary chambers of rabbles moved by mechanical power, and by the use of chambers rotating upon an inclined axis, in connection with which latter the names of Maudslay, Sir John Alleyne, and Pernot should be mentioned. The principal difficulty connected with the rotary puddling furnace consisted in providing a lining of sufficient power to resist the corrosive action produced by siliceous slags, and it is important, therefore, that the pig metal introduced into the rotative puddler should be as free from silica as possible. By charging fluid metal into the furnace, the silica adhering to the pigs in the form of sand is got rid of; but efforts have latterly been made, with satisfactory results, I believe, to subject the pig iron itself to a simple finery process on its way from the blast furnace to the rotative puddler, with a view of removing the silicon chemically combined with the pig. M. Hamoir, of Belgium, has been engaged upon this subject for some years, as you will have seen from the "Report on the Progress of the Iron and Steel Industries in Foreign Countries" in our "Journal;" while, in this country, Mr. I. Lowthian Bell has called the Bessemer converter into requisition for effecting the desired object. We are informed that not only does the lining of the furnace stand better in using this semi-refined metal, but that the yield per furnace per diem, as well as the quality of the metal obtained, are much improved. It is intended to roll the metal thus produced into railway bars, without any intermediate process of re-heating, and to subject the rails to a process of case-hardening similar to what was practised some years ago by Mr. Dodds, in South Wales. The case-hardened iron rails are expected to rival steel rail in quality, but it remains to be seen whether these wearing properties are not obtained at the cost of brittleness, and whether rails manufactured by this method can compete in price with steel rails. Three years ago, I had the honor of bringing before this Institute a plan of producing wrought iron directly from the ore, in a rotative furnace of special construction, and heated by gas. This process was at that time only carried on upon a small scale at my sample steel works, in Birmingham. It has since been carried out upon a working scale, at Towcester, and in Canada, and although the results hitherto obtained cannot yet be considered entirely satisfactory from a commercial point of view, I see no reason to feel discouraged as regards the ultimate result of this method of treating iron ores. By it, iron of almost entire freedom from sulphur and phosphorus is obtained from ores containing a considerable percentage of these impurities. If steel is to be produced, the raw balls, as they leave the rotary furnace, are either immediately transferred to the bath of the open-hearth furnace, or are previously subjected to the processes of squeezing and hammering for the removal of scoria, which otherwise carries some of the impurities contained in the ore into the metallic bath, and prevents the attainment of steel of a high quality. One of the drawbacks to the use of iron and steel for structural purposes is found in their liability to rust when exposed to air and moisture. The ordinary means of protection against rust consists in covering the exposed surfaces with paint, and if this is renewed from time to time, iron and steel may be indefinitely preserved from corrosive action. Another mode of protection consists in dipping articles of iron and steel while hot into a bath of oil, when some of the oil penetrates to a slight depth into the pores of the metal, while other portions become decomposed, and form a very tenacious resinous coating. For the protection of iron and steel when in the form of thin sheets or wire, galvanising, as is well known, is largely resorted to. The principle of protection in this case depends upon the fact that zinc, although more oxidisable than iron, forms, with oxygen, an oxide of a very permanent nature which continues to adhere closely to the metal, and thus prevents further access of oxygen to the same. This mode of protection presents the further advantage that so long as any metallic zinc remains in contact with the iron in presence of moisture, the latter metal forms with the zinc the negative element of an electrolytic couple, and is thus rendered incapable of combining with oxygen. Galvanising is not applicable in those cases in which structures of iron and steel are put together by the aid of heat, or are brought into contact with sea-water, which would soon dissolve the protecting zinc covering. But even in these cases the metal may be effectually protected against corrosion by attaching to it pieces of zinc, which latter are found to dissolve in lieu of the iron, and must, therefore, be renewed from time to time. Captain Ainslie, of the Admiralty, has lately made a series of valuable experiments, showing the relative tendency towards corrosion of both iron and steel when in contact with sea-water, and of the efficacy of pieces of zinc in preventing this corrosion. These experiments further show that mild steel is—contrary to the results obtained by M. Gautier—more liable to corrosion than wrought iron in its unprotected condition, but that zinc acts most efficaciously in protecting it. Quite recently, another mode of protecting iron and steel plates from corrosion has been suggested by Professor Barff. This consists in exposing the metallic surfaces while heated to redness, to the action of superheated steam, thus producing upon their surface the magnetic oxide of iron, which, unlike common rust, possesses the characteristic of permanency, and adheres closely to the metallic surface below. In this respect it is analogous to zinc oxide adhering to and protecting metallic zinc, with this further advantage in its favor, that the magnetic oxide is practically insoluble in sea water and other weak saline solutions.

CENTER OF GRAVITY OF EARTHWORK.

By J. WOODBRIDGE DAVIS, C. E.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

To the engineer, engaged on the construction of a railroad or canal, perhaps no problem is more constantly presented than that of finding the average haul of a piece of excavation. It is shown in all treatises that the average haul is the distance between the center of gravity of the material as found to its center of gravity as deposited. It is, furthermore, shown that to find the center of gravity in either place, we must use the principle of moments, dividing the mass into elementary volumes, multiplying each by its distance from an assumed axis, adding these products and finding what distance multiplied by the whole volume will produce this sum. The accompanying diagram represents a longitudinal section of that part of a cutting whose material has been transported in one direction. We know the volumes, V_1, V_2, etc., between the equi-distant cross sections, *a* and *b*, *b* and *c*, etc.; and, consequently, we know the volume, V, of the series: we wish to find its center of gravity. Now, it is evident that the volumes, V_1, V_2, etc., are too great to be considered elementary volumes for finding the center of gravity of V; for instance, we cannot assume that the center of gravity of V_1 is midway between *a* and *b*; because it may be $\frac{4}{7}$ or $\frac{3}{5}$ of the volume's length, D, from the first end, and the error would be serious. We must, therefore, find the center of gravity of each component volume. There are three ways of accomplishing this. The first is to divide the volume into short portions and assume the center of gravity of each to be midway its length. This requires immense labor, and is only approximate. The true position can be determined by the calculus; but a long formula must be used. The third method, and most common, is to approximate the center of gravity by a mental estimate. The first two of these methods are inconvenient; The first and last are inaccurate.

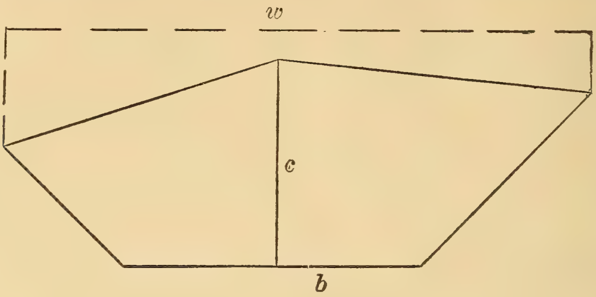


If we *might* assume each volume to be concentrated at a point midway between its ends, we could quickly find the center of gravity of the series, by ascertaining successively the centers of gravity of pairs of these points, disregarding figures in units and decimal places of volumes, till we should finally concentrate the entire weight at one point. If we cannot make this assumption, the labor of finding the center of gravity of each volume, by one of the above methods, must first be undertaken. In considering this problem, the writer has discovered an expedient, by which the foregoing assumption may be immediately made, and the error corrected by an exceedingly simple formula, that remains the same whatever number of volumes compose the series.

First, to explain this, we must have an expression for the cross-sectional area. There is room for much choice here.

Let us consider the most ordinary shape of cross-sections, represented in the diagram, and assume the formula: for its area,

$$\tfrac{1}{2} w\,(c+sb)-sb^2,$$



from the writer's *Formulae for R.R. Earthwork;* the readers of which will recognize the expression, while others can readily deduce it by inspection of the values of the symbols in the diagram, S being the ratio of side-slope, equivalent to *vertical* ÷ *horizontal.* Considering this to be the area at a, at b we may likewise assume

$$\tfrac{1}{2} w'\,(c'+sb)-sb^2.$$

The area of a section at the distance x from a is, by the same formula,

$$\tfrac{1}{2}\left(w+(w'-w)\,\frac{x}{\mathrm{D}}\right)\left(c+(c'-c)\,\frac{x}{\mathrm{D}}+sb\right) -sb^2.$$

This, multiplied by dx, after performing the multiplication indicated, is the differential of the volume,

$$d\mathrm{V}_1=\tfrac{1}{2}\left\{\begin{array}{c} w(c+sb)dx \\ (w'c'-w'c-wc'+wc)\,\dfrac{x^2dx}{\mathrm{D}^2} \\ (wc'-2wc+w'c+sb\,[w'-w])\,\dfrac{xdx}{\mathrm{D}} \end{array}\right\}-sb^2dx.$$

By the principle of moments, using the calculus, x_1 being the distance of the center of gravity of V_1 from a, we have

$$x_1=\frac{\int_0^{\mathrm{D}} xd\,\mathrm{V}_1}{\mathrm{V}_1}=\frac{\int_0^{\mathrm{D}} xd\,\mathrm{V}_4}{\int_0^{\mathrm{D}} d\,\mathrm{V}_1}$$

$$=\frac{[wc+3w'c'+w'c+wc'+2sb(w+2w') -12sb^2]\,\dfrac{\mathrm{D}^2}{24}}{[w'c+wc'+2\,(w'c'+wc)+3\,sb(w+w') -12sb^2]\,\dfrac{\mathrm{D}}{12}}$$

$$=\frac{[w'c+wc'+2(w'c'+wc)+3sb(w+w') -12sb^2]\,\dfrac{\mathrm{D}^2}{24}}{[w'c+wc'+2(w'c'+wc)+3sb(w+w') -12sb^2]\,\dfrac{\mathrm{D}}{12}}$$

$$+\frac{[w'c'-wc+sb(w'-w)]\,\dfrac{\mathrm{D}^2}{24}}{[w'c+wc'+2(w'c'+wc)+3sb(w+w') -12sb^2]\,\dfrac{\mathrm{D}}{12}}$$

$$=\tfrac{1}{2}\,\mathrm{D}+\frac{[(\tfrac{1}{2}w'(c'+sb)-sb^2)-(\tfrac{1}{2}w(c+sb)-sb^2)]\,\dfrac{\mathrm{D}^2}{12}}{\mathrm{V}_1}$$

$$=\tfrac{1}{2}\,\mathrm{D}+\frac{\left(b-a\right)\dfrac{\mathrm{D}^2}{12}}{\mathrm{V}_1}=\tfrac{1}{2}\,\mathrm{D}+n.$$

If the center of gravity of the first volume be assumed at half its length, an error, represented by the distance n, is made. The effect of this error, on the average haul of the whole series, is the same as if the center of gravity of the series were moved a distance m, such that

$$m:n::\mathrm{V}_1:\mathrm{V},\ \text{or}\ m=\frac{\left(b-a\right)\dfrac{\mathrm{D}^2}{12}}{\mathrm{V}}$$

To compensate for this error, then, we must move the center of gravity of the series, as found by the assumption, a distance m, in the direction from a. By a process exactly similar, we find that the assumption that the center of gravity of V_2 is midway its length, may be corrected by moving the center of gravity of the series a distance m_1, such that

$$m_1 = \frac{\left(c-b\right)\frac{D^2}{12}}{V}$$

A similar correction is found for the other volumes, till

$$m_4 = \frac{\left(g-e\right)\frac{D^2}{12}}{V}$$

Adding, we obtain

$$m + m_1 + \text{etc.} + m_4 = \frac{\left(g-a\right)\frac{D^2}{12}}{V} = M.$$

M is the total correction for the average haul, as first inaccurately determined by assuming the center of gravity of each volume to be half way between its ends. We may, therefore, consider each volume to be concentrated in its mid-section; or easily make the well known graphical or analytic solution of the center of gravity problem, and, finally, correct by the formula just derived.

If, instead of the formula for regular sections, we had used the formula for irregular sections,

$$\tfrac{1}{2}v(m+sb)+\tfrac{1}{2}x(p+sb)+\tfrac{1}{2}m'(c-l)$$
$$+\tfrac{1}{2}n'(c-p)+\tfrac{1}{2}p'(n-r)-sb^2,$$

which represents the area of any irregular section (*Formulae for R.R. Earthwork*), v and x being the side-widths, m, n, p, the heights of breaks, m', n', p', their distances from center, we should have reached the same expression. Therefore, transverse irregularity does not alter the correction. In all cases, *subtract the area of first end from area of last, multiply difference by $\frac{1}{12}$ the square of distance between consecutive sections, and divide by total volume.* If the volume be in cubic yards, the numerator must also be divided by 27.

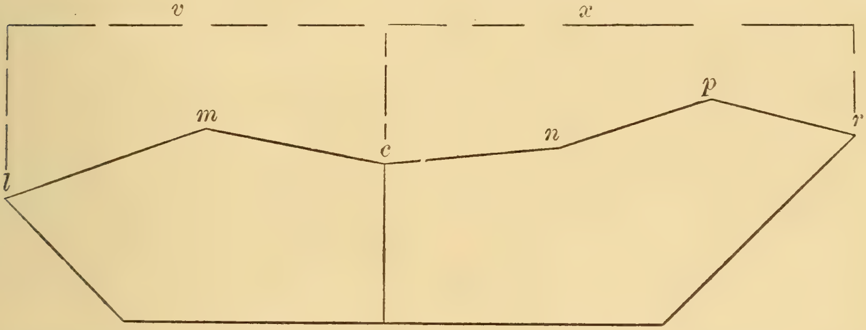
The same method may be applied to borrow-pits laid out between equi-distant cross sections.

Also, conceiving the first diagram to be a series of trapezoidal areas, the same method applies, and the corrected formula is identical, V being now the whole area, a the length of first ordinate and g the length of last.

We see by this correcting formula,

$$\frac{\left(g-a\right)\frac{D^2}{12}}{V},$$

that, if a piece of earthwork, between equi-distant cross sections, have the same end-areas, the center of gravity of each component volume may be freely assumed to be in its mid-section, and the resulting center of gravity of the series is perfectly correct.



It is also to be observed that, whatever be the difference between end areas, the error of the assumption is less as the square of the distance between sections. By the calculus this distance is reduced to dx, an infinitesimal of the second order, which, divided by the finite quantity V, gives zero for the correction. Hence the method of the calculus is perfect.

The centers of gravity of pyramids, prismoids, of all solids of revolution, whose generatrices are lines represented by equations of the first and second degrees, and of all segments of these between parallel planes, can be easily determined by this method. Thus, if h be the height of a cone, r the radius of its base, we obtain for the distance of its center of gravity from the vertex,

$$\tfrac{1}{2}h + \frac{\tfrac{1}{12}h^2(\pi r^2 - 0)}{\tfrac{1}{3}h.\pi r^2} = \tfrac{3}{4}h.$$

The center of gravity of the paraboloid, whose height is h, and the radius of whose base is r, is

$$\tfrac{1}{2}h + \frac{\tfrac{1}{12}h^2.\pi r^2}{\tfrac{1}{2}h.\pi r^2} = \tfrac{2}{3}h.$$

The center of gravity of a hemisphere, whose radius is r, is, measuring from center of sphere, at a distance

$$\tfrac{1}{2}r - \frac{\tfrac{1}{12}r^2.\pi r^2}{\tfrac{2}{3}\pi r^3} + \tfrac{3}{8}r.$$

A NEW INVESTIGATION OF ONE OF THE LAWS OF FRICTION.

By A. S. KIMBALL, Professor of Physics in the Worcester Institute of Industrial Science.

From "The American Journal of Science and Arts."

Reuleaux, in the appendix to his recently published "Cinematics of Machinery," says that "many engineering schemes have failed because they were designed in accordance with the statements given in our text-books as the laws of friction." He furthermore adds, "that it is time that the experiments of Bochet and Hirn should be raised from their place as foot-notes to a position in the text."

During the last year, I have conducted experiments, on as extensive a scale as our laboratory would allow, for the purpose of settling, if possible, certain contested points in the doctrine of friction.

Our manuals of mechanics, following Morin and Coulomb, say the co-efficient of friction *does not vary with the velocity.* Bochet says that it *decreases as the velocity increases.* Hirn says that it *increases as the velocity increases.* Contradictory as these statements are, it is probable that each contains a partial truth. They need to be combined to make a complete statement.

The results of my experiments, which this paper is to describe, would indicate that the following is the true law, within the range of my experience. The coefficient of friction at very low velocities is small; it increases rapidly at first, then more gradually as the velocity increases, until at a certain rate, which depends upon the nature of the surfaces in contact and the intensity of the pressure, a maximum coefficient is reached. As the velocity continues to increase beyond this point, the coefficient decreases. An increase in the intensity of the pressure (the number of pounds on a square inch), changes the position of the maximum coefficient, and makes it correspond to a smaller velocity. The more yielding the materials between which the friction occurs, the higher is the velocity at which the maximum coefficient is found. Heating the rubbing bodies changes the position of the maximum coefficient to a higher velocity, since by heat the bodies are made softer, and are caused to yield to pressure with greater ease. For a considerable range of velocities in the vicinity of the maximum coefficient the coefficient is sensibly constant.

The experiments upon which I base my conclusions may be classified as follows:

(1.) Sliding friction down an inclined plane.

(2.) Sliding friction at uniform velocities on a horizontal plane.

(3.) Friction of belts on the surface of cast iron pulleys.

(4.) Friction of wrought iron journals in boxes or bearings of different materials.

(1.) *Sliding friction down an inclined plane.*—A full description of the apparatus used, and some of the results obtained, will be found in this Journal, March, 1876; also in Van Nostrand's Eclectic Engineering Journal, June, 1876. It is sufficient for my purpose to say that the sliding body was made to carry a smoked glass, upon which was traced a wave line, which by direct measurement gave the time of sliding and the spaces passed over, from which it was easy to compute the corresponding coefficients of friction. In the article referred to, no velocities less than two feet a minute were examined. An extension of the same experiment, to the case of much lower velocities, showed a curve concave toward the time line, indicating that at these velocities the coefficient of friction was increasing. As the velocities increased, the line changed its direction and became convex toward the time line; thus giving in the limits of one experiment a verification of the statements made above. For further particulars respecting this method of experiment, I refer to the article published in March of last year.

(2.) *Sliding friction at uniform velocities on a horizontal plane.*—A heavy pine plank, fifteen feet long, whose surface had been planed, was carefully leveled on the floor of the laboratory. The weight-box was mounted upon shoes which could be covered with the material experimented upon. To its forward end a spring dynamometer was attached, which was pulled by a cord wound around a drum, which was made to revolve at a constant velocity. The motive power was a fifteen horse-power Corliss engine, belonging to our machine shop, whose fly-wheel runs with great regularity at the rate of sixty revolutions per minute. A shaft from the shop runs underground to the cellar beneath my laboratory, whence through several countershafts the power is transmitted to any part of the room. By means of change pulleys, I can easily command a great range of velocities.

The experiments were made by drawing the box along the plane at various velocities, and reading the friction from the dynamometer. This combination answers very well for low velocities, but the slide can not be easily stopped when the speed is great.

Several series of experiments were made, with wood on wood, also with leather on wood. The results verify the first part of my statement, that the coefficient of friction increases with the velocity, when this is small. The following are some of the results obtained by this method of experiment: (*next column*)

TABLE I.—PINE ON PINE. SLIDE LOADED WITH 100 POUNDS. VELOCITY IN INCHES IN A MINUTE.

V.	Coefficient of Friction.
5	.19
11	.20¾
75	.24
300	.25

TABLE II.—LEATHER ON PINE. SLIDE LOADED WITH 100 POUNDS. VELOCITY IN INCHES IN A MINUTE.

V.	Coefficient of Friction.
.79	.22
1.58	.27
3.94	.33
9.98	.36
29.14	.38
72.50	.41
157.50	.43
226.80	.45
300.	.46
466.	.47½

These experiments show the increase of friction with the velocity at low speeds quite clearly; and in connection with the series published last March, which showed its decrease at high velocities, would prove the high probability of the law as I have stated it, especially since the experiment on the inclined plane has been made to show a variation of the coefficient from a low value, through a maximum, to a low value again, while the velocity constantly increases.

(3.) *Friction of belts on the surface of cast iron pulleys.*—A piece of leather belting was hung over a cast iron pulley. To one end a determined tension was given by a fixed weight; to the other end was attached a spring dynamometer. The tension of the ends of the belt being known, the coefficient of friction was easily found. Several pulleys were used, and various kinds of belting; and a considerable range of tensions was employed, with uniform results.

Two tables are here given, one selected to show the increase of the coefficient at low speeds, the other to show the existence of a maximum coefficient at a definite velocity. In the third table, the first column gives the velocity in feet in a minute; the second and third give the tensions of the ends of the belt; and the fourth gives the relative values of the coefficients found, the maximum in each case being represented by 1.00. I give relative values, since they show variations more clearly than the absolute values. (*See Tables on following page.*)

(4.) *Friction of wrought iron journals in boxes of different materials.*—In this course of experiments a modification of the friction brake was used. A description of the arrangement in one series will serve for all the others. A shaft 1″ in diameter was adjusted so that

TABLE III.

V.	T₂.	T₁.	C.
.37	30	13	.42
.52	30	12½	.44
1.1	30	11½	.48
2.3	30	10½	.53
2.9	30	10	.55
4.4	30	9½	.58
15.4	30	6½	.78
34.1	30	5½	.86
80.3	30	4½	.96
104.5	30	4¼	.99
228.8	30	4¼—	1.00

In the following table only velocities and relative coefficients are given:

TABLE IV.

V.	C.
18	.82
92	.93
660	1.00
1190	.96
1980	.82
2969	.69

it could be driven at almost any rate between one revolution in two days and 1,000 in a minute. A hole was bored through a block of cast iron 3½″×3½″×1½″, and carefully fitted to the shaft; rigid iron rods were screwed into the top and bottom of this block, and adjusted to stand in a vertical line at right angles to the shaft. Upon these rods slotted weights could be placed, and thus the pressure upon the shaft and the center of gravity of the brake could be readily adjusted. Upon the front of the block a plane mirror was fastened, and before it, at a convenient distance, were placed a scale and telescope. When the shaft was turned, the friction between it and the brake caused the latter to turn until the moment of the friction was equal to that of the brake, and the angle at which this equality was established could be read from the scale by the telescope. As the center of gravity was always adjusted so that the brake never revolved through an angle of more than three degrees, the scale readings were approximately proportional to the coefficient of friction; and since relative and not absolute results were sought for, the labor of reduction was not undertaken. Several tables will be given to illustrate the method of conducting a series of experiments with this apparatus.

The results given above were made with high velocities, and show coefficients of friction decreasing as the velocity increases.

TABLE V.—WROUGHT IRON SHAFT, 1″ DIAM.; BOX, CAST IRON, 1½″ LONG; LOAD, 100 LBS. SHAFT WELL OILED.

Velocity of the circumference of the shaft

	No. 1, 72″.	No. 2, 272″.	No. 3, 605″.	No. 4, 1320″.
Scale readings	515	500	...	...
	515	500	...	...
	515	495	...	...
	515	495	...	...
	515	495	...	...
	515	495	...	...
	515	...	485	...
	515	...	485	...
	520	...	485	...
	520	...	485	...
	520	...	485	...
	520	...	490	...
	525	...	...	480
	525	...	...	480
	525	...	...	480
	525	...	...	475
	525	...	...	480
	525	...	...	485
Mean	519	497	486	480
Position of equilibrium.	464	464	464	464
Deflections	55	33	22	16

Relative values of the coefficient of friction.

No. 1, 1.00, No. 2, .60. No. 3, .40. No. 4, .29.

The results of a similar series with very low speeds are given in the next table:

TABLE VI.

Velocity of the circumference of the shaft: No. 1, .007″; No. 2, .027″; No. 3, .060″; No. 4, .132″ in a minute.

No. 1, .37. No. 2, .51. No. 3, .73. No. 4, 1.00.

These results, unlike those of the former table, show a coefficient increasing as the velocity increases.

A large number of experiments similar to those given above, have been made with uniform results.

I have been able to verify experimentally the law stated early in this paper in the following cases; wood sliding on wood, wood on iron, leather on iron, zinc on iron, and copper on iron; and to obtain results verifying the first half of the law in the case of leather on wood.

The experiments above detailed make it easy to explain the various results obtained by the three authorities quoted at the beginning of this paper. Morin experimented under conditions which gave him a coefficient very near the maximum, and thus his results are approximately constant. Bochet experimented with railway trains, his conditions were high speeds, hard rubbing surfaces, and great intensity of pressure. All these circumstances are favorable to the result he obtained, namely, a coefficient decreasing as the velocity increases. Hirn, on the other hand, employed very light pressures, less than two pounds on a square inch, and kept his rubbing surfaces so thoroughly lubricated that the friction was between oil and oil instead of two metal surfaces; his speeds were not very great. These conditions are precisely the ones I have found favorable to the results he reached,— a coefficient, increasing as the velocity increases. It would be very easy to form a theory which would account for the variation of friction with the velocity, under the rule I have given.

It is well known that a given deflection in a bar is produced by a weight acting for five seconds, for example, that the same deflection may be produced, by a less weight acting for a longer time. Now, as the force required to overcome friction is, partially at least, expended in bending down the minute irregularities on the surface of the rubbing bodies, it becomes evident how, other things being equal, a rapid motion would call for the exertion of a greater force than would be required if the motion were slow.

On the other hand, the longer two surfaces under pressure are in contact the greater must be the interlocking of the irregularities upon the rubbing surfaces. On this account a rapid motion would not require the expenditure of so great a force to overcome the friction. Thus, we have two effects, varying with the velocity, but having opposite signs. Now it is not probable, from the nature of the case, that these effects are numerically equal, or even proportional, and thus we can, at the least, say that the conditions are favorable to the existence of a maximum resultant effect. Having, however, ascertained the fact by experiment, the explanation becomes a matter of minor importance.

It may be said that these facts have no practical importance at the velocities ordinarily employed. I would call attention to Table V, where it will be seen that by increasing the velocity of shafting within the limits of ordinary shop practice, a reduction of the coefficient of friction of quite fifty per cent. may be made. The pressure on the shaft might also be reduced, for it would be unnecessary to maintain so great a tension upon the belts, and thus in some cases a very considerable economy of power might be effected. We know that in many shops a large fraction of the power developed by the engine is expended in overcoming the friction of the shafting and machinery.

On the other hand, it is desirable that the friction of the belt upon the pulley should be as great as possible. The conditions usually met with, which determine the friction of belts, are, low intensities of pressure, a rubbing surface which yields with considerable ease. In such cases a high speed is needed to develop the greatest amount of friction. See Tables III. and IV.

REPORTS OF ENGINEERING SOCIETIES.

American Society of Civil Engineers.—Among the recent papers presented to the Society are:—Brick Dams, by Edward P. North C.E.: and Approximate Determination of Stresses in Eye-Bar Heads, by W. H. Burr C.E.

This latter paper forms the chief part of the May number of the Transactions.

We shall present an abstract of it in some number of the present volume.

IRON AND STEEL NOTES.

New Process for Making Steel.—The Red Moss Metal Company, Warrington, have been some years developing a method of making steel direct; and having succeeded so far as to get the new steel in considerable quantities, and with highly satisfactory results, into the market, we propose to give a short account of the process, principally in the words of Mr. Larkin, the inventor of the process, and to add a few supplementary statements of what we ourselves have seen of the company's works, and the steel they are daily producing. The initial efforts of the company were directed to the magnetic iron-sands as the most convenient for their operation. These, however, have been for some time, and for commercial reasons chiefly, abandoned in favor of ore from Marabella on the south coast of Spain. The large and small lumps of this ore first passed through the jaws of a Blake's crusher, set as closely together at the bottom as practicable, and the crushed material is sifted as it falls. The coarser portion is then passed through a disintegrator. In this way the whole bulk of the ore is very cheaply and readily reduced to the condition of the iron-sand already described. But of course the gangue of the ore is crushed equally with the ore itself; and the next step is to separate the actual ore from all such extraneous matter, and get as nearly as possible the pure oxide of iron. This is very effectually done by means of a self-acting magnetic separating machine, specially devised for the purpose, and capable of dealing with large quantities of material. In this machine the particles of magnetic oxide are picked up by magnetic attraction, and carried in their proper receptacle, while the refuse is safely deposited in another. Having thus got as pure and rich a material as possible in a powdered condition, the next operation is to thoroughly mix with it a sufficient quantity of powdered carbonaceous matter to combine with the oxygen of the ore, and thus effect its reduction. The carbonaceous matter used consists of powdered charcoal and powdered resin, or other suitable bituminous substance, the two being reckoned together somewhat in excess of the oxygen to be removed. This mixture of powdered ore and carbonaceous powders is slightly warmed, and compressed into bricks in an ordinary brick press, and will then be ready for the reducing furnace. The reducing furnace consists of a series of D-shaped gas retorts, with doors to open at each end. These retorts are heated by a fire acting somewhat upon the principle of a Siemens' gas producer, and are thoroughly supported throughout their entire length by an intricate arrangement of brickwork, which also serves to prevent a too ready escape of hot air into the flue. The burning gases from the fire are also made to completely envelop the retorts by being carried over and under in a zigzag way, thus still further delaying their passage and arresting the heat with which they are charged. Air-holes are opened at regular intervals in order to complete the combustion of the gases as they circulate around the retorts, thus securing the greatest heat where it is actually wanted, and also securing complete combustion of the fuel used. The consumption of the smoke is perfect. There are other points of importance in connection with the furnace, but as they would not help to a clear idea of the method as a whole they may be omitted. Let the reader now imagine one of these retorts at an average working heat, empty, and ready to be charged. The door being removed from the feeding end of the retort, a small stack of pressed bricks, consisting of ore and carbonaceous matter, and of bulk to fill the section of the retort, is closely packed on a rectangular iron plate and pushed into the further end by means of an iron rod. The plate is then withdrawn, leaving the small stacks of bricks securely placed. A second and third feed immediately follow, filling the retort, which is at once closed. After having been exposed to a pretty full red heat for nearly twenty-four hours, gas will have ceased to be given out, the carbonaceous matter will have become practically consumed, and the oxide of iron will have become converted into red-hot iron powder. The next problem is how to convey this red-hot powder from the retort without exposure to the atmospheric air, and to keep it so till it is cold. A charge is now supposed ready for removal. Ordinary coal gas is first, by means of pipes provided for the purpose, turned on into the inside of the discharging end of the retort, in order to produce a full outward pressure of gas while the discharging door is removed, the door being at the underside of a projecting end-piece of the retort. The door being thus removed, an iron receiver is brought up closely under the projecting end-piece, and securely supported there. By a similar arrangement of pipes, gas is now let also into the inside of the feeding end of the retort, when the door of that end is quickly removed, and a temporary door with a wide slot half way down the middle of it is put in its place. The slot is for the introduction and working of the discharging tools, by which the red-hot powder is quickly pushed forward into the receiver placed at the discharging end. As soon as the retort is empty, the gas at both ends is turned off, and the iron receiver containing the metallic powder is removed and kept carefully closed until its contents are cool. When the metallic powder is sufficiently cooled down, and no injury can arise from its exposure, it is turned out of the receiver, and again passed through the disintegrator and the magnetic machine for a final purification. Thus by a few simple, and almost self-acting operations, requiring little more than faithful attention and accuracy in weighing and mixing, the steel maker is able to produce pure metallic powder. For the production of tool-steel the operator mixes with the metallic powder (besides some small percentage of flux) whatever additional amount of carbon may be needed, chiefly in the form of resin. This resin easily makes it possible to compress the finished powder into solid cakes, in the same way as the bricks of ore and charcoal were compressed in the first instance. The cakes of finished material are then stacked up, ready to be melted in crucibles in the usual way, with the addition of manganese or any other alloy that may be found advantageous.—*Iron.*

RAILWAY NOTES.

In 1867 experiments were made with iron cross rods in substitution for oak sleepers on the Nassau Railway, a stretch of seven and a half miles, from Oberlahnstein to Ems, being the first line laid with them. They have stood well, and the Lahn line is now being laid with them, on Hilf's system. Experiments have been made on other lines with, it is reported, complete success. The Belgian lines, as laid with Vignoles rails and oak sleepers, cost the State 3 fr. 28 c. per current meter per annum for maintenance, and 33 fr. 66 c. for first laying. On Hilf's system the State would have to expend 2 fr. 12 c. in maintenance, and 33 fr. 66c. per meter running first cost. The gain to the state would be thus 62 centimes per meter per annum. These calculations are on the basis of the duration of oak sleepers not exceeding ten years, while it is shown on the other hand that pine sleepers, treated with sulphates, will last fifty. The Germans are still complaining of the high rates they have to pay for the carriage of heavy goods. The addition made last August to rates then current, was as high as 20 per cent., and dear carriage makes competition with foreign makers the more difficult. The increase in outlay on carriage alone naturally becomes a very serious item, now that competition is so exceptionally close. One establishment alone, the Hord Smelting Works, lost—we may perhaps venture to say—close on £2000 last year by the operation of this increased percentage. How to lower freights is a question, the solution of which, in the opinion of many Germans, is possible only by the purchase of all the railways by the State, and their management by the State on the public behalf. As is well-known, this idea is finding favor with the heads of the paternal Government of Germany, and it is confidently expected that on the next assembly of the Prussian Landtag, a proposal in that sense will be laid before it. Coal is active, both in Westphalia and the Saar district. Last year saw a rise in production in both of 5 and 6 per cent. respectively.

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ENGINEERING STRUCTURES.

The Bergen tunnel.—The second tunnel under Bergen Hill, begun in the fall of 1873, has just been completed. This tunnel is at the eastern terminus of the Delaware, Lackawanna and Western R.R. The tunnel has been made under the supervision of James Archibald, of Scranton. The tunnel is 27 feet wide in the clear and 18 feet over top of iron clear; its entire height is 20 feet and 7 inches. From end to end the bore has been through Dolerite, commonly known as "trap rock," a stone which is said to be hard to drill but easy to blast. For four fifths of its length it is brick-arched. It is ventilated by seven shafts, all of which are brick-lined, with one exception. Three are elliptical, opening the full width of the tunnel, and 8 feet wide; one is 16½ feet by 7 feet; two are 6 feet in diameter, and another, opening the full width of the tunnel, is 12 feet wide. With a west wind it is claimed no inconvenience will be felt by passengers through the tunnel from smoke and steam, the shafts serving as excellent chimneys with the wind in this quarter. The cost of the excavation and shafts was $800,000, of the brick arching $105,000.—*Railway Review.*

Testing the Niagara Suspension Bridge.—The suspension bridge over the Niagara river was constructed by John A. Roebling, in 1855. It has a span of 821 feet, and a deflection of 59 feet; 14,560 wires are employed in the cables, the ultimate strength of which is 12,000 tons. Since its completion, in the year mentioned the bridge has been subjected to the almost constant strain of heavy railroad trains; and thus for a period of 22 years it has undergone a trial of the greatest severity. Quite recently it was deemed advisable to overhaul the structure thoroughly in order to determine whether any repairs were required, or whether the jarring or straining to which the wires had been submitted had—as some theorists believed possible—impaired the quality or tenacity of the iron. Accordingly the bridge was closed to traffic; and to Colonel William H. Paine, Assistant Engineer of the East River bridge, was assigned the duty of critically inspecting the structure. Colonel Paine has sent to the *Scientific American* a short account of his investigations, which have resulted in his conclusion that the safety of the bridge is in no wise lessened. The anchorage cables were imbedded in masonry and cement, which it was necessary to remove in order to admit of their examination; a task of no small difficulty, as the masonry was like solid rock. It was found that out of the 14,560 wires less than a dozen were seriously corroded, and these were in the first anchorage. The metal on the other wires showed the original grain with distinctness. Not content, however, with this highly-favorable appearance, Colonel Paine proceeded to experiment upon the wire, in order to discover whether the means provided to allow of its expansion and contraction—namely, the placing of the bed-plates which receive the cables on top of the towers, on rollers—had been sufficient to prevent the longitudinal stress upon the filaments destroying their elastic quality. An apparatus was used capable of marking a stretch to 1-10,000th of an inch; and this being adjusted, a heavy freight train was moved upon the bridge. The elongation of the wire was found to be very nearly equal to that which the formula, used by engineers for ascertaining such results, showed the stretch of a perfect cable, similarly made, should be under like strain: so that not only had twenty-two years' service not resulted in any corrosion of the wire, but the elasticity of the same under the enormous strains had not been impaired. In the second experiment, a single wire from one of the main cables was tested. A strand of 520 wires was selected, and the binding removed, so that every wire was perfectly free. Three wires were then chosen, and across them a knife mark was made. The middle wire of the three was cut at the mark; and on testing it by the delicate instrument above noted, it was found to have contracted to within a small fraction of what it should have reached when relieved from its portion of the weight of the bridge. In experiment No. 3 a single wire was detached and weighted until it broke.

The object here was to see whether the nature of the metal had been altered; for if the iron had become granular and had lost its cohesiveness, the fracture would be a straight one, similar to that of cast iron. On the contrary, the wire, when subjected to the stress, extended until its diameter was reduced fifty per cent. before it broke. A cable guy was next selected and made to sustain a dead weight. The construction estimate places the maximum burden which that guy would ever be required to bear at fifteen tons. The guy parted at fifty-three tons. To show how perfectly the wire had retained its original characteristics, Colonel Paine, having observed that the wire, on being removed from the cable, tended to coil in a circle of about five feet in diamater, sent to the person who had originally prepared the wire for its place for information as to the size of its former coil. He learned that it had been wound on a drum two feet in diameter, and that it had been subjected to a straightening process which took about half the curve out of it. Certainly no more remarkable proof could be adduced to show that the spring of the wire had in no respect been overcome. Although the bridge has thus been shown to be thoroughly safe, and to have wonderfully withstood wear, still more elaborate examinations are to be made, and the structure will not be open to traffic until these are completed.—*English Mechanic.*

ORDNANCE AND NAVAL.

Torpedoes at Sea.—A very curious experiment has been tried off Cherbourgh, with one of Messrs. Thorneycroft's torpedo boats. This is a little gray craft, which floats with her deck close to the water-level, and is capable of steaming at 18 knots an hour. She carries a powerful torpedo at the end of a long spar. Admiral Jaurez, commanding the French Channel Squadron, had an old craft, the Bayonaise, taken in tow by a powerful tug, and run out to sea. The torpedo boat, manned by Lieutenant Lemoinne and three others, started in pursuit, and after a little time overtook the Bayonaise and ran into her. The torpedo exploded immediately, and the Bayonaise went down like a stone. Two days later the experiment was repeated with the same success on another ship. No one doubted that an old wooden ship could be sunk in this way, or probably an ironclad; but there was great room for doubt whether the torpedo boat and her crew could sustain the shock of the explosion. The Cherbourg experiments are eminently useful, as demonstrating that torpedo boats can be used apparently with fair safety, that is to say, they will not injure themselves. It would be quite possible to over-estimate the worth of this system of attack, however. An ironclad having the power of depressing her great guns sufficiently would in the day-time wait quietly until the boat was within a few hundred yards, and then with one or two shots dispose of her. But at night the attack of such a foe would be a different matter.

BOOK NOTICES

The Art of Electro-Metallurgy. By G. Gore, F. R. S. London, 1877. For sale by D. Van Nostrand. Price $1.50.

This work presents fully the theory of Electro-deposition and a description of all known processes.

The use and management of magneto electric machines is fully treated.

A fair supply of figures illustrates the text.

The book is the last addition to the series of Text Books of Science.

Hand Book of Electrical Diagrams, Second Edition. By Charles H. Davis & Frank B. Rae. New York: D. Van Nostrand. Price $2.00.

The first edition of this book is only a year old.

In a series of plates, thirty in number, all the important electrical connections employed in telegraphy are in the fullest manner illustrated.

The plates are exceptionally neat and the small amount of descriptive text is compactly written and sufficiently supplements the illustrations.

International Law. By John A. Dahlgren, late Rear Admiral U. S. N. Edited by Charles Cowley. Boston: For sale by D. Van Nostrand. Price $1.50.

This book presents the following topics. Biographical Sketch of the Author; Law of Blockade; Contraband of War; Visitation and Search; Duties of Naval Commanders of Foreign Stations; Addenda by the Editor.

The Steam Engine. By F. J. Bramwell, F. R. S. London: Macmillian & Co. For sale by D. Van Nostrand. Price 25 cts.

This is an illustrated pamphlet of the Science Lecture Series, prepared for the South Kensington Course.

The discussion of the action of steam for different degrees of expansion is quite complete.

There are thirteen illustrations.

The Architect's Guide. By Frederick Rogers, Architect. London: Crosby, Lockwood & Co. For sale by D. Van Nostrand. Price $3.00.

This is a work for the master builder rather than the professional architect, and is designed to supply deficiencies in rudimentary education. There are many useful tables compiled from various sources.

The typography and cuts may be ranked between indifferent and poor.

Fire Protection. By Eyre M. Shaw, Chief Officer of London Fire Brigade. London: Charles & Edwin Layton. For sale by D. Van Nostrand. Price $6.00.

This is a manual of organization, machinery and discipline of the Fire Brigade of London.

The elaborate description of fire-escapes and their management is a prominent feature of the work, although nothing seems wanting that pertains to the duty of the firemen in any emergency.

The Alkali Trade. By Charles Thomas Kingzett. London: Longmans, Green & Co. For sale by D. Van Nostrand. Price $6.00.

This is a new contribution to practical technology. A complete description of the processess of manufacture of the salts of the alkalies is given accompanied with abundant illustration of the necessary machinery.

Proceedings of the Institution of Civil Engineers.—We have lately received the following:

Experiments and Observations on the Emission of Heat by Hot-Water Pipes. By Wm. Anderson, C.E.

Also Canadian Narrow-Gauge Railways. By Edmund Wragge C.E.

The Forces of Nature. By Amidie Guillemin. Translated from the French by Mrs. Norman Lockyer. Part I. Illustrated by nearly 500 engravings.

This is a voluminous work on general Physics of most attractive appearance. Judging from the portion before us, it will be when complete, an indispensable compend of such scientific subjects as we are accustomed to group under the title of Natural Philosophy. It will be a complete reference book for the general reader.

A PRACTICAL TREATISE ON LIGHTNING PROTECTION. By HENRY W. SPANG. With Illustrations 12 mo. cloth, $1.50. Claxton, Remsen & Haffelfinger. For sale by D. Van Nostrand.

This book aims to demonstrate that the electricity of the earth is principally accumulated in the subterranean water-bed, and shows that nearly all the lightning rods or conductors now erected cannot be relied upon for the easy passage of heavy lightening discharges, owing to the small quantity of metal they contain and the dry condition of the earth around and beneath their lower terminals.

It gives plain and explicit directions for the proper protection of buildings of every description, ships, oil-tanks, steam-boilers, wooden bridges, telegraph poles, etc.

The author insists that the metal roofs, rain and gas pipes, iron fronts, stacks, etc., about buildings are much better conductors of lightning discharges than the lightning rods with polished points, etc., as heretofore employed, and if properly connected with the earth will effect absolute protection.

A simple and reliable method is explained, by which the rain and waste water can be utilized to always maintain an easy path in the earth for lightning discharges, and thereby enable buildings and other structures to be properly protected.

REPORT OF CHIEF ENGINEER J. W. KING, UNITED STATES NAVY, ON EUROPEAN SHIPS OF WAR AND THEIR ARMAMENT, NAVAL ADMINISTRATION AND ECONOMY, MARINE CONSTRUCTIONS AND APPLIANCES, &c. Washington: Government Printing Office.

This is a very remarkable book, which we have read with some astonishment, and just a little dismay. In July, 1875, Mr. J. W. King, Chief Engineer of the United States Navy, and late Chief of the Bureau of Steam Engineering, was instructed by his Government to proceed to Europe and collect information concerning Old World navies. He left New York in Aug., 1875, and did not return to the States until July, 1876. The volume before us has just reached this country, and is Mr. King's report on what he saw and what he was told. The most noteworthy feature of the work is, that it contains more minute and copious information regarding the British Navy than any other book in any language. Indeed, the volume ought to be in the hands of every officer of our navy, for it contains information which very few of them possess; and we confess that while we feel alarm at the publication of such a work in a way that supplies information concerning every detail of the strength of our ships to foreign Governments, we also feel some annoyance that all this information has been carefully kept back from Englishmen. Is it not strange that the best book of reference on the British Navy should be the report of an American Engineer to his government? The policy which we deprecate has, however, nothing to do with the merits or demerits of the work before us; and leaving its consideration, we may proceed at once to say that Mr. King has done his work thoroughly well; his style is concise and lucid, and as a text-book of the British navy, nothing can be found to compare with the volume before us. Of the two hundred and seventy-three large octavo pages of which the work consists, no fewer than 115 are devoted to describing our ships of war, while much of the remainder of the work is occupied with the discussion of questions almost entirely concerning English systems of constructing engines, &c. The information supplied concerning the navies of France, Russia, Germany, &c., is very meagre; no doubt Mr. King did not find that all that he asked for was placed at once and without hesitation into his hands. The work is illustrated with very effective diagrams, none of which, so far as we are aware, have been published before. Thus for example, the hydraulic loading gear of the Thunderer's guns is illustrated and minutely described.

A chapter is devoted to each of our best and most recent ships, and each chapter contains a plan and an elevation of the vessel, showing the arrangement of her armor, armor-plated deck, turrets, and guns. A description of the machinery of the ship is contained in each chapter, and we may say shortly that these descriptions are admirably written. Mr. King deals but little in criticism; and when we have said that the work is an accurate and complete description of the most powerful men-of-war in the world, and that in this respect it stands alone, we have said all perhaps that need be said. We cannot conclude without warning our readers that as this is a Government publication some difficulty may be experienced in obtaining it in this country.—*Engineer*.

STRENGTH AND DETERMINATION OF THE DIMENSIONS OF STRUCTURES OF IRON AND STEEL, WITH REFERENCE TO THE LATEST INVESTIGATIONS. An elementary appendix to all text books upon iron and steel constructions: by Dr. Ph. JACOB J. WEYRAUCH, Professor in the Polytechnic School at Stuttgart. Translated by A. JAY DUBOIS, Ph. D., Professor of Civil and Mechanical Engineering, Lehigh University. New York, John Wiley & Sons; also another translation published by D. Van Nostrand.

[From "The Railroad Gazette."]

Reading the title of this book and taking a first glance at its contents we experienced a feeling of pleasure and satisfaction that the time had come when theorists, after so many brilliant mathematical fireworks, begin to appreciate better the hard, slow work of practice and experiments, and as it were to return from the realms of hypothesis to reality and truth.

But examining the entire book carefully, we were sorry to observe that its title promises more than its thirty chapters realize. With pleasure we admit that Professor Weyrauch has done his best to collect data from the labors of Kirkaldy, Styffe, Woehler, Bauschinger, Thurston, etc., and also to study the views of a great number of commentators on old and new experiments, as evidenced by numerous references, constituting by no means the least valuable part of the book. Herr Weyrauch has touched many questions which undoubtedly may lead to further valuable experimental researches. In the course of his deductions he could not help letting us see how many formulæ and theories of constructive design, though satisfactory to the student, are nevertheless based upon wild hypotheses, but little in reality we know about the properties of material, and how much less about the details of construction.

If these facts present themselves clearly to the reader of Herr Weyrauch's book, and if many will read it, Herr Weyrauch has done a good work. But the new book can hardly be considered an appendix to *all* text books upon iron and steel construction, and it appears to us that Professor Dubois, who is a literal translator of German idioms, would perhaps do still more good if he would translate into German the second volume of the theory of strains by B. B. Stoney. This volume of 268 pages, with the exception of Woehler's and Bauschinger's experiments, contains at least as much real knowledge as the book now presented, and it is more suited to be an appendix to German text books than that of Herr Weyrauch is adapted to be an appendix to English or American ones.

The English Iron Commissioners of 1849 made experiments on impact and repeated strains of cast and wrought-iron bars, and found the "law" that repeated strains below the ultimate strength cause rupture. These experiments were not further followed up in England, except that Fairbairn—about the time that Woehler began his experiments proper, namely, in 1860—tested a riveted plate girder under repeated flexures. Mr. Fairbairn discovered that this girder, whose strains per square inch he probably computed too low (he did not add the concentrated rivet strains, the girder being loaded with a single load in the center), broke after a comparatively small number of repetitions (3,463,000) of the strain of 18,600 lbs. per square inch.

Mr. Fairbairn did not describe the pitch of the rivets used, so that we cannot judge how great the strain per square inch actually was.

Herr Woehler, being Engineer of the shops of the Silesian Railroad in Prussia since 1858, began to investigate the strains *actually* sustained by the axles of railroad freight and passenger cars.

Preliminary results thus obtained were given in the Berlin official engineering periodical (*Zeitschrift fuer Bauwesen*) in the year 1860, and then presented a number of most interesting facts, worthy of careful study. Herr Woehler continued the experiments, got up new apparatus for testing axles under as nearly as possible the same strains as they have to sustain in practice, and, the results being very interesting, extended his labors to repeated flexures in one direction and in both directions, under a permanent strain with superposed variable strains, under repeated extensions, etc., etc. The results are clearly and modestly set forth and are well interpreted in the volumes of 1863, 1866 and 1870 of the Berlin periodical quoted.

These reports in part were translated and published in England and in America. They were frequently quoted and for quite a number of years have influenced the views held by our designers, by whom they probably were at first more appreciated than even in Germany. Experiments somewhat similar to those of Herr Woehler were directed by Stummer, of Austria, and Herr Woehler, having been called to other duties, arranged to have an engineer charged with the continuance of his own.

This labor was transferred to Professor Spangenberg, who made a report in 1875, a translation of which has appeared in Van Nostrand's "Science Series." The book written by Herr Weyrauch is principally founded on Woehler's results, or rather on a formula which was believed by Professor Launhardt to embrace these results. One would think that especial pains would have been taken to prove the stability of this formula which has to support Professor Weyrauch's mode of calculation of sections of bridges and similar structures. But Professor Weyrauch has not given or interpreted Woehler's specific experiments, and he has failed to bring sufficient proof of the formula on which he builds.

The old method of proportioning the parts of a structure by adopting one or two maxima strains per square inch, and therefore calculating the sections singly from *the maximum* strain in a member, has been known to be crude for some time. In fact, at the time of the famous English Iron Commission it was proposed to use a higher (double the) "factor of safety" for variable than for static stress. The late Professor Rankine also recommended this method; it was used in England and elsewhere; it was embraced in a modified form by the Engineer Gerber of Munich; it has been recommended in this country, and many years ago Mr. Shaler Smith in a pamphlet showed how to take into consideration the *frequency* of the maxima strains *and* the distinction between *static* and *live* strains.

It is known that car springs are exerted to calculated statical strains as high as 100,000 lbs. per square inch, and that the motion of the cars increases this strain not inconsiderably on rough parts of the road. Woehler examined the properties of such springs by putting steel bars under the action of constant flexure increased by variable flexures similar to those caused by motion.

Thus far, then, Professor Weyrauch tells us nothing new. But what is new is his somewhat pedantic application of Professor Launhardt's hypothetical formula to subjects inaccessible to mathematical reasoning, like riveting.

We will now briefly give the material which has led to Launhardt's formula and to Professor Weyrauch's book and to the translation before us:

Of Herr Woehler's eighteen tables of experimental results, we here give only Table IX., which is intended to form the basis of Launhardt's formula: (*See following page.*)

The manner in which the formula was derived from these figures is about this: Considering experiment No, 17 of this first series, it is *assumed* that 55,000 lbs. is the maximum variable strain which will just leave a bar durable for infinity. From experiment 9 of the second series it is further *judged* and *assumed*

EXPERIMENTS ON BARS, BENT IN ONE DIRECTION WITHIN FIXED STRAINS.

Bars of crucible spring steel (by Krupp).

No. of Experiments.	Maximum Strain per sq. in.	No. of Flexures Causing Rupture.	Remarks.
11	110,000	39,950	Steel not hardened.
12	99,000	72,450	
13	88,000	132,650	
14	88,000	117,000	
15	77,000	197,400	
16	66,000	468,200	
17	55,000		Not broken after 40,600,000 repetitions.
18	49,500		Not broken after 32,942,000 repetitions.

	Strains limited between Pounds per square inch. Maxima. Minima.		
1	132,000 and 33,000	22,900	Hardened.
2	132,000 and 44,000	35,600	
3	132,000 and 55,000	86,000	
4	132,000 and 66,000	191,100	
5	132,000 and 77,000	50,100	
6	132,000 and 77,000	251,400	
7	132,000 and 88,000		Not broken after 35,600,000 repetitions.
8	132.000 and 99,000	33,878,700	Not hardened.
9	110,000 and 18,300	62,000	
10	110,000 and 36,666	149,800	
11	110,000 and 55,000	400,050	
12	110,000 and 64,100	576,700	
13	110,000 and 72,690		Not broken after 19,673,300 repetitions.
14	99,000 and 22,000	81,200	Not hardened.
15	99,000 and 33,000	156,200	
16	99,000 and 44,000	225,300	
17	99,000 and 55,000	1,238,900	
18	99,000 and 55,000	300,900	
19	99,000 and 66,000		Not broken after 33,600,000 flexures.
20	88,000 and 11,000	99,700.	Not hardened.
21	88,000 and 22,000	176,000	
22	88,000 and 33,000	619,600	
23	88,000 and 33,000	2,135,670	Not broken after
24	88,000 and 44,000		35,800,000 flexures.
25	88,000 and 44,000		38,000,000 flexures.
26	88,000 and 61,600		36,000,000 flexures.
27	77,000 and 11,000	286,100	
28	77,000 and 22,000	701,800	
29	77,000 and 27,500		36,600,000 flexures.
30	77,000 and 33,000		31,150,000 flexures.

that 121,000 lbs. will constitute the ultimate strength of the steel.

It is further *interpreted and assumed* that the *maximum* strains

99,000, 88,000, 77,000 lbs. can be carried for an infinitely long time if the *permanent* strains respectively are:

66,000, 44,000, 27,500 lbs. per square inch, so that the *variations* repeated *ad infinitum* would be:

33,000, 44,000, 49,500 lbs. per square inch.

It is seen that the variations decrease if the maxima increase, and the minima or permanent strains increase if the maxima increase. All strain is variable strain with no permanent strain for 55,000 lbs. (1), all is permanent strain with no variation for 121,000 lbs. per square inch (2).

The formula is made to *suit* these two final conditions by putting:

Maximum = 55,000+Coefficients×unknown function.

The function must contain the minimum or permanent strain as factor, for then we get:

Maximum = 55,000+ Coefficients × (permanent or minimum strain) × other function; which by making the permanent or minimum strain = 0 gives:

Maximum=55,000+0=55,000. This answers (1).

Again, put the maximum into the unknown function as divisor, and you get:

$$\text{Max.}=55{,}000+\text{coefficent}\times\frac{\text{permanent strain.}}{\text{maximum strain.}}$$

For the end relation (2) maximum=121,000, all is permanent strain, and we get:

$$121{,}000=55{,}000+\text{coefficient}\times\frac{121{,}000,}{121{,}000,}$$

or coefficient=121,000−55,000=66,000 lbs. The discovery is now made, and all we have to do is to manipulate the experiments so that it will answer them. For this purpose Herr Weyrauch has *assumed:*

					Ultimate strength.
Permanent strain...	0	27,500	44,000	66,000	121,000
Maximum strain for infinite durability by experiment....	55,000	77,000	88,000	99,000	121,000

which agree with the values:

By formula...........	55,000	78,200	88,000	99,000	121.000

We confess that we have not sufficient confidence in this test. Experiment 16 of the first series for strains of 66,000 lbs. caused rupture after 468,000 repetitions; hence it does not follow that 40,600,000 repetitions of strains of 55,000 lbs. constitute infinite durability.

Comparing series III. of Woehler's experiments, we find rupture to have taken place after 45,000,000 repetitions, while another bar, under precisely the same strain, broke after 4,163,000 repetitions.

Experiments 6 and 7 of Table III., on Bochum's steel, also give evidence that it is unsafe to consider even 57,000,000 repetitions as unlimited durability. A bar was strained to 33,000 lbs., when it broke after 57 millions of rotations; and yet another bar of the same material broke after 3½ million rotations, the strain being less and only 30,800 lbs. per sq. in.

The experiments 6, 7 and 8 of the second series quoted show that 35,000,000 impacts do not secure unlimited durability.

The variations from 77,000 to 132,000 gave rupture after 257,000 repetitions; the variations from 88,000 to 132,000 did not cause rupture after 35,600,000 repetitions; and yet the *easier* variations of 99,000 to 132,000 lbs. again gave rupture after 33½ million repetitions.

Again, it is doubtful whether a new set of experiments on the same class of *steel* bars would have been equally accommodating, and it is also unproved that the result holds good for other kinds of steel or for wrought iron.

We may assume that other "formulæ" give equally good results, and can be made to suit the experiments just as well, as far as they go.

That we know nothing definite about this subject is rather indicated by the different methods adopted by Gerber, Muller and Schæffer. The formula of Launhardt has the sole merit of being arranged to suit the beginning and the end of the series (the figures 55,000 and 121,000 again being only guessed at), and this can be done in many different ways by otherwise equally empirical expressions.

Herr Weyrauch censures the three gentlemen named for following too closely Woehler's results. This would rather constitute merit, in our eyes.

Woehler's experiments are not finished and not complete. They have the great merit of showing the right way, which is that of testing material in the *same* manner in which it is *used.* Woehler's experiments are of greater value still, because of the experimenter's cautious interpretation being free from new theories. He does not involve us in a multitude of new formulæ, and he especially points at the great amount of work still to be done. He says in his resume: "The strength of connections, as *riveting,* wedge connections, etc., as well as other forms, needs *special* investigations by *experiments.* The results of experiments on bars with sudden changes of sections and the influence observed of the pressure of axles on the wheel centers have proved sufficiently the necessity of such special experiments."

But instead of waiting for these very experiments, Herr Weyrauch has poured over us a flood of formulæ on riveting, all based on the empiricism of Launhardt.

There is another difficulty about this formula which is touched upon by Herr Weyrauch. What shall we do in regard to compression? Woehler's results in this regard only confirm what we knew, namely, that iron is more durable under pressure than under tension, proof of which is breakage of axles in their tensile parts. But more than that: the strength of long compression members cannot be measured by these experiments. Here we have undoubtedly to consider the *ultimate breaking strength* in some respect, for it does not follow that because a member stands with 10 tons per square inch it also must be safe with 10½ tons. Therefore the *maximum* strain is to be considered, whether caused by live or dead load. Herr Weyrauch has entirely neglected this subject.

Another reason why Launhardt's or any other similar formula is insufficient is this: According to Woehler's experiments, *rarely occurring* high strains may be admissible, whereas those strains that occur at every passage of a locomotive must be correspondingly lowered. But the Professors Launhardt and Weyrauch would consider only the *maximum strains* of each kind instead of the *average strains.* It is known that some theorists have taken special pleasure in supposing imaginary engines in all sorts of improbable positions on single and double-track bridges, and then have calculated or determined graphically the maximum maximorum. Such calculations should not be considered as of practical value. These questions also are touched upon by Herr Woehler, but have not been taken up by Prof. Weyrauch.

Of the other parts of the new book we have this to say:

Chapter V. treats of the ultimate tensile and compressive (crushing) strength of iron and steel bars, plates, wire, etc.; but this chapter is rather short and does not do justice to the importance of the subject. We find in a heap the average strength of Swedes, Lorraine, Low Moor, Borsig, etc., iron, without notice of their origin and without consideration of their qualities.

Also the other chapters on elastic limit, annealing and tempering, influence of form, amount of carbon, influence of temperature and valuation of material are somewhat too cursory and incomplete to give a clear representation of the various properties. However, there are many points touched upon to which attention is directed, all of which can be more fully studied in the original books from which the material is collected.

The inadmissibility of Launhardt's formula for riveting we have mentioned. We may add that if Herr Weyrauch had divested this chapter of algebraic expressions, and had carefully studied and explained the experimental results which are at our disposition, he probably could have given a very valuable addition to the literature on this subject, which is still remarkably poor, on account of deficient interpretation of otherwise valuable material. There are some excellent remarks contained in this part of the Professor's book which, though familiar to experts in detail construction, are not so generally known among railroad engineers as could be wished. Finally, Herr Weyrauch speaks of shape iron (Professor Dubois translates Facon-Eisen by "figured or ornamental iron") as of inferior tenacity because difficult to roll. We can however state that there is no reason why shape iron should be of inferior quality because of its manufacture. Where this is found it is more a proof of ignorance in roll-turning and working the material. Beams, channels, angles, etc., are rather of higher than inferior quality, other things being equal.

Weyrauch's book can not be recommended to those who know nothing about the quality of material and whose judgment in such matters does not stand above that of its author.

So far as the translation is concerned, Professor Dubois has taken great pains to come as near as possible to the author's original text. There are some slight oversights. Instead of "notches in the rail flanges" he translates "rail chairs." Instead of "resistance to crippling" of the compression flange of a girder, he translates "resistance of flexure." Instead of "vertical flange plate" of a girder he translates "stay plates" (*Steh-Bleche*).

Where Professor Weyrauch corrects his earlier views as to the saving of metal in continuous bridges, Mr. Dubois, who has himself some weakness as to this interesting theory, qualifies the author's sentence, "the saving of material by reason of continuity is thus less," etc., by adding "somewhat" before the word "less," which remark would have been better in a foot note.

Another translation of the same work comes from the house of D. Van Nostrand, who claims to have obtained the prior right to publish the work in the way agreed upon among American publishers of foreign books, by having first announced its intended publication.

The Van Nostrand translation in some cases is more successful than the other in making the heavy sentences of the German author intelligible to the reader. It is a smaller volume than Professor Dubois' translation.

VAN NOSTRAND'S
ECLECTIC
ENGINEERING MAGAZINE.

NO. CIV.—AUGUST, 1877.—VOL. XVII.

NEW CONSTRUCTIONS IN GRAPHICAL STATICS.

By HENRY T. EDDY, C. E., Ph. D., University of Cincinnati.

Written for Van Nostrand's Magazine.

VIII.

SPHERICAL DOME OF METAL.

The dome which will be treated in the following construction is hemispherical in shape; but the proposed construction applies equally to domes of any different form generated by the revolution of the arc of some curve about a vertical axis: such forms are elliptic, parabolic or hyperbolic domes, as well as pointed or gothic domes, etc. Let the quadrant *aa* in Fig. 18, represent the part of the meridian section of a thin metallic dome between the crown and the springing circle. The metallic dome is supposed to be so thin that its thickness need not be represented in the Figure: the thickness of a dome of masonry, however, is a matter of prime importance and will be treated subsequently.

In a thin metallic dome the only thrust along a meridian section is necessarily in a direction tangent to that section at each point of it. This consideration will enable us to determine this thrust as well as the hoop tension or compression along any of the conical rings into which the dome may be supposed to be divided by a series of horizontal planes.

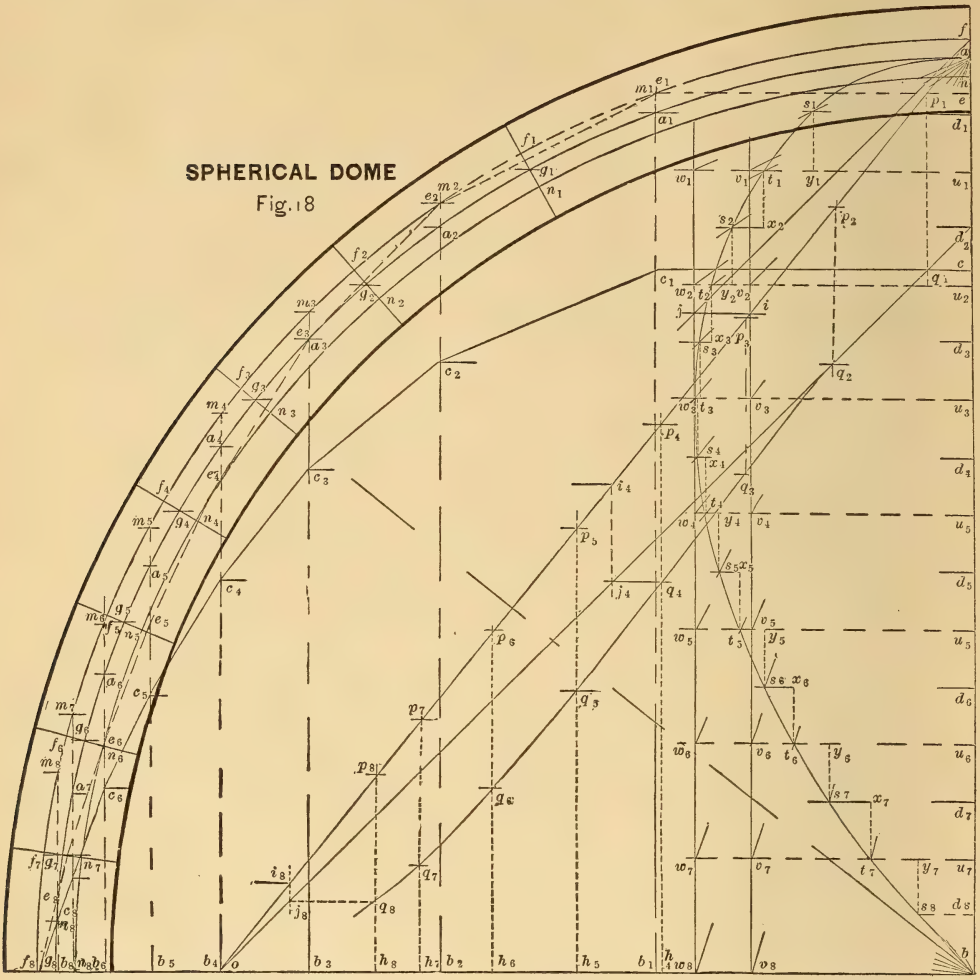
Let the height *ab* of the dome be divided into any number of parts, which we have in this case, for convenience, made equal. Let these equal parts of the type *du* be the distances between horizontal planes such that the planes through the points d_1, d_2, etc., cut small circles from the hemisphere which pass through the point a_1, a_2, etc., and similarly the planes through u_1, u_2, etc., cut small circles which pass through g_1, g_2, etc. Now suppose the thickness of this dome to be uniform, and if *ab* be taken to represent the weight of a quadrantal lune of the dome included between two meridian planes making some small angle with each other; then from the well-known expression for the area of the zone of a sphere it appears that ad_1 will represent the weight of that part of the lune above a_1d_1. Similarly au_1 is the weight of the lune ag_1; ad_2 the weight of aa_2, etc.

This method of obtaining the weight applies of course in case the dome is any segment of a sphere less than a hemisphere and of uniform thickness. If the thickness increases from the crown, the weights of the zones cut by equi-distant horizontal planes increase directly as the thickness. In case the dome is not spherical the weights must be determined by some process suited to the form of the dome and its variation in thickness.

Now the weight of the lune aa_1 is sus-

SPHERICAL DOME

Fig. 18



tained by a horizontal thrust which is the resultant of the horizontal pressures in the meridian planes by which it is bounded, and by a thrust, as before remarked, in the direction of the tangent at a. Draw a horizontal line through d_1, and through a a parallel to the tangent at a: these intersect at s_1, then is ad_1s_1 the triangle of forces which hold in equilibrium the lune aa_1. Similarly, au_1t_1 is the triangle of forces holding the lune ag_1 in equilibrium, etc. Draw a curve st through the points thus determined. This curve is a well-known cubic which when referred to ba as the axis of x and bg_8 as that of y has for its equation

$$\frac{y^2}{x^2}=\frac{r-x}{r+x}$$

On being traced at the right of a it has in the other quadrant of the dome a part like that here drawn forming a loop; it passes through b at an inclination of 45° and the two branches below b finally become tangent to a horizontal line drawn tangent to the circle aa of the dome. The curve has this remarkable property:—If any line be drawn from a, cutting the curve here drawn and, also, the part below bg_8, the product of these two radii vectores of the curve from the pole a is constant, and the locus of the intersection of the normals at these two points is a parabola.

Draw a vertical tangent to this curve: the point of contact is very near t_3, and g_3, the corresponding point of the dome is almost 52° from the crown a. A determination of this maximum point by means of the equation gives the height of it above b as $\frac{1}{2}(\sqrt{5}-1)\,r$, corresponding to about 51°49′. Now consider any zone, as, for example, that whose meridian section is g_1a_2: the upper edge is subjected to a thrust whose radial horizontal component is proportional to u_1t_1, while the horizontal thrust against its lower edge is proportional to d_2s_2, and the difference s_2x_2 between these radial forces produces a hoop compression around the zone proportional to s_2x_2. It will be seen that these differences which are of the type sx or ty, change sign at t_3. Hence all parts of the dome above 51° 49′ from the crown, are subjected to a hoop compression which vanishes at that distance from a, while all parts of the dome below this are subjected to hoop tension. This may be stated by saying that a thin dome of masonry would be stable under hoop compression as far as 51° 49′ from the crown, but unstable below that, being liable to crack open along its meridian sections. A thick dome of masonry, however, does not have the resultant thrust at every point of its meridian section in a direction which is tangential to its surface,—this will be discussed later.

It is necessary to determine the actual hoop tension or compression in any ring in order to determine the thickness of the dome such that the metal may not be subjected to too severe a stress.

The rule for obtaining hoop tension (we shall use the word tension to include both tension and compression) is: Multiply the intensity of the radial pressure by the radius of the hoop, the product is the tension at any meridian section of the hoop. The correctness of this rule appears at once from consideration of fluid pressure in a tube, in which it is seen that the tensions at the two extremities of a diameter prevent the total pressure on that diameter from tearing the tube asunder.

Now in the case before us t_1y_1 is the radial force distributed along a certain lune. The number of degrees of which the lune consists is at present undetermined: let it be determined on the supposition that it shall be such a number of degrees as to cause that the total radial force against it shall be equal to the hoop tension. Call the total radial force P and the hoop tension T, then the lune is to be such that $P=T$. Also let θ be the number of degrees in the lune, then $90^\circ \div \theta$ is the number of lunes in a quarter of the dome, and $90\,P \div \theta$ is the radial force against a quarter of the dome, which last must be divided by $\frac{1}{2}\pi$ to obtain the hoop tension; because if p is the intensity of radial pressure, $\frac{1}{2}\pi rp$ is the total pressure against a quadrant and rp, as previously stated, is the hoop tension. The ratio of these is $\frac{1}{2}\pi$, and by this we must divide the total radial pressure in every case to obtain hoop tension

$$\therefore \frac{180\,P}{\theta\pi}=T, \quad \therefore\ \theta=\frac{180^\circ}{\pi}$$

$$\text{for } P=T \quad \therefore\ \theta=57^\circ.3-$$

This is the number of degrees of which the lune must consist in order that when ab represents its weight, t_1y_1 shall represent the hoop tension in the meridian section a_1g_1. The expression we have found is independent of the radius of the ring, and hence holds for any other ring as g_1a_2, in which s_2x_2 is the hoop tension, etc. To find what fraction this lune is of the whole dome, divide θ by 360°

$$\therefore \frac{\theta}{360}=\frac{180}{360\pi}=\frac{1}{2\pi}=\frac{4}{25}\text{nearly},$$

from which the scale of weight is easily found, thus; let W be the total weight of the dome and r its radius, then

$2\pi r : W :: 1 : n$, the weight per unit, or the hoop tension per unit of the distances ty or sx.

Distances at or as, on the same scale, represent the thrust tangential to the dome in the direction of the meridian sections, and uniformly distributed over an arc of 57°.3− : *e.g.* if we divide at_2 measured as a force by $\theta \times u_2g_2$ measured as a distance we shall obtain the intensity of the meridian compression at the joint cut from the dome by the horizontal plane through a_2.

Analogous constructions hold for domes not spherical and not of uniform thickness. Approximate results may be obtained by assuming a spherical dome, or a series of spherical zones approximating in shape to the form which it is desired to treat.

SPHERICAL DOME OF MASONRY.

Let the dome treated be that in Fig. 18 in which the uniform thickness of the masonry is one-sixteenth of the internal diameter or one-eighth of the radius of the intrados. Divide ab the radius of the center line into any convenient number of equal parts, say eight, at u_1, u_2, etc.: a much larger number would be preferable in actual construction. At the points a_1, a_2, etc., on the same levels with u_1, u_2, etc. pass conical joints normal to the dome, so that b is the vertex of each of the cones.

If we consider a lune between meridian planes making a small angle with each other, the center of gravity of the parts of the lune between the conical joints lie at g_1, g_2, etc. on the horizontal midway between the previous horizontals. These points are not exactly upon the central line aa, but if the number of horizontals is large, the difference is inappreciable. We assume them upon aa. That they fall upon the horizontals through d_1, d_2, etc., midway between those through u_1, u_2, etc., is a consequence of the equality in area between spherical zones of the same height.

In finding the volume of a sphere it may be considered that we take the sum of a series of elementary cones whose bases form the surface of the sphere, and whose height is the radius. Hence, if any equal portions of the surface of a sphere be taken and sectorial solids be formed on them as bases and having their vertices at the center, then the sectorial solids have equal volumes. The lunes of which we treat are equal fractions of such equal solids.

Draw the verticals of the type bg through the centers of gravity g_1, g_2, etc. The weights applied at these points are equal and may be represented by au_1, $u_1u_2 = w_1w_2$, etc. Use a as the pole and w_1w_8 as the weight line; and, beginning at the point f_8, draw the equilibrium polygon c due to the weights.

We have used for pole distance the greatest horizontal thrust which it is possible for any segment of the dome to exert upon the part below it, when the hoop compression extends to 51° 49′ from the crown.

Below the point where the compression vanishes we shall not assume that the bond of the masonry is such that it can resist the hoop tension which is developed. The upper part of the dome will be then carried by the parts of the lunes below this point by their united action as a series of masonry arches standing side by side.

Now it is seen that the curve of equilibrium c, drawn with this assumed horizontal thrust falls within the curve of the lune, which signifies that the dome will not exert so great a thrust as that assumed. By the principle of least resistance, no greater horizontal thrust will be called into action than is necessary to cause the dome to stand, if stability is possible. If a less thrust than that just employed be all that is developed in the dome, then the point where the hoop compression vanishes is not so far as 51° 49′ from the crown, and a longer portion of the lune acts as an arch, than has been supposed by previous writers on this subject,* none of whom, so far as known, have given a correct process for the solution of the problem, although the results arrived at have been somewhat approximately correct.

To ensure stability, the equilibrium curve must be inscribed within the inner third of that part of the meridian section of the lune which is to act as an arch; as appears from the same reasons which were stated in connection with arches of masonry.

And, further, the hoop compression will vanish at that level of the dome where the equilibrium curve, in departing from the crown, first becomes more nearly vertical than the tangent of the meridian section; for above that point the greatest thrust that the dome can exert, cannot be so great as at this point where the thrust of the arch-lune is equal to that of the dome.

Now to determine in what ratio the ordinates of the curve c must be elongated to give those of the curve e which fulfills the required conditions, we draw the line fo, and cut it at p_1, p_2, etc. by the horizontals m_1p_1, m_2p_2, etc., the quantities mb being the ordinates of exterior of the inner third. Again draw verticals through p_1, p_2, etc., and cut them at q_1, q_2, q_3, etc. by horizontals through c_1, c_2, c_3, etc. Through these points draw the curve qq, whose ordinates are of the type qh. Some one of these ordinates is to be elongated to its corresponding ph, and in such a manner that no qh shall

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* See a paper read before the Royal Inst. of British Architects, "on the Mathematical Theory of Domes," Feb. 6th, 1871. By Edmund Beckett Denison, L.L.D., Q.C., F.R.A.S.

then become longer than its corresponding *ph*. To effect this, draw oq_2 tangent to the curve qq; then will oq_2 enable us to effect the required elongation: *e.g.* let the horizontal through c_4 cut oq_2 at j_4, and then the vertical through j_4 cuts *fo* at i_4, then is e_4 (which is on the same level with i_4) the new position of c_4. Similarly, we may find the remaining points of the curve *e*; but it is better to determine the new pole distance, and use this method as a test only.

The curve *qq* made use of in this construction for finding the ratio lines for so elongating the ordinates of the curve *c*, that the new ordinates shall be those of a curve *e* tangent to the exterior line of the inner third, may be applied with equal facility to the construction for the arch of masonry. This furnishes us with a direct method in place of the tentative one employed in connection with Fig. 14.

To find the new pole distance, draw $fj \parallel oq_2$ cutting *ww* at *j*, then will *i* the intersection of the horizontal through *j*, be the new position of the weight line *vv*, having its pole distance from *a* diminished in the required ratio.

The equilibrium curve *e* will be parallel to the curve of the dome at the points where the new weight line *vv* cuts the curve *st*. It should be noticed that the pole distance which we have now determined is still a little too large because the polygon *e* is circumscribed about the true equilibrium curve; and as the polygon has an angle in the limiting curve *mm* the equilibrium curve is not yet high enough to be tangent to the limiting curve. If the number of divisions had originally been larger (which the size of our Figure did not permit) this matter would be rectified.

The polygon *e* is seen at e_5 to fall just without the required limits, this would be partly rectified by slightly decreasing the pole distance as just suggested; the point, however, would still remain just without the limit after the pole distance is decreased, and by so much is the dome unstable. A dome of which the thickness is one fifteenth of the internal diameter, is almost exactly stable.

It is a remarkable fact that a semi-cylindrical arch of uniform thickness and without surcharge must be almost exactly three times as thick, viz., the thickness must be about one fifth the span in order that it may be possible to inscribe the equilibrium curve within the inner third.

The only large hemispherical dome, of which I have the dimensions, which is thick enough to be perfectly stable without extraneous aid such as hoops or ties, is the Gol Goomuz at Beejapore, India. It has an internal diameter of 137½ feet, and a thickness of 10 feet, it being slightly thicker than necessary, but it probably carries a load upon the crown which requires the additional thickness.

The hemispherical dome of uniform thickness is a very faulty arrangement of material. It is only necessary to make the dome so light and thin for 51° 49′ from the crown that it cannot exert so great a horizontal thrust as do the thicker lunes below, to take complete advantage of the real strength of this form of structure. A dome whose thickness gradually decreases toward the crown takes a partial advantage of this, but nothing short of a quite sudden change near this point appears to be completely effective.

The necessary thickness to withstand the hoop compression and the meridian thrust can be found as previously shown in the dome of metal.

Domes are usually crowned with a lantern or pinnacle, whose weight must be first laid off below the pole *a* after having been reduced to the same unit as that of the zones of the dome.

Likewise when there is an eye, at the crown or below, the weight of the material necessary to fill the eye must be subtracted, so that *a* is then to be placed below its present position. The construction is then to be completed in the same manner as in Fig, 18.

It is at once seen that the effect of an additional weight, as of a lantern, at the crown, since it moves the point *a* upward a certain distance, will be to cause the curve *st* to have all its points except *b* to the left of their present position, and especially the points in the upper part of the curve, thus making the point of no hoop tension much nearer the crown than in the metallic dome. It will be noticed that the addition of very small weight at the crown will cause the point m_2 of no hoop tension in the dome of masonry to approach almost to the crown, so that then the lunes will act entirely as stone arches with the exception of a very small segment at the crown.

On the contrary, the removal of a segment at the crown, or the decrease of the thickness, or any device for making the upper part of the dome lighter will remove the point of no hoop tension further from the crown, both for the dome of metal and of masonry. In any dome of masonry the thickness above the point of no hoop tension, as determined by the curve *st*, need be only such as to withstand the two compressions to which it is subjected, viz; hoop compression and meridian compression: while below that the lunes acting as arches must be thick enough to cause a horizontal thrust equal to the maximum radial thrust of the dome above the point of no hoop tension.

Several large domes are constructed of more than one shell, to give increased security to the tall lanterns surmounting them: St. Peter's, at Rome, is double, and the Pantheon, at Paris, is triple. The different shells should all spring from the same thick zone below the point of no hoop tension; and the lunes of this thick zone should be able to afford a horizontal thrust equal to the sum of the radial thrusts of all the shells standing upon it.

Attention to this will secure the stability in itself of any dome of masonry spherical or otherwise; and, though I here offer no proof of the assertion, I am led to believe that this is the solution of the problem of constructing the dome of a minimum weight of material, on the supposition that the meridian joints can afford no resistance to hoop tension.

Now, in fact, it is a common device to ensure the stability of large domes by encircling them with iron hoops or chains, or by embedding ties in the masonry; and this case appears to be of sufficient importance to demand our attention.

If the hoop encircles the dome at 51° 49′ or any other less distance from the crown the dome will be a true dome at all points above the hoop. Suppose the hoop to be at 51° 49′, then the curve *e* should, below that point, be made to pass through the points f_3 and f_8, from which it is seen that the dome may be made thinner than at present, and the horizontal thrust caused will be less. The tension of the hoop would be that due to a radial thrust which is the difference between that given by the curve *st* for this point and the horizontal thrust (pole distance) of the polygon *e* when it passes through f_3 and f_8. That the curve *e* passes through these last mentioned points is a consequence of the principle of least resistance.

Again, suppose another hoop encircles the dome at f_5; the curve *e* must pass through f_8 and f_5, and in this part of the lune will have a corresponding horizontal thrust. The curve *e* must also pass through f_5 and f_3, but in this part of the lune will have a horizontal thrust corresponding to it, differing from that in the part between f_8 and f_5: indeed the horizontal thrust in the segment of a dome above any hoop depends exclusively upon that segment and and is unaffected by the zone below the hoop. The tension sustained by the hoop is, however, due to the radial force, which is the difference of the horizontal thrusts of the zones above and below the hoop.

It is seen that the introduction of a second hoop will still further diminish the thickness of lune necessary to sustain the dome, unless indeed the thickness is required to sustain the meridian compression.

Had a single hoop been introduced at f_5 with none above that point, the dome above f_5 should then be investigated, just as if the springing circle was situated at that point. The curve *e* must then start from f_5, as it before did from f_8, and be made to become tangent to the limiting curve at some point between f_5 and the crown.

By the method here employed for finding the tension of a hoop it is possible to discuss at once the stresses induced in the important modern domes constructed with rings and ribs of metal and having the intermediate panels closed with glass.

On introducing a large number of rings at small distances from each other, it will be seen that the discussion just given leads to the method previously given for the dome of metal.

The dome of St. Paul's, London, is one which has excited much adverse criticism by reason of the novel means employed to overcome the difficulties inherent in so large a dome at so great a height above the foundations of the building. The exterior dome consists of a framework of oak sustained by conical dome of brick which forms the core. There is also a parabolic brick dome under the cone which forms no essential part of the system. Since the conical dome in general presents some peculiarities worthy of notice we will give an investigation of that form of structure as our concluding construction.

CONICAL DOME OF METAL.

In Fig. 19, let *bd* be the axis of the frustum of a metallic cone cut by a vertical plane in the meridian section *a*. The cone is supposed to have a uniform thickness too small to be regarded in comparison with its other dimensions. Suppose the frustum to be cut by a series of equi-distant horizontal planes as at g_1, g_2, etc., into a series of frustra or rings: then the weight of each ring is proportional to its convex surface. The convex surface of any ring $=2\pi r\times$ slant height; when r is half the sum of the radii of the two bases, *i.e.*, r is the mean radius. Consequently, the weights of these rings, or any given fraction of them included between two meridian planes, is proportional to their mean radii. Let us draw these mean radii d_1a_1, d_2a_2, etc., between the horizontals through g_1,g_2, etc., and use some convenient fraction, say $\frac{1}{3}$, of these quantities of the type *da* as the weights. The line *ii* cuts off $\frac{1}{3}$ of each of these: then lay off $du_1=d_1i_1$ as the weight of the ring ag_1, lay off $u_1u_2=d_2i_2$, $u_2u_3=d_3i_3$, etc., as the weights of the rings g_1g_2, g_2g_3, etc.

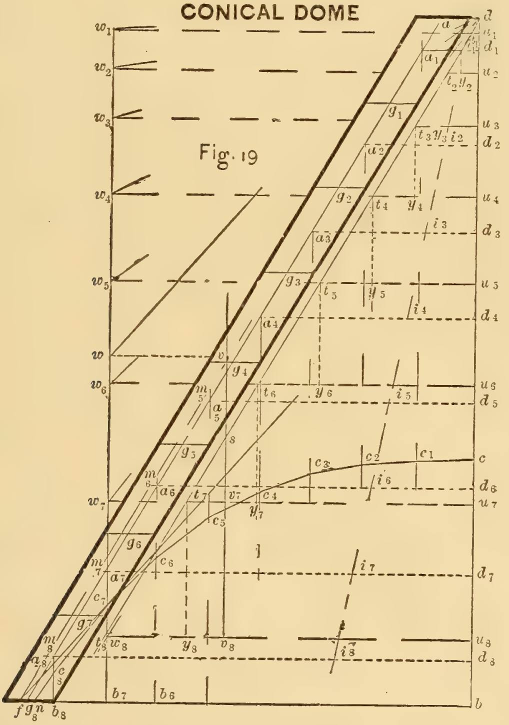

Fig. 19

Draw the line *dt* || *aa*, it corresponds to the curve *st* of Fig. 18; then the quantities of the type *tu* represent the horizontal radial thrust which the cone exerts upon the part below it, while the radial thrust borne by any ring is the difference between two successive quantities of the type *tu*, *i.e.*, the radial thrust in the ring g_7g_8 is represented by t_8y_8, that in g_6g_7 by t_7y_7, etc. As previously shown in connection with the spherical dome, if the scale of weights be such that du_8 represents a part of the cone between two meridian planes which make an angle of $\theta=180°\div\pi=57°.3-$, then will t_8y_8, t_7y_7 etc., be the total hoop compression of the corresponding rings of the cone. It is to be noticed that this quantity does not change sign in the cone, and is always compression.

The meridian compression is expressed, under the same circumstances by the quantities dt_8, dt_7, etc.

Such a cone as this must be placed upon a cylindrical drum or other support which can exert a resistance in the direction aa, but if this support is very slightly displaced by the horizontal radial thrust, a hoop tension will be induced at the base of the cone. As this displacement is very likely to occur it is far better to have the base of the cone sufficiently strong to withstand this tension, which is t_8u_8 when du_8 is the weight of 57°.3 : then the supports will sustain a vertical force alone.

This discussion applies equally well to a cone formed of a network of rings and inclined posts with intermediate panels of glass or other material.

CONICAL DOME OF MASONRY.

Let us assume that the uniform horizontal thickness of the dome to be treated, is one sixteenth of the internal diameter of the base, or one eighth of the internal radius, as shown in Fig. 19. The actual thickness is less than this, but since the horizontal thickness is a convenient quantity, we shall call it the thickness unless otherwise specified.

Pass equidistant horizontal planes as previously stated: then the volumes of these rings may be found by the prismoidal formula. The volume

$$=\tfrac{1}{6}\pi h\,[r_1^2-r_1'^2+4(r^2-r'^2)+r_2^2-r_2'^2],$$

in which h is the height of the ring, r_1 and r_1' are the radii external and internal of one base, r_2 and r_2' of the other, and r and r' of the middle section. Now $r_1-r_1'=r-r'=r_2-r_2'=t$ the thickness of the cone; and

$$r_1+r_2=2r,\ r_1'+r_2'=2r'$$
$$\therefore \text{Volume}=\pi ht(r+r')=2\pi ht\bar{r}$$

when $\frac{1}{2}(r+r')=\bar{r}$ the mean radius of the middle section. From this it is seen that the weights vary in the same manner, and are represented by the same quantities as previously stated in case of a thin cone. Assume that the centers of gravity of any thin lunes cut from these rings by meridian planes making a small angle with each other, are at the middle points a_8, a_7, etc., this assumption is sufficiently exact for the part of the cone near the base, which we are now specially to investigate.

By means of the weights $w_8w_7=u_8u_7$, etc., at some assumed distance from the pole d, describe the equilibrium polygon c, starting from n at the inner third of the base.

Now if the cone stands upon a drum which necessarily exerts a sufficient radial thrust to keep the meridian joints of the cone closed down to the base, then all the circumstances will be precisely as before explained in respect to the metallic dome: but if the drum exerts a less radial thrust, the meridian joints will open near the base, and the conditions of stability of that part of the cone will need to be investigated, as was done in the spherical dome of masonry, by considering the upper part of the dome as sustained by a series of stone arches. From f draw fc_7 tangent to the curve c; then must c_7b_7 be elongated to m_7b_7 and the other ordinates of c must be elongated in the same ratio in order that the equilibrium polygon may be tangent to the exterior limit fm; and, further, fm and fc_7 are the ratio lines by which to effect the elongation. To find how much the thrust is diminished, draw through the intersection of fm with bd, a line parallel to fc_7 intersecting the weight line at w, and then v the point where the horizontal through w intersects fm gives us the new position of the weight line, and its distance from the pole d. This vertical intersects tt about midway between t_6 and t_7, thus showing that the meridian joints of the cone will be open from the base to about the point g_6. It is unnecessary to draw the equilibrium polygon in its new position.

We thus obtain the least horizontal thrust against which the dome can stand. The actual thrust which the drum exerts may have any value greater than this least thrust.

It is seen that the effect of diminishing the thickness of the cone, is to carry the tangent point c_7 and the point of no compression nearer to the base. In other words the thin dome of masonry of given semi-vertical angle necessarily exerts a greater thrust in proportion to its weight than does a thick dome, though that proportion is unchanged if the joints are to remain closed all the way to the base.

All of the circumstances respecting radial thrust above the point of no hoop compression, and respecting meridian thrust, are the same as in the metallic cone.

Any additional loading above that of the weight of the cone itself, as for example, the weight of a lantern, or of an external dome, as in the case of St. Paul's, can be introduced and treated as an additional height or thickness of certain rings of the cone. The same method which has been here applied may be applied to all such cases, if the weights be determined by some suitable process. For example, it may be shown by the help of the prismoidal formula, that the volume of the ring cut from a uniformly tapering cone by equidistant horizontal planes, varies as the product of the mean radius of the mid-section by the thickness at the mid-section.

OTHER VAULTED STRUCTURES.

Similar principles to those above developed apply to domes with an elliptical or polygonal base, to domes whose meridian sections are ogee curves, to Skew Arches, to Groined Arches formed by the combination of cylindrical arches, as well as to Groined Arches which are dome-shaped.

By the application of the principles developed it is easy to treat the cone or dome which sustains the pressure of earth or water. Indeed, it is not too much to say that the complete solution of the problem of the stability of vaulted structures has now been set forth for the first time, and that the proper connection and relationship between similar structures, in metal and masonry, may now be clearly seen. In particular, the discussions have made manifest the applicability of a particular equilibrium polygon among the infinite number which are due to a given set of weights, and which are all projections of any one of them, and the possibility of deriving from it in each of the structures treated, a complete and sufficiently exact solution.

CONCERNING THE ST. LOUIS ARCH.

By C. SHALER SMITH, C.E.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

In the sixth number of Prof. Eddy's series of articles on "New Constructions in Graphical Statics" it is stated, that—"the St. Louis Arch is wanting in initial stiffness to such an extent, that the weight of a single person is sufficient to cause a considerable tremor over an entire span."

This belief, which the writer has found quite common in the East is erroneous in the highest degree. Light teams habitually trot at a good rate of speed over these arches without producing a tremor. Railway trains have less effect than on a stiff truss bridge: military and other processions will produce a tremulous wave not exceeding the motion caused by the same agency on such a span as that in the Newport Bridge at Cincinnati, while the maximum effect of all is produced by the passage of a coal wagon loaded to seven tons, jolting over the rough flooring. This tremor exceeds any that the writer has observed on other structures, excepting the suspension bridges, but as the wave motions produced by jarring loads increase with the length of span, it is very clear that *until a beam truss span of 520 feet is built somewhere for highway purposes, we will have no opportunity of making a valid comparison.*

Theorising on this special subject is useless, as the nearest approach to the peculiar tremor alluded to, is to be found in the case of the 210 feet span *plate girder* over Mill Creek in Cincinnati, in which bridge the floorway is carried through on the line of the neutral axis. (On this point see Du Bois' Graphical Statics, second edition, page 363, paragraph 12).

Prof. Eddy's opinion as to the greatest economy of the flexible arch with continuous stiffening girder would probably be changed in case he were called on to design and proportion the actual structures at 500 feet span, and 50 feet rise. The writer made an exhaustive comparison between these two forms of arch (flexible and braced ribs) in 1870, at the request of Mr. Wm. MacPherson, then President of the St. Louis Bridge Co., and found that, contrary to his previously expressed belief, there was no practical difference between them in weight, while as was subsequently proved, the fact that the braced arch *could* be erected without false work, while the flexible arch could not, placed the latter far in the rear in actual economy of construction for that particular place. The conclusion concerning the St. Louis Arch arrived at by Prof. Du Bois, see pages 407 and 408 Graph. Statics, second edition, is more nearly correct than Prof. Eddy's belief "that the stiff arch rib was a costly mistake," or than Capt. Eads' own opinion as to the relative economy of the braced continuous rib as compared to the rib with hinges.

THE SEWAGE QUESTION.

By C. NORMAN BAZALGETTE, Barrister-at-Law.

From Proceedings of the Institution of Civil Engineers.

I.

The object of this Paper is twofold. First, to limit and define the proper application of the various systems which have from time to time been introduced, as a more or less efficient means of dealing with the sewage of towns. Secondly, to direct attention to certain subordinate questions arising upon the practical operation of these systems.

The Sewage Question has not made much progress, so far as discovery and invention are concerned, during the last quarter of a century, nor does there seem to be much prospect of any new or startling light being thrown upon it in the future. The fact is the field of investigation is nearly exhausted. Earth, air, fire and water have all been resorted to, and the relative merits of processes based upon them are pretty well known, while the resources of chemistry have been ransacked in turn. And yet even now many sanitary authorities stand in an attitude of doubt and expectation, as though, distrustful of the present, they waited for the advent of some miraculous invention which was to supplant all previous doctrine and set the whole question at rest. The causes of such hesitation are doubtless, first, the introduction by empirics of numerous processes which, professing to deal with sewage at a profit, have little or no real value. Secondly, the division among experts themselves, arising from the tendency to study and elaborate special crotchets to the exclusion of a comprehensive form of practice, adapted to the varying conditions and necessities of different localities. Thirdly, the proneness of every inventor to overrate the merits and exaggerate the capacity of his own invention. The most recent and conspicuous illustration of this latter defect may be found in the pretensions of Intermittent Downward Filtration, as put forward by the Rivers Pollution Commissioners.

The following classification of the various systems has been adopted for the purposes of this Paper :

I. Treatment with chemicals.
II. Application of sewage to land, including irrigation and intermittent downward filtration.
III. The dry earth system.
IV. The Liernur or pneumatic system.
V. Seaboard and tidal outfalls.

I. TREATMENT WITH CHEMICALS.

This category may be taken to include all those multifarious processes which, by the admixture of chemicals, have sought the purification of sewage by the precipitation of the dissolved and suspended impurities it contains, and the ultimate realization of the precipitate in the form of manure. It would be beyond the province of this Paper to attempt even the most meagre summary of the various methods which have been suggested for the accomplishment of these objects. More than a century had elapsed since the first experiments were instituted at Paris, with a view to the extraction of the manurial elements of sewage in a compact form; and since that time, more than sixty permutations and combinations of the original idea of treatment by chemicals have seen the light. But although minute criticism of the comparative merits is impossible, it will still be necessary, for the purpose of defining their appropriate application, that some reference should be made to a few of the more conspicuous modern processes.

Before proceeding to their separate consideration, it may be stated that no chemical process can, unaided, effectually deal with sewage, but that its application must be ancillary to some other process, as, for instance, the disposal of sewage upon land. This partial incapacity of chemical reagents is attributable to the well-established fact that, in the adoption of any process, one of two results is inevitable; either purification must be purchased at the cost of insolvency, or solvency must be preserved at the sacrifice of purification; in other words, any chemical process to effect purification must be worked at a loss. For, conceding that even some of the agents employed can produce a fairly satisfactory effluent, there is no instance upon record where such a result has been obtained concurrently with a commercial profit, or even without a positive loss, although there is abundant evidence to the contrary. This failure must be mainly ascribed to the cost of the chemicals; for while to secure purification it is absolutely necessary that they should not be stinted, the only set off against their cost is the return derivable from the sale of the precipitate as manure. Experience has proved that sewage manures are unmarketable; and even assuming that this were not the fact, still, inasmuch as the value of the manure is pretty nearly in a direct ratio to the chemicals introduced into it, it is clear that, making allowance for waste, there must be an inevitable deficit. Apart, however, from this financial objection, there is a formidable practical difficulty attaching to the use of these processes, which up to the present time has proved an almost fatal bar to their efficient operation, namely, the difficulty of dealing with the enormous quantity of sludge developed by the employment of precipitants. From time to time a number of expedients have been suggested (the most recent by General Scott, R.E., Assoc. Inst. C.E.) to reduce this sludge into a manageable and inoffensive form. But hitherto the success attending the application of these expedients has been so partial, and the difficulty has been so insuperable, that in most cases, where treatment by chemicals has been or is practised, a limited manipulation only has been attempted, and the great bulk of the sewage residuum has been allowed to go to waste. Accordingly, even the doubtful return derivable from the sale of manure, when considered as recoupment for the cost of chemicals, must be subjected first of all to a liberal discount in respect of the difficulty of extracting it in a saleable form. Thus there are three urgent objections to the general adoption of any precipitating process as a means of dealing with sewage. First, it is so costly that purification can only be effected at a serious pecuniary loss; secondly, the extreme difficulty of manipulating the sludge and extracting the manure; and, lastly, the unmarketable character of the extract. A conviction of the truth of these conclusions is steadily forcing itself upon the public mind, and the various companies who are working chemical sewage patents begin to find bankruptcy as one alternative, or abandonment of the purification as the other.

As long ago as 1865, Mr. Rawlinson, C.B., M. Inst. C.E., in the Third Report of the Sewage of Towns Commission, epitomised the results gained up to that time, and stated that experience, so far as carried, proved that fluid sewage could not be manipulated into a solid manure so as to pay. It remains to be seen whether the results he cited have been substantially varied by subsequent practice.

1. *The Lime Process* may be considered as the progenitor of most chemical methods of treatment in this country,

having been adopted more than twenty years ago at Leicester. Its comparative efficiency is proved by the fact that its use survives in some of the more conspicuous instances of modern practice. It was included in the condemnation pronounced by Mr. Rawlinson in 1865; and the opinion then expressed has been confirmed by the Rivers Pollution Commissioners in their first report, 1870. They say, referring to the operation of the lime process at Leicester, Tottenham, and Blackburn, "In all these places the plan has been a conspicuous failure, whether as regards the manufacture of valuable manure, or the purification of the offensive liquid;" and again, "the method obviously failed in the purification of the sewage to such an extent as to render it admissible into a river." So far as Leicester is concerned, the accuracy of these statements has been verified by a personal inspection which the Author made of the works in the spring of 1876. The river Soar was then in a state of active pollution, although the most serious effect was observable at a point comparatively remote from the outfall works; but this apparent anomaly was easily accounted for by the fact, that a certain time must elapse before the effluent undergoes complete putrefaction, and it is thus enabled to flow some distance down the stream before arriving at its most offensive condition. About 300 tons of semi-solid sludge were being produced weekly, the estimated value of which was 6*d.* per ton, and yet even at this nominal price it could only command a market among the farmers to the extent of from 400 to 500 tons per annum, or something less than a fortnight's supply, while the remainder had to be got rid of upon 500 acres of drying ground, to the admitted nuisance of the adjacent cottages. The estimated cost for works incurred in respect of this process at Leicester, exclusive of sewerage works, is about £40,000, and the result is a complete failure, both as regards purification and the profitable extraction of a manurial compound. As Leicester is about to abandon the lime process for irrigation, it is unnecessary to dwell further upon the unsatisfactory effect of its adoption there.

At Birmingham the history of the lime process is perhaps more curious, and scarcely less instructive. Previous to 1871, the sewage there was allowed to subside in tanks, before being passed into the rivers Rea and Tame. In that year the Town Council, being alive to the defects of the system, and having an additional stimulus to action in a couple of injunctions obtained against them, appointed a committee to report upon the best means of dealing with the sewage of the town. This committee in the course of the same year presented a valuable and exhaustive report, in which, after describing the *modus operandi* of the lime process, they pass the following opinion upon its merits: "The deposit, in the form of a highly putrescible mud, is taken out, dried, and sold for manure; the process of drying being tedious and offensive, and in winter almost impossible. This process is the simplest and least costly of any, but according to the report of the Rivers Pollution Commissioners, the effluent water, though clarified, is not purified or fit to be admitted into a running stream, since it still contains about one-half of the putrescible organic matter, which is precisely what is most important to keep out. Moreover, as the effluent water still contains most of the valuable constituents, the manure is of little value, and it is difficult, and on a large scale might be impossible, to dispose of it."

Bearing in mind that this stricture on the lime process was pronounced as recently as 1871, it is noteworthy that this process has since been selected and applied by Mr. Hawksley, Past President Inst. C.E., for the purpose of dealing with the sewage of the town. Judging from the information obtained by a personal inspection of the works in the spring of 1876, the Town Council appear to have spared neither pains nor expense to verify the conclusions previously expressed by their Sewage Committee. The population contributing sewage is about 354,000; and, although it is not easy to ascertain the exact expenditure upon land and works at the outfall, yet, approximately, £38,000 must have been paid for 115½ acres of land, then there is the lease of 105 acres, while the cost of the old and new tanks may be calculated at £18,000 and £30,000 respectively, so that the outlay in round numbers has probably exceeded

£100,000. On arrival at the works the sewage is treated daily with about 15 tons of lime. It is next passed through eighteen depositing tanks, in which the accumulation of sewage residuum varies in amount and density in proportion to the distance of the tanks from the sewer outfall. The clarified effluent is then allowed to pass by various outlet sluices into the Rea or the Tame, or is disposed of by irrigation on the Corporation land. The amount of sludge produced daily is about 500 tons, an infinitesimal portion of which is converted into cement by General Scott's process. At the time of the Author's visit only three men were engaged in the manufacture of this cement. This will afford some criterion of the limited quantity of sludge dealt with. The residue of this enormous bulk of sewage matter, with the exception of a few boat loads, sold to the farmers at £1 a boat load, has to be dug into drying beds covering a large acreage, which is being continually extended. But although the operation of digging it into the ground is incessant, the accumulation of sludge is so enormous as to be almost unmanageable, even the large area at the disposal of the Corporation being insufficient for present requirements—the soil, in many parts to a considerable depth, being saturated and sodden with the impurities which it contains. Such a condition of things, in the neighborhood of a populous and important town, cannot be too seriously deprecated, both as tending to prejudice the health of the inhabitants and to depreciate the value of property. The machinery by which the process is applied is costly, ingenious, and elaborate, but the results from a sanitary point of view, except as a temporary expedient, can only be regarded as a calamitous failure.

The Corporation of Birmingham have, it is true, been exceptionally unfortunate in their efforts to deal with the sewage difficulty. In 1872, through an extraordinary exercise of land-owning influence, they lost a Bill, the object of which was an irrigation scheme, by a narrow majority upon third reading, after it had previously received the sanction of a Parliamentary Committee. It is to be hoped that the lime process, as practised at Birmingham, is merely a temporary means, pending the adoption of some more substantial and efficient mode, although the permanent and expensive character of the works rather tends to preclude so sanguine an expectation. But it is not only in the creation of this cumbersome mass of sewage mud that the process fails at Birmingham, for the effluent water, to reiterate the expression of the Sewage Committee, "though clarified, is not purified, or fit to be admitted into a running stream." Indeed, at the time of the Author's visit, some of the Corporation land was actually under irrigation by the effluent, and the attendant remarked naively enough, that for this purpose they preferred the effluent to the raw sewage. In that simple remark lay the confession of the practical inefficiency of the whole system; for it proved that the effluent still contained the putrescible organic matter which while the most fertilising is also the most actively polluting property of sewage, and which, therefore, should be most jealously excluded from the stream. It may perhaps be argued, that the effect of some of these chemical processes is not only to purify the sewage, but to give to the effluent water a manurial principle non-polluting in itself. This, however, is not the case, at all events with the lime process, for the fertilising power of the effluent is not due to any innocuous manurial principle which is added, but rather to the presence of the nitrogenous organic matter which it has failed to abstract. This view is confirmed by the Rivers Pollution Commissioners in their first report, where in speaking of the lime and Sillar's processes, they say: "The material, however, which it is of the greatest importance to remove from the dissolved constituents of sewage, is nitrogenous organic matter, because it is this kind of organic matter which enters rapidly into putrefaction, and becomes an active agent in the pollution of rivers. This material is represented in the analytical results by organic nitrogen. It is precisely here that both processes signally fail (although the lime process is slightly superior to the other) in accomplishing such an amount of purification as would render the sewage admissible into an open watercourse."

The lime process must therefore be taken to have failed at Birmingham, as at Leicester, and if it has failed at Birmingham it will never succeed elsewhere, for all that experience can suggest or skill effect, in providing appropriate mechanical apparatus has been accomplished there. Yet it has failed in the first requirement of any process, namely, to produce a sufficient purification of the treated sewage. Moreover, an enormous amount of comparatively worthless sewage mud is being continually accumulated, the disposal of which is both difficult and offensive.

2. *The A B C, or Sillar's Process* (as worked by the Native Guano Company) claims a Mosaical origin, but it has been more sensational than successful in its public career.

The Rivers Pollution Commissioners, in their first report, placed the A B C process, as a purifier of sewage, upon a par with the lime process, but condemned both as failing to purify to such an extent as to render sewage admissible into running waters. In a subsequent Report of 1870, after detailing their investigations, extending over a period of nearly two years, the Commissioners present certain summarised conclusions which may be shortly paraphrased as follows:—1. On no occasion has the purification been sufficiently complete to render the effluent admissible into running water: 2. The effluent is little better than what can be obtained by mere subsidence: 3. The manure has a low market value, and cannot repay the cost of production: 4. The manipulations incident to the process are nauseous and offensive.

These conclusions are based upon irrefragable evidence, which is set out in the Report. It may be instructive to extract an answer from the evidence of Dr. Odling, who was examined as an independent witness before the Commissioners, and whose evidence is appended to their Report: *Q.* 51, (by Dr. Frankland), "Looking at the whole result of the experiments, do you consider that this is a process to be recommended for the purification of town sewage?—*A.* Certainly not. No doubt this method, like all other methods of precipitation, does keep out a considerable proportion of filth from the river, but there was a great deal of putrescible matter in the effluent liquid, and in comparing this mode of precipitation with others, it did not seem to me that its alleged superiority had any foundation. As regards its superior defecation of sewage, and the high value of the product yielded, I came to the conclusion that it was simply a juggle."

To the sweeping condemnation expressed in this report, one invariable plea has been put forward by the promoters of the process, to the effect that, upon each occasion when the test experiments were made, there was some abnormal cause in operation, which unfortunately disturbed the accuracy of the results obtained, and therefore precluded their adoption as a basis upon which a judgment could be formed as to its general merits and capabilities. But the Commissioners, in the same Report, referring to the failure of the process, completely dispose of this allegation, for they say, "And if this failure be charged upon any alleged unfairness in the condition under which the experiments took place at Leicester and at Leamington, it must be pointed out in reply, that when the experiment was tried in the laboratory under circumstances which excluded all possible sources of error, the same result was still more unmistakable."

But a still more practical answer was soon afforded. On the 29th of January, 1871, the sanction of the Metropolitan Board of Works was obtained by the Native Guano Company to a proposal that experiments should be instituted at Crossness, in order to illustrate the efficacy of the A B C process. On the 11th of January 1873, or two years subsequently, Sir Joseph Bazalgette, C.B., M. Inst. C.E., the Engineer, and Mr. Keates the Chemist, of the Metropolitan Board, respectively, presented reports upon the results observed and recorded during that period, the gist of which may be shortly stated. The Engineer reported that the cost of producing a manure amounted to £6 6*s.* 4*d.* per ton. The Chemist put the net value of the manure at 20*s.* a ton, but reported "That the effluent water was, on the whole, very good. The A B C treatment so far clarified and defecated the sewage that, looking solely to the physical condition and chemical composition of the water produced at Crossness, I am of opinion that such water was in a fit state to be admitted into any ordinary river without producing a dangerous degree of pollution. I must here again direct attention, however, to the extremely dilute state of the sewage during the experiment. The effect of this was to render the results somewhat inconclusive, as it is, of course, impossible to say, at least from this experiment, how far the A B C treatment would defecate sewage of a stronger character." So that, in fact, such a "very good" effluent was obtained from "extremely dilute" sewage at a dead loss of £5 6*s.* 4*d.* per ton of manure, or at such a price as effectually forbade the general adoption of such mode of treatment. The effect of the Crossness experiments was to afford a forcible illustration of the proposition, that purification can only be purchased at the sacrifice of solvency. It is true Mr. Rawson, the General Manager of the Native Guano Company asserted in discussion last year upon the reading of Mr. Shelford's Paper at the Institution, that the Company having proved the efficacy of the process as a purifier, would, if they had been given time, have also proved its value as a source of manurial profit. But, even assuming that the Company could produce a paying manure, which is more than doubtful, they could never do so concurrently with the production of a purified effluent. The two results of profit, or even solvency, and purification, are inconsistent and incompatible with each other. First, there is a bankrupt expenditure upon chemicals, and the sewage is purified, then money runs short, chemicals are stinted to save their cost, and the sewage returns to its native impurity. Or, it may be for a time, perhaps, that a sort of intermediate course is adopted, which, while aiming at economy of chemicals, at the same time attempts a partial purification; but this can only be a postponement of the inevitable result, for just as surely as the margin of indebtedness becomes reduced, so the margin of impurity is increased.

This proposition receives an apt illustration in the quasi-defence set up by Mr. Shelford, on behalf of the A B C system, in his Paper already referred to. Starting with the statement that the effluent was extremely good, he attributed the financial failure of the process at Crossness to the extravagant use of chemicals there as compared with Leamington, namely, 31.8 lbs. per 1,000 gallons at the former, as compared with 1.86 lbs. per 1,000 gallons at the latter place. That is to say, that when, as at Crossness, the admixture of chemicals was ruinously extravagant, the quality of an effluent derived from dilute sewage was very good; but when, as at Leamington, the chemicals were sparingly apportioned, the quality of the effluent was notoriously bad, although even under the latter state of things it does not appear that the Company were enabled to show any commercial profit.

The experience also at Hastings, Southampton, Bolton, and Leeds, all tends to point to the same conclusions. Great things were expected, and have been asserted, of the operation of the process at Leeds; but on account of the impossibility of obtaining a sale of the manure, the contract between the Corporation and the Native Guano Company has been annulled. After two or three years intermittent experience of the A B C system, the Leeds Corporation have discovered that the process can purify, but has not paid, and it has therefore been abandoned by mutual consent.

Before finally dismissing this process, the value of the manures produced by it remains to be considered. Some years ago Dr. Voelcker, in the Journal of the Royal Agricultural Society, thus stated the results of his analyses of five samples:

	£	*s.*	*d.*	
No. 1 sample was worth	0	18	6	per ton.
" 2 " "	1	13	6	"
" 3 " "	0	14	0	"
" 4 " "	0	18	6	"
" 5 " "	0	14	6	"

Basing the value on a comparison of the manure with phosphate of lime at £10 a ton, and ammonia at £60 a ton, Mr. Keates, the Chemist of the Metropolitan Board of Works, put the value, as has been already observed, at 20*s.*, so that he and Dr. Voelcker are practically agreed in their valuations. These, then, may be taken to be the theoretical values of the manure per ton. But in speculating upon the probability of profit, it is not the theoretical but the market value which must be considered. This fact was well put by the Chairman of the Council of the Society of Arts in his inaugural address on the 15th of November, 1876, when alluding to the conference held in May last on the health and sewage of towns. He remarked that exaggerated notions had been dispelled by experience, and the public were beginning to learn that, in the disposal of sewage, profit must not be looked for; and that at Leeds, where perhaps more experience had been gained on an extended scale than at any other town, the difficulty had been to dispose of the solid precipitate, arrangements having recently been made with a contractor to purchase it and take it away at 12*s.* a ton, a sum wholly inadequate to defray the cost of treatment.

Enough has been said to illustrate the limits of the capacity of the A B C process, although the results which have been stated could be fortified by much additional evidence.

3. *Sulphate of Alumina and Milk of Lime* (as worked by the General Sewage Manure Company, Limited).—The following particulars of this process as practised at Coventry, obtained at a personal inspection of the works last year, will sufficiently indicate the limits of its ability to purify sewage and produce a profitable manure. The total quantity of sludge precipitated daily is twenty-two tons, of which eleven tons, or one-half only is dried by Milburn's apparatus, the remaining half being deposited in a semi-solid state upon the adjacent land, or occasionally disposed of in small quantities at 4*s.* a load, or given away to the farmers. The drying grounds were offensive in their character, and the effluent which passed from the tanks into the river Sherburne was insufficiently purified, on account of the sparing admixture of chemicals, the Company being compelled to keep them down in order to avoid the cost contingent on their use. It was stated indeed that the manurial product was in some cases worth as much as £15 a ton, but that, in order to obtain this extreme value, it had to be previously fortified. In other words, the money had first to be put into the sewage, and then taken out again minus the waste.

4. *Phosphate of Alumina* (as practised by the Phosphate Sewage Company, Limited).—This process, the subject of a patent by Messrs. David Forbes and Astley Paston Price, professes to accomplish the defecation and utilization of sewage, either by precipitation and irrigation, or by means of precipitation only. With regard to its ability to produce a satisfactory effluent, the following analysis of the effluent discharged during certain trials at Tottenham, the results being stated in grains per imperial gallon, shows that it does not attain the degree of purity considered necessary by the Rivers Pollution Commissioners:

Organic matter:		
In solution	5.74	
In suspension	none	
		5.74
Mineral matter:		
In solution	57.71	
In suspension	none	
		57.71
Total organic and mineral matter		63.45
Organic nitrogen:		
In solution	0.47	
In suspension	none	
Total organic nitrogen		0.47
Equal to ammonia	0.57	
Saline ammonia	3.32	
Total nitrogen calculated as ammonia	3.89	

It is difficult to ascertain to what extent this Company professes to deal with sewage apart from irrigation. After having been the subject of experiment at Tottenham in 1871, when the foregoing analysis was obtained, the phosphate process was subsequently, in 1873, applied to the treatment of a small portion of the metropolitan sewage, previously to its being passed upon the Lodge Farm at Barking. When the works were visited by the shareholders and others, after having been in operation for seven months, favourable opinions with regard to the process appeared in most of the leading metropolitan Journals, which were afterwards collected and published in pamphlet form by the Phosphate Sewage Company. The *Times*, December 18th, 1873, thus interprets the aims of the Company: "What does this Company profess to accomplish? Certainly not to arrest all or even the chief portion of the valuable fertilising constituents held in solution in town sewage; indeed, according to Dr. Voelcker's analysis, the effluent water after the Company have done with it, is quite as rich in manurial matter as it was before. Hence this process does not claim to arrest and collect in a dry, portable form the wealth of organic matter which our cities are now wasting and the land is crying for; though, like some inventions, it does profess to facilitate the application of the liquid in irrigation farming by removing the suspended solid matters which have been found a terrible and, in some cases, an unwholesome nuisance."

This only claims for the process, that it prepares the sewage so as to render the effluent suitable for subsequent distribution upon land; but the preface to the pamphlet goes much further, and asserts its ability to deal independently with sewage, to produce a purified effluent without need of subsequent treatment. An inconsistency of statement seemingly underlies not only the claims put forward by the advocates of these processes, but even the opinions recorded by the chemical experts, namely, that the effluent is equally fit to be passed directly to the stream or to the land.

For instance, Dr. Voelcker, speaking of the clarified sewage of this process, observes, "It is free from any perceptibly disagreeable odour and may be safely run into a watercourse; and having lost none of its mineral fertilising matters, and become slightly richer in saline ammonia, while it has been freed from suspended matters, which greatly interfere with sewage irrigation, the effluent sewage is more valuable, bulk for bulk, for irrigation purposes than the raw liquid."

But if it be true, from the chemical standpoint, that the most fertilising elements of sewage are at the same time the most putrescible in their character—always excepting the ready-formed ammonia retained in the effluent, which is unimportant as regards pollution—how can sewage consistently be described as equally fit for the land or the stream? These propositions appear to be incompatible, and certainly are not reconcilable with the chemical opinions which will shortly be referred to.

However, to recur to the practical history of the process. It was tried in combination with irrigation at the Lodge Farm, Barking, but had to be given up upon the ground of expense.

With regard to its experience at Hertford, where it superseded the lime process, Captain Flower, Engineer to the Lee Conservancy Board, referring to its introduction there, says in a Paper presented to the Conference at the Society of Arts last year, "Since then I have received fewer complaints, but the same remark which I made just now as to the necessity of filtering the effluent produced by chemical process applies here. It is true that it may be said filtration is carried on at Hertford, inasmuch as small coke filters are used, but I think the area is too limited. Here again the sewage is much diluted by infiltration of subsoil water."

This testimony can hardly be considered favorable, so far as the purifying power of the phosphate process is concerned, and no details of its financial results have come to the knowledge of the Author. Suffice it to say, that when the Corporation of Leeds abandoned the A B C process, the Committee advertised, inviting any one who had a better system to propose to test it at the trial tanks, and that amongst others the Phosphate Sewage Company responded to this request, but the process was not tested, the Company either not foreseeing a successful issue, or being unwilling to bear the cost of a trial.

The report of Professor Tanner, the Chemist of the Phosphate Sewage Company, seems to dispose of any claims put forward as to its ability to deal independently with sewage apart from filtration or application to land. Referring to its purifying power, he says it can effect "the production of an effluent water which may be applied direct to land, and is valuable for irrigation purposes; or the effluent, if passed through filter beds of limited area, can be so completely purified, as to be capable of being discharged into any river or stream without causing a nuisance." So while it is stated that the effect of precipitation *per se* is to produce an effluent fit for irrigation, it is also conceded that, if it is to be passed into a stream, a subsequent filtration is necessary for the purpose of preventing a nuisance.

5. *Goodall's Process* (The Clarifying and Utilization of Sewage Company,

Limited) failed at Leeds. It professed to deal with sewage at a cost of 7*s.* 6*d.* per 1,000,000 gallons, but incurred a cost of £4 6*s.* 6*d.* at the experimental tanks, reduced to £2 13*s.* 4*d.* at the larger works.

6. *Bird's Process* (Bird's Sewage Company), applied at Cheltenham and Stroud, was reported unfavorably of by the Rivers Pollution Commissioners with regard to its application at the latter place.

7. *Dugald Campbell's Patent*, 1872.—This process, as well as Whitthread's process, having been brought before the notice of the Institution in the course of last year by Mr. Shelford, M. Inst. C.E., and spoken of in favorable terms as dealing *per se* with sewage requires to be briefly mentioned. The model works at Battersea were the scene of its practical operation, the agents employed being lime and superphosphate, and the sewage treated at all times of a weak description. First as regards the quality of the effluent. This must have been unsatisfactory, for Mr. Shelford observes that "it was more suitable for irrigation than raw sewage." If so it was plainly unfit to be admitted into running water. The process, even when operating upon sewage at all times weak, failed to effect a proper degree of purification. Secondly, as regards the cost of producing so unsatisfactory an effluent, Mr. Shelford's estimate is as follows :

	£.	*s.*	*d.*
Cost of superphosphate and lime per ton of manure......................	2	13	4
Cost of manufacture..............	1	8	10
Total cost per ton.........	4	2	2
Value per ton.............	4	7	2
Manurial profit........	0	5	0

This shows a profit of 5*s.* a ton; but on some occasions the manure yielded a value of only £3 per ton, when of course this slight profit was converted into a loss. From these figures Mr. Shelford deduced the inference that Dugald Campbell's process may be made to pay its own expenses, leaving the public to bear the expense of labor and drying, or £1 8*s.* per ton of manure produced. In order to appreciate this view, it should be applied upon a working scale. Thus, at Leamington the manurial product was put down at 5 tons a day, which, at £1 8*s.* per ton, represents an expense to the public of £7 a day, or £2,555 a year; or, capitalising at slightly more than 5 per cent., a permanent charge upon a capital of £50,000. And this expense, it must be borne in mind, is for the working of a process which, after treatment of weak sewage, produces an effluent more suitable for irrigation than raw sewage.

8. *Whitthread's Process, patented* 1872, was tested at the Tottenham Sewage Works, the agents being dicalcic and monocalcic phosphate, and a little milk of lime. As regards the quality of the effluent, Mr. Shelford observed that it bore out the favorable opinion expressed by Mr. Hope. But Mr. Hope's opinion, as recorded at a meeting of the Social Science Congress of the same year, scarcely appears to be so unqualified in its praise, for he says "although of course it cannot extract the ammonia in solution, it does remove altogether the other forms of 'organic nitrogen' in solution"; and again, that it "to some extent purifies." It is unnecessary to consider the question of cost, for the Company working the patent failed, and though a report of Colonel Cox has been cited in its favor, it was finally abandoned at Tottenham for Hille's process.

There would be no advantage in multiplying evidence beyond the stage which has now been reached, for the practical experience of any of the many chemical processes—and their name is legion—all tends to point the conclusion, namely, the incompetency of any method of chemical treatment to deal independently with sewage. In this respect, what is true of the processes specifically mentioned, is also true of many others which, on account of their number, cannot be referred to; as, for instance, Holden's, Hille's, Lenk's, Suvern's and Scott's.

The dicta of three eminent authorities will fittingly close this section of the Paper.

Frankland (Rivers Pollution Commissioners, first report, p. 52). "The operations of the chemists have, therefore, been directed chiefly to the soluble constituents of sewage; and have had for their object, either the precipitation in a solid form of the valuable, but offensive, ingredients so as to convert them into portable manure, or, secondly, the rendering them inoffensive by the action of disinfectants. Although these operations have not been altogether unsuccessful, they have hitherto entirely failed in purifying average sewage to such an extent as to render it admissible into running water. We have formed this opinion both from observation of the polluting effect of such chemically purified sewage upon the streams into which it was admitted, and from the amount of putrescible organic matter revealed by the chemical analysis of the sewage after treatment."

Krepp, on "The sewage question," 1867, p. 36. "These instances are enough to prove that solids extracted from fluid sewage cannot be manufactured into dry manure, so as to pay for the trouble. Apart from this, chemical analyses, most carefully conducted, of the results of various processes of deodorisation prove, that none of them have thus far succeeded in precipitating or retaining fertilising parts held in solution, the only substances precipitated being those which are held in suspense, and which would just as well have been retained by the mere mechanical process of filtering."

Corfield, "The treatment and Utilization of Sewage," 2nd edition, p. 522. "All these precipitation processes do, then, to a certain extent, purify the sewage and prevent the pollution of rivers, chiefly by removing the suspended matters from the sewage; but they all leave a very large amount of putrescible matter in the effluent water, and at least all the ammonia contained in the sewage (sometimes they add to it); the greater part of the phosphoric acid is precipitated by some of them, while they increase the hardness of the river water, a matter of great importance if the stream be a small one.

"The manures that they produce are in every case very inferior, as may be expected from the known value of the sewage constituents that can be precipitated. They have all failed in producing valuable manure, because the valuable constituent of sewage *par excellence* is the ammonia, which, of course, invariable totally escapes in the effluent water, and is lost to the manure: this shows the futility of all attempts to utilise sewage by precipitation alone."

Surely this concurrence of opinion, coupled with the results derived from practice, justifies the following limitations of the province of chemistry with regard to the Sewage Question.

1. That no chemical treatment can of its own independent ability deal permanently with sewage upon a practical scale, for these reasons :

   (*a*) Because it either fails to effect purification, or
   (*b*) Because purification is purchased at such a cost as to render the adoption of the process impracticable, or
   (*c*) Because it is impossible to manipulate the enormous accumulations of sludge necessarily incident to treatment by chemicals.

2. That chemical treatment has therefore no province as an independent means of treating sewage, and its adoption must be ancillary to the adoption of some other process.

3. That the province of chemistry must be limited to the treatment of sewage in combination with subsequent natural or artificial filtration of the effluent.

4. That a chemical process may be employed with advantage in the case of irrigation or downward filtration, either, when contiguity to human habitation, or limited surface area, requires a more efficient screening of the raw sewage, before it is passed upon the land, than can be effected by mere subsidence or mechanical extraction. In these cases, however, as in every other where chemicals are introduced, the accumulation of sludge is an objection inseparable from their use.

The Author thinks these views must be recognized at last, as he cannot suppose that the state of things disclosed in the most modern American sanitary textbook, published in January of last year, by the State Board of Massachusetts, will continue to hold good in England. Referring to the numberless array of chemical processes from time to time paraded before the notice of the public, it says (p. 333), "This list might be extended almost indefinitely, and seems to be limited only by the bounds of human credulity. In France and Gemany the precipitating processes have been given up as inefficient. In England a new 'successful' patent process is hawked about every few months, to be soon found only an addition to the list of failures; and the public is perfectly bewildered by the maze of conflicting statements and propositions."

It is to be hoped that the example of France and Germany may be soon followed in this country.

II. THE APPLICATION OF SEWAGE TO LAND, INCLUDING (1) IRRIGATION, AND (2) INTERMITTENT DOWNWARD FILTRATION.

Irrigation.—It will readily be acknowledged that, where land for the purpose can be obtained at a reasonable cost, the practice of broad irrigation presents at once the most efficient and the most satisfactory means of dealing with the sewage of towns. But though the merits and defects of broad irrigation are now generally understood, there are two points which have recently excited the greatest difference of opinion:

(1) Whether a profit can be derived from it; and

(2) What proportion population should bear to acreage.

With regard to the first point, there is abundant evidence, but of a conflicting character.

Thus, from the Parliamentary Return of 1873, it appears that out of twenty-five places practising irrigation, only two professed to show a profit. Swindon exhibited a profit of £12; but Warwick, the large profit of £1,310. With a view of ascertaining what exceptional circumstances were in operation to take Warwick out of the general category, the Author visited Warwick in the course of last year, when the following facts were obtained. The farm was worked up to Michaelmas 1875 by the Town Council, and was then abandoned because it had proven to be a dead loss. It has since been taken by a limited company, composed of members of the Town Council, and no statistics can be had as to its subsequent financial results. The population is 11,000; the area of the sewage farm, 133 acres; the rent paid, £3 10*s.* an acre; the sewage is very weak; the land is clay; rough surface irrigation is practised. Vegetables are grown, but there is no sale for them; and to improve the financial position, a dairy farm was just being started. The effluent goes to the Avon.

The Conference of the Society of Arts last May on the health and sewage of towns was supplied with numerous returns as to the various methods of treating sewage. It had the additional advantage of a Paper by Mr. W. Hope, V.C., as to the profit derivable from sewage agriculture, and similar communications referring to the Beddington and Wrexham farms from Dr. Carpenter and Colonel Jones, V.C., Assoc. Inst. C.E. Consequently the judgment pronounced by the Conference upon this point may be taken as based upon a considerable accumulation of the most recent evidence. It is to the following effect: "It is essential, however, to bear in mind that a profit should not be looked for by the locality establishing the sewage farm and only a moderate one by the farmer." The Author adopts this view; but, before leaving the question of the agricultural value of sewage, he would wish to invite serious consideration to the question of the real value of sewage, as compared with water, for the purposes of irrigation. The use of water as an irrigant is well known, and its effect as a fructifying agent upon crops of a certain kind well established. Can it be shown that sewage produces any appreciable effect, beyond that which might be obtained by irrigation with an equal quantity of water? The kind of vegetable growth produced most successfully upon sewage farms is just that which might be expected to thrive upon water irrigation; succulent roots, grasses, the cabbage tribe, and plants absorbing into their system a large amount of water, are those which flourish best, and the effect upon plants requiring a higher form of nourishment is by many considered doubtful. The measure of nutriment in sewage is the nitrogen it contains, and this exists in the ammonia ready-formed by decomposition which has advanced more or less; and in the organic compounds containing nitrogen may appear as ammonia as the result of subsequent decomposition. These are the pabula of the plant, so far as that particular kind of nourishment is concerned; but is it certain that the nitrogen applied to the land in sewage is actually taken up by the growing plants? Does such nitrogen form indeed the active nourishing principle of the sewage so far as the vegetation grown upon the sewaged area is concerned? Upon these points it appears that uncertainty exists, even if evidence be not forthcoming to show that the nitrogen does really not produce the effect ascribed to it, and that sewage, therefore, as an irrigant, possesses little or no real value beyond water for agricultural purposes.

In a report upon the results of the use of sewage at the Barking farm, by Mr. Morgan, dated September, 1871, there appear two analyses by Dr. Frankland; one of the sewage employed on the farm, and the other of the effluent water draining away from the farm.

RESULTS OF ANALYSIS EXPRESSED IN PARTS PER 100,000.

Description.	Total Solid Matters in Solution.	Organic Carbon.	Organic Nitrogen.	Ammonia.	Nitrogen as Nitrate and Nitrites.	Total combined Nitrogen.	Chlorine.
Barking.							
Sewage from wooden carriers on E., 10.30 A.M., Sept. 5th, 1871	96.1	2.583	1.752	5.450	..	6.240	22.1
Effluent water from main outfall on F., 1.30 P.M., Sept. 5th, 1871	91.3	0.676	0.198	0.005	4.143	4.345	13.4

Description.	Hardness.			Remarks.
	Temporary.	Permanent.	Total.	
Barking.				
Sewage from wooden carriers on E., 10.30 A.M., Sept. 5th, 1871	..	..	..	Suspended matter: Mineral. 5.24 | Organic. 5.60 | Total. 10.84
Effluent water from main outfall on F , 1.30 P.M., Sept. 5th, 1871	7	39.3	40.0	Few suspended parts.

These analyses present this remarkable fact that, whereas the sewage contains 6.24 of combined nitrogen, the effluent water contains no less than 4.34 of the same ingredient, or more than 70 per cent. of the whole quantity. In considering what amount of the remaining 30 per cent. can be credited to the fructification of the crop, it must be remembered that by far the greater part of this nitrogen was in the form of ammonia—really carbonate of ammonia, a volatile salt—and it can hardly, therefore, be believed that some portion of this was not dispersed into the atmosphere; so that, in reality, much less nitrogen than is shown by these analysis would actually find its way to assimilation with the plant. Whether or not experience bears out this view, it practically raises the questions : Is sewage irrigation anything more than water irrigation; and does sewage, as applied to land, possess any special monetary value? If not, it goes far to explain the financial difficulty which has attached to the working of sewage farms, for it shows that the money paid for the use of sewage as a fertilising agent has been thrown away, and that water, if employed in its stead for the cultivation of crops, would have proved nearly, if not quite, as efficacious.

The proportion of population to acreage is a question upon which, although it has formed the subject of much discussion, it is impossible to do more than found a very general conclusion. In fact the proposition is clearly governed by such a variety of circumstances, as character of soil, depth of natural drainage, contiguity to population, strength of sewage, &c., that it would be folly to attempt to set up any definite standard. All that can be done therefore, is to indicate what would be a safe proportion under ordinary conditions, by taking the proportions adopted in various cases, and finding a mean. For example, taking the eleven towns referred to in the Report of the Birmingham Sewage Committee, where the relative proportions of population and acreage are stated, and striking an average between them, a mean result is obtained of 103 persons to an acre. Perhaps, bearing in mind the two things required of broad irrigation, namely, sufficient purification and agricultural return, 100 persons to an acre, the soil being favorable, is a safe proportion for general practice. At the same time if the eleven instances referred to, and which have yielded the above standard, be analytically examined, it will appear how impossible it is to lay down a universal rule. At Bury, with a population of 15,000, the proportion runs as high as 577 persons to an acre, and there is absolutely no effluent; while at Banbury, population 11,500, it is 90 to the acre. At Warwick, population 11,000, 81 to the acre, and at Romford, population 7,000, only 58 to the acre. The explanation of the enormous difference in the proportion adopted in various localities depends no doubt to some extent on the porosity or retentiveness of the soil, but much more upon the depth of soil which remains clear above the level of the subsoil water. For even in broad irrigation it is not merely the surface contact of the sewage with the soil, assisted by the oxidising influence of vegetation, which conduces to the resolution of sewage into its innocuous elements, but above all, its passage through that aerated earth filter which intervenes between the surface and the subsoil water. This is the principle upon which Intermittent Downward Filtration claims to proceed, the capacity of which will now be considered.

2. *Intermittent Downward Filtration* is a new term coined to express a very old meaning, and which claims as a novelty a very old principle. There is nothing in Intermittent Downward Filtration, so far as principle is concerned, which did not exist before the so-called invention came forth from the laboratory of the Rivers Pollution Commissioners. Intermittent Downward Filtration is merely the artificial production of what is frequently found in nature, and its only claim to novelty lies in the fact, that though found in some instances, it does not exist in all. There is nothing in Intermittent Downward Filtration which is not sometimes common to irrigation; in fact irrigation sometimes is Intermittent Downward Filtration, in the strongest sense in which the latter system is explained by its advocates. To illustrate this: The theory of those under whose patronage Intermittent Downward Filtration has been introduced as a new system is shortly this, that if deep drainage, say to the depth of six feet, be adopted, the proportion of population to acreage may be enormously increased, and, consequently, the surface area correspondingly diminished. But this is the same old principle which accounts for the varying proportions in the practice of irrigation. Take a professed example of Intermittent Downward Filtration, namely, Merthyr Tydvil, and one of irrigation, namely, the Eton Sewage Farm. Where is the difference which constitutes irrigation in one case, and Downward Filtration in the other, save that the conditions at Eton are those of nature and at Merthyr they are imitations of nature? At Merthyr there is artificial drainage to the depth of six feet, at Eton there is natural drainage to the depth of eight feet, so that, as far as deep drainage goes, irrigation has the best of it, and drainage therefore to a depth of six feet can scarcely be claimed as a startling innovation. At Merthyr Tydvil the filter is divided into plots, and the sewage is applied intermittently to each plot. At Eton the sewage is turned on to one portion of the farm for a while, and then diverted and poured upon another section. The Eton Sewage Farm fulfills all the

conditions of Intermittent Downward Filtration, and yet it is called irrigation. But it is not the name that Intermittent Downward Filtration bears which calls for any serious comment; it is rather the careless and erroneous pretensions asserted with regard to its capacity which really demand serious criticism and investigation. Certain standards have been persistently put forward as indicating the capacity of the new system, never justified in the extent to which they go, either by the experiments out of which the new system was evolved, or by the subsequent experience of its practical operation.

To make the bearing of the argument plain, by which it is proposed to establish the fallacious character given to Intermittent Downward Filtration, it will be advisable to state the points to which attention is sought to be particularly directed. First, it may be premised that when reference is made to the standards set up by the Rivers Pollution Commissioners, it is not to be inferred from the use of the word "standard" that the Commissioners have actually committed themselves to the publication of any absolute rule. What is intended to be conveyed is that they have stated certain authoritative opinions, as to the proportion population should bear to acreage, first of all with certain reservations, but afterwards in such dogmatic terms that the record of such opinions has become little removed in its character from the promulgation of a definite standard. In the first instance the proportion was variously stated at from 2,000 to 3,300 persons to an acre, with certain reservations as to its effects pending the application of the experiments on a practical scale.

It was afterwards claimed that such a test had been afforded by the experience of the system at Merthyr Tydvil, and all reservations were consequently withdrawn. Accordingly, it is proposed to show first, that the proportions stated by the Commissioners in their reports were not justified by the experiments upon which they are based; secondly, to prove that the experience of Merthyr, instead of confirming, entirely refuted the accuracy of the suggested standards; and thirdly, to establish the fact, that in no case does experience warrant the permanent adoption of a larger proportion than 500 or 600 persons to the acre.

It appears from the First Report of the Rivers Pollution Commissioners, that a number of experiments were instituted in their laboratory by Dr. Frankland, extending over the years 1868–1869, for the purpose of determining the effect of the downward filtration of sewage through various soils, of which the following short summary will suffice:—Satisfactory purification of London sewage can be effected through fifteen feet of sand and chalk, at the rate of 5.6 gallons of sewage per cubic yard in twenty-four hours; through Beddington soil, at the rate of 7.6 gallons; through Dursley soil, at the rate of 9.9 gallons; through Hambrook soil, barely at the rate of 4.4 gallons; while through Barking soil, purification was not effected at the rate of 3.8 gallons; and Leyland peat might be educated into effecting purification at the rate of four gallons.

Before examining these results with a view to found a logical conclusion upon them, it is submitted that whatever the mathematical mean may prove to be, it is still too high, looking to the fact that it is derived from laboratory experiments. A certain margin must be allowed for the well-known unreliability of such experiments, as affording an absolute criterion of what will occur in practice. Such an element of unreliability must be a necessary incident of all laboratory experiments, for the experiment is freed from those disturbing influences which attach to a similar operation on a large scale, exposed to the ordinary conditions of nature. The chemist who conducts the experiment adjusts everything with a nicety of methodical arrangement which must conduce to the most favorable results. The earth is carefully packed layer upon layer in the glass cylinder, and that irregularity of composition is avoided which almost invariably occurs in the soil forming the natural filter bed. The chemicals are weighed out in scales, and allowed to mingle with the sewage in accurate proportion, and thus that rough-and-ready admixture which distinguishes the practical application is entirely excluded from the experiment. The sewage itself is probably collected by means of a bucket from the top waters of a sewer, and so those heavier particles are avoided, which, gravitating downwards, pass along the invert, and which distributed upon the land tend to clog the pores of the natural filter. Then again, there is no rainfall to impede filtration, no sunshine to develop noxious exhalations, and no wind to waft such odors in the direction of contiguous habitations. Freed from every disturbing influence, the experiment, proceeding in the uninterrupted quiet of the laboratory, necessarily yields the most favorable results. Laboratory experiments must therefore be discounted before they can be accepted as affording a standard of any real practical value.

But have the experiments referred to been so discounted? On the contrary, they have been strained, and the opinions based upon them exaggerated. To test this: It is clear that for the purpose of arriving at the true indication of the experiments, a process of selection must not be adopted. If a proportion is to be asserted or a standard set up, it must not proceed upon the best or the worst, but upon the mean results afforded by the experiments from which they are derived. But if the six experiments in the Commissioner's Report be grouped together for the purpose of striking an average, the mean result will be too favorable, as two out of the six experiments show negative results. Thus in the case of filtration through Barking soil, not only was no purification effected at the rate of 3.8 gallons per cubic yard in twenty-four hours, but deterioration was actually taking place; so that, for the purpose of obtaining a true mean, a lower figure than 3.8 gallons should be taken to represent the positive result of the experiment. And again, in the case of filtration through Leyland peat, the Commissioners merely express a hope that it might be educated into purifying at the rate of four gallons per cubic yard in twenty-four hours; so that, for the purposes of a true mean, a lower figure than four gallons should be adopted. Nevertheless, both these negative results are for the present purpose treated as though they were positive. Striking an average, the mean obtained indicates that a satisfactory purification of London sewage may be effected at the rate of 5.9 gallons of sewage per cubic yard of soil in twenty-four hours. Now the first suggestion of a standard is to this effect (p. 69 of the same Report): "These experiments upon the filtration of sewage through various materials leave no doubt that this liquid can be effectually purified by such processes. . . . In this way the sewage of a water-closet town of 10,000 inhabitants, could at a very moderate estimate, be cleansed upon five acres of land if the latter were well drained to the depth of six feet." Setting on one side the discount due to a laboratory experiment, it is clear that, even then, this proportion of population to acreage is not justified in its extent by the mean of the experiments. A population of 10,000 persons to five acres is in the proportion of 2,000 persons to 1 acre. The sewage of these 2,000 persons must be taken at thirty gallons a head, the London water supply, for it was upon London sewage that the experiments were made; and as the water supply represents the amount of dilution, and, consequently, determines the strength of dry-weather sewage, it is important that, in applying the experiments, the same ratio of thirty gallons per head should be preserved. To say, therefore, that the sewage of 2,000 persons can be disposed of every twenty-four hours, upon an acre drained six feet deep, is equivalent to saying that 60,000 gallons of sewage can be purified every twenty-four hours upon 9,680 cubic yards of soil, or that purification can be effected at the rate of 6.2 gallons per cubic yard in twenty-four hours. But the mean of the experiments was only 5.9 gallons, so that the first standard suggested by the Commissioners is in excess of the mean.

The element of exaggeration becomes more manifest in subsequent utterances. For instance, in their First Report, a population of 3,300 is stated to represent the purifying capacity of an acre drained 6 feet deep, which at once raises the proportion to 10 gallons per cubic yard in twenty-four hours, as compared with 5.9 gallons, the mean of the experiments. In their third Report, the Commissioners appear to have cast off the reserve with which they had expressed their conclusions in the first Report, and at the same time to have strained the experiments to their fullest extent. They say: "And 100 acres or more might be needed to cleanse, certainly to profitably utilise, the drainage of a town of 10,000 people by means of irrigation, it would need but 3 acres of a porous medium 6 feet deep, worked as an intermittent filter, to oxidise and therefore purify the drainage water of such a town, provided the mass of earth through which it percolated were frequently and effectually aerated, and the foul liquid were so added that every part of this aerated filter had its equal share and equal interval of aeration. The laboratory experiments, on which we build our confidence, in filtration thus conducted for cleansing sewage, may be considered conclusive as to the satisfactory and permanent efficacy of the remedy thus provided; and although it is by no means certain that such an apparatus, on the scale needed for a large town, would not itself be a formidable nuisance, yet there is every reason to believe that the water running from it would be sweet and clean."

This extract contains an absolute expression of opinion on the part of the Commissioners, that though the filter might be a nuisance, the sewage of 3,333 persons may be permanently purified upon an acre of land drained 6 feet deep, or that permanent purification can be effected at the rate of 10 gallons per cubic yard in twenty-four hours. But the highest result afforded by the experiments was in the case of the Dursley soil, where purification was effected at the rate of 9.9 gallons per cubic yard in twenty-four hours, while the mean result, as already stated, only justified application at the rate of 5.9 gallons in the same period; and yet the Commissioners have laid down a standard for adoption in practice, which is actually in excess of the highest and most favorable experimental return, and nearly twice as high as the mean result of their experiments. How, then, can it be contended that the experiments have not been strained, and that the conclusions of the Commissioners, even as they stand upon their own experiments, are not fallacious and calculated to mislead?

THE RUSTIC THEORY OF INDUSTRIAL ART.

From "The Architect."

Amongst the art-preachers of the moment who assume the function of revolutionising our industrial design and establishing upon its wreck some better mode, Mr. C. L. Eastlake deserves honourable mention. Indeed he is fully entitled to be called a representative man; for not only is he possessed of a pleasant style of writing—which in itself is half the battle—but he has long indentified himself with the practice of what he preaches, and has met with no inconsiderable share of that success which is the reward of merit.

Such teachers have, as a matter of necessity, the majority against them; but so much the better. Indeed it is this that contributes more than anything else to the delight of their position, which is of course the delight of battle. This it is that holds them together and stimulates them to exertion. Let them but win the conflict—which happily they are never destined to do—their bond of union becomes dissolved, and internal differences spring up to shift altogether the ground of controversy. Still they may be right, and no doubt in most cases are, in one degree or another, seen to be as clearly right in this perverse world as they could expect to be. In the present case, in particular, we need not hesitate for a moment to assume our reformers to be so far right that the necessity for reform is palpable.

The passive majority on the other side must generally be excused, however, if they fail to understand what the new school would have them to do. The wisdom is not always so considerately developed as might be, and the wit especially—of which there is a great deal too much—is scarcely conductive to patient reflection. It comes to be, therefore, quite a godsend in such cases when some professor of the new faith consents to lay on the shelf for a moment both philosophy and ridicule and to give an instance in plain language of what it is he founds his faith upon. "To this day," thus says Mr. Eastlake in his Spitalfields lecture reported last month, "I know no specimen of native manufacture more satisfactory in construction and more picturesque in appearance than a *rustic cart.* It is always solidly made of stout timber and well hammered iron. . . It serves its object well and honestly and has filled the page of many an artist's sketch book. . . That is the kind of work we should try and revive in this country." Now no one can resonably plead misunderstanding here. The idea which is expressed is perfectly plain to the critical mind; it is at once seen to possess, now that we have it fairly before us, the true ring of the reforming doctrine of the class which Mr. Eastlake represents; and it constitutes a specific illustration of that doctrine which will bear handling. Moreover it is an illustration which one may fairly be called upon, in spite of temptation, to handle considerately —with as much of sober wisdom and as little hysterical wit as may be; it being, as we have already ventured to hint, much better just now, in dealing with whatever proposals of revolutionary art, to rely upon the solid sense of argument than to trust to the empty sound of pleasantry.

The rustic cart doctrine, then, is simply this. The industrial fine-art proper to the nineteenth century, as practiced in England, has been a feeble and vulgar sham, expending its small stock of taste —a very small stock indeed—upon hollow affectation and mistaken because ignorant copyism; from which deplorable and indeed contemptible state of things there is but one way of escape, namely, a return to such almost primitive simplicity as shall admit of our being at least honest if we cannot be refined. Honesty, in other words, is, in industrial art as in all else, the best policy; and consequently dishonesty the worst; let us, therefore, discard all our unrealities of design at whatever cost, and fall back upon what is real however humble.

Let us pitch the Lord Mayor's new state carriage, for instance, into the river, and send him to Court in a rustic cart; an extreme measure, of course, but one which would at any rate do honor to those first principles which a perverted generation has so effectually lost.

It is only an incident in this train of thought that "our forefathers" are taken to have done altogether as well in respect of common art as we do altogether ill. This allegation satisfies a well-known prejudice belonging to our day, without really affecting the question at issue; and it may, therefore, be passed over as a sort of rhetorical flourish. At the same time it may be well to understand what is the presise form in which such ancestor-worship is made to do duty in the case in hand. In old times, then, as it is said, our mechanics, remaining stationary in their native towns and villages, "content to work as their fathers and grandfathers did before them," acquired local habits of workmanship, equivalent in fact to traditionary systems, of which "the result was most beneficial to every branch of industrial art." But "railroads were laid down; schools were set up; cheap newspapers were circulated." The old traditions have thus been broken up, or if any have survived it is "probably because no shams could be practised in them without at once rendering the objects produced useless for their purpose;" and it is to emphasise this statement of the case that Mr. Eastlake introduces his illustration of the rustic cart, after which he goes on very pointedly to say—"Pray don't suppose that I am putting an extreme case; . . . all I desire to point out is that (our) object should be to start from the principles which guided our forefathers in design and construction."

Now, as we have said, it would be idle to ridicule this rustic cart, however easy; and we may also again add that it is unnecessary to mix up the historical question with the artistic. The rustic cart stands for a whole order of unpretentious design-work; and the proposition of our refomers is that we ought to revert to the principles involved in this order of unpretentious design-work, and to throw aside those principles which otherwise are our own. For instance, let cast iron ornament give place to wrought; let the joiner strike his mouldings in the solid instead of planting them on the surface; abandon cement work in imitation of stone; abandon the art of graining and marbling; let "crockery" be painted by hand; the contrary of all this is "cheap and nasty work, and a neglect of those honest trades which were once the pride of the British workman."

Mr. Eastlake, in the sequel of his lecture, may no doubt be said to ride off from the simplicity of his doctrine and to dwell rather upon the very different subject of the recent imitative revival of old work—that is, work about a hundred and fifty or two hundred years old—in furniture; but this need not in any way diminish our interest in the original and more important question, how we are to improve the condition of English industrial art by rupudiating our system of counterfeit, and seeking in the crude integrity of the rustic cart a new inspiration. This is a most important and most interesting question, and one that obviously has nothing to do with the graces of Chippendale furniture and Japanese ornaments.

It is not improbable that if Mr. Eastlake had happened to know more of the rustic cart as regards its uses in the farmer's work, and less of it as regards its uses in the painter's picture, he would have hesitated to rest his argument upon this particular illustration. Either the rustic cart is a model of scientific construction and artistic design combined, or it is nothing at all to the purpose. Now the rustic cart of the present day is, more than most things, a subject of diversity of design, variety of quality, and keen competition in price. In a word, if Mr. Eastlake thinks it worth his while to go to some agricultural show and cotemplate the article in its utilitarian integrity, he will probably decline to pin his faith to it any longer. The more purely scientific it is in construction, certainly the less he would like it; and the more completely it is devoid of even the humblest grace, the less would it suit the artist's sketch-book. If, however, it is merely the *old-fashioned* or rather the *old* cart that he would wish to be understood to refer to, then the question arises whether it is not in reality the old-fashioned and used up character of picturesqueness, the oddity and quaintness and obsoleteness, that he admires, and not by any means the mere sturdy substantiality and honesty of crude workmanship. Then, if it is truly this element of picturesqueness that affords him pleasure—that which, beyond doubt, causes it to have "filled the page of many an artist's sketch-book"—the further question arises whether he ought not to prefer the cart of romote localities—of Wales or Ireland, of Normandy or Sweden, of Spain or Siberian Tartary. As for honesty of workmanship, all these are probably very much alike, but in the quality of quaintness they differ to any extent you wish. Not only so, but if he will accept with the cart the carter and the horse, and take his stand upon the indubitable fact that the artist's sketch-book will cheerfully admit Old Dobbin and Hodge—and all the more cheerfully the more obsolete and forlorn they look—while it will emphatically refuse to admit on any terms my lord and his glossy bay—we may surely ask whether Mr. Eastlake is prepared, like an artistic Marat, to advocate the abolition of my lord and his glossy bay as simulacra and impostures, and to throw civilization back upon Hodge and melancholy Dobbin as the honest realities which have descended to us from our forefathers.

Upon the desirableness of our escaping from a condition of artistic pretence and vulgarity there cannot be two opinions; but whether we are to escape from it by adopting the style of the rustic cart is quite another question. It must not be forgotten that the acceptance of anything whatever as a model for imitation is still an act of pretence and counterfeit; and what the reformers of industrial art have to accomplish is the very difficult task of separating the inner principle from the outer example—a task so difficult that it has certainly never been accomplished since the memorable time when the Revival of Arts and Letters endowed the modern world with the artistic wealth of the antique. "When we have mastered those principles (which guided our forefathers)," says Mr. Eastlake, "and learned to work well and thoroughly in *any one style of a by-gone age* it will be time enough to think of inventing a new style." Instead of *principles* it is manifestedly *practice* that is here meant.

If our reformers can do no more than direct us to the adroit imitation of one period of past work instead of another, so far so good, but surely no farther—we may escape, to a certain extent, from vulgarities, but not from shams.

ON FRICTION BETWEEN SURFACES AT LOW SPEEDS.

By FLEEMING JENKIN, F.R.S., and J. A. EWING.

Proceedings of the Royal Society.

The common belief regarding friction, which is based on the researches of Coulomb and Morin, is that between surfaces in motion the friction is independent of the velocity, but that the force required to start the sliding is (in some cases at least) greater than the force required to overcome friction during motion; in other words, the static coefficient is usually considered to be greater than the kinetic. It occurred to the authors that there might possibly be continuity between the two kinds of friction, instead of an abrupt change at the instant in which motion begins. We should thus expect that when the relative motion of the surfaces is very slow there will be a gradual increase of friction as the velocity diminishes. Whether any such increase takes place at very low speeds is left an open question by the experiments of Coulomb and Morin, whose methods did not enable definite measurements of the friction to be made when the velocity was exceedingly small. The authors have succeeded in measuring the friction between surfaces moving with as low a velocity as one five-thousandth of a foot per second, and have found that in certain cases there is decided increase in the coefficient of friction as the velocity diminishes.

The apparatus made use of consisted of a cast-iron disk 2 feet in diameter and weighing 86.2 lbs., supported on a steel axle whose ends were less than one tenth of an inch in diameter. These ends were supported in bearings which consisted of rectangular notches cut in pieces of the material whose friction against steel was to be measured. The disk was caused to revolve and then left to itself, when it came to rest in consequence of the friction on the ends of the axle. The rate of retardation was found as follows:—A strip of paper 2½ inches broad was stretched round the periphery of the disk, and a pendulum was caused to swing across this paper in a plane perpendicular to that of the disk. On the pendulum was fastened a fine glass siphon, one end of which dipped into a box containing ink, whilst the other stood at a short distance from the paper strip, across which it was carried as the pendulum oscillated. By keeping the ink-box strongly electrified ink was deposited on the paper by the point of the siphon in a rapid succession of fine spots. By this means, without the introduction of any new source of friction, a permanent record was made of the resultant motion of the pendulum and the revolving disk. This frictionless method of recording was designed by Sir William Thomson for telegraphic purposes, and is employed in his siphon recorder. From the curve drawn in this way it was easy to determine the rate of retardation of the disk (and therefore the friction) corresponding to various velocities of the rubbing surfaces. The lowest velocity for which the determinations were definite was about 0.0002 foot per second, and the highest velocity to which the experiments extended was 0.01 foot per second. The surfaces examined were steel on steel, steel on brass, steel on agate, steel on beech, and steel on greenheart—in each case under the three conditions, dry, oiled, and wet with water. In the cases steel on beech oiled or wet with water, and steel on greenheart oiled or wet with water, the coefficient of friction increased as the velocity diminished between the two limits given above, the increase amounting to about twenty per cent. of the lower value. It appeared that at the higher limit of velocity there was little further tendency to change in the coefficient; but it is impossible to say how much additional change might take place between the lower limit of the velocity and rest. In the case of steel on agate wet with water there was a similar but much less marked increase of friction as the velocity decreased; and in the case of steel on steel oiled there was a slight and somewhat uncertain change of the opposite character—that is, a decrease of friction as the velocity decreased. This case, however, would require further examination. In all other cases the friction seemed to be perfectly constant and independent of the velocity. Out of all the sets of circumstances investigated, the only ones in which there was a large difference between the static and kinetic values of the coefficient of friction were those in which a decided increase was observed in the kinetic value as the speed decreased. This result renders it exceedingly probable that there is continuity between the two kinds of friction.

TORPEDO LAUNCHES.

From "Engineering."

The very interesting paper which Mr. Donaldson, of the well-known firm of Thornycroft and Co., of Chiswick, read before the United Service Institution, has called public attention forcibly to the subject of torpedo warfare. As a builder of launches, not torpedoes, Mr. Donaldson speaks only of that which he knows, so he broaches no new theory of warfare, but contents himself with describing things already done or built.

A more valuable contribution to our knowledge could hardly be made than his modest list of torpedo launches built and ordered, and we shall take the earliest opportunity of printing it *in extenso*, together with the diagrams by which the lecture was illustrated.

The famous river launch Miranda, built in 1871, and which in the following year was found to go at 18.65 miles, or nearly $16\frac{1}{4}$ knots, per hour, though less than 50 ft. in length, was apparently the prototype of the modern fast torpedo launch. Built of thin steel plates and fitted as lightly as possible, engined to the utmost, and conspicuous in every detail for that perfection in design and workmanship which is never absent from the work of those who achieve, in new fields, such rapid and complete success as Messrs. Thornycroft have attained, the Miranda was seen at once to offer just what torpedoists required. The Norwegian Government, in 1873, gave the first order for a boat of 57 ft. length, 7 ft. 6 in. beam, and 3 ft. draught and having —as appeared in the final trials—the remarkable speed of $17\frac{1}{4}$ miles, or 14.9 knots, with an indicated horse power of about 90. The engine-room and what might be called the steering compartment, were protected against musketry fire by $\frac{3}{16}$ in. steel plaiting, and safety was further sought in division into six water-tight compartments. This boat was armed with a towing torpedo attached to the funnel, in such a manner as to diverge at an angle from the boat's course, in the same manner as the Harvey torpedo. Like some other apparently unpromising types, the Norwegian launch have very respectable sea-going qualities. Two similar boats were shortly after built for the Swedish and Danish Governments, that for the Danish Government, and probably the other also, being armed in a manner resembling the boat first described.

The next boats built were larger and more powerful, the class comprising, apparently, two for France and one for Austria. The length was 67 ft.; beam, 8 ft. 6 in.; draught, 4 ft. 3 in.; indicated horse power about 200; and speed over 18 knots. The armament consisted of spar torpedoes, fired by electricity, either automatically upon contact, or at pleasure of the operator. The French boats on their passage out crossed boldly from the Thames to Cherbourg, without hugging the coast, and in February and March of this year they proved their efficiency in some experiments which have attracted a great deal of attention. The Bayonnaise, an old wooden frigate, "which had been damaged in one of the earlier experiments, and was on this occasion kept afloat by means of empty casks," was towed by another vessel at a speed of about six knots, and was attacked by one of the torpedo boats. As the rehearsal was incomplete, so far as any attempts at defence were concerned, a hole was made in the Bayonnaise large enough "to admit a full sized omnibus" —a result not at all surprising, nor calculated to teach anything not known many years back, but which, from its sensational character, drew great attention from the English press. (The accounts published at the time were absurdly exaggerated, as any one reading them might see.) The real interest of the experiment, and the point it was intended to throw light upon was the effect of the explosion upon the attacking boat. "At the moment of explosion a slight shock was felt, and immediately afterwards a large wave, was upheaved between the Bayonnaise and the torpedo boat, *which was driven backward a considerable distance* and completely covered with water, so much so that M. Lemoine and his brave companions for the moment could not say whether they had gone to the bottom or not. This state of doubt was soon dispelled, however, and M. Lemoine steamed slowly off to report himself on board the Coligny." The torpedo used contained 15 kilogrammes of damp guncotton, and was fired 8½ ft. below the surface, at the end of a steel pole 40 ft. long. Six other boats, considerably larger—indeed the largest torpedo launches yet commenced—are now in hand for the French Government, to be armed, it is believed, in the same way. They are 87 ft. long by 10 ft. 6 in. beam, and are more heavily built than any yet constructed, and Messrs. Thornycroft guarantee they shall maintain the speed of 18 knots during a three hours run. The plates and frames below water are galvanized.

Between the earlier and later French boats come some for the Dutch and Italian Governments; they measure 76 ft. by 10 ft.; and are to have a guaranteed speed of 18 knots. The Dutch boats will be armed with spar or outrigger torpedoes, like the French, and the Italian with the Whitehead or fish torpedo.

Nearly last on the list comes the single small torpedo craft yet constructed for our Government, the Lightning, whose successful speed trials on the Thames attracted attention a few weeks back, when (running a little light) she attained the speed of 19.4 knots. Her length is 84 ft., and breadth 10 ft. 10 in., with a draught of 5 ft., and indicated horse power of about 350. As might be expected from her size she has considerable sea-going qualities and internal accomodations, but she can hardly be called a steam launch. She will be armed with the Whitehead torpedo to be discharged *from the deck* forward.

It cannot be doubted that this principle of attack is destined to play a very large part in naval warfare, but those upon whom it now bursts as a new revelation (which it is not, for Messrs. Thornycroft's work, valuable as it is, has been only to render more practicable the application by a principle long since recognized) appear likely to even exaggerate its importance. So far as we can see at present, neither attacks by fast launches nor by Whitehead's torpedoes, discharged under water from other large vessels, are likely to render heavy fighting ships obsolete. They introduce an extremely formidable element of offensive warfare, which no country can afford to neglect, and which England ought to adopt upon an extensive scale, and they set us also a problem of defence of a very difficult, but, we think, not hopeless kind. There has been little time yet for devising means of protection, and it must not be inferred, because none of a perfectly satisfactory kind present themselves at once, that none can be found. Mr. Donaldson, with a natural partiality for fast torpedo launches, advocates their employment for defence as well as for attack; a small squadron of them is to watch around the ironclad, and to stop any of their own kind which may approach. Mr. Donaldson wisely adds that these should be small enough to be hoisted on board the ironclad, but thinks the original 57 ft. type which weighs 7 tons, is not too large for the purpose. Writing some weeks back, we expressed a strong opinion in favour of furnishing ironclads with small torpedo launches of this kind, so we are to a great extent of Mr. Donaldson's opinion, but we believe that other modes of defence may be found as effectual.

The proper use of defensive torpedoes we long since pointed out as a method of meeting this new and terrible danger which might very probably prove successful, and we are not sure that it would not be practicable to surround a ship with such dangers as to make successful approach almost impossible. So long as she is under way a sufficient number of Harvey or other towed torpedoes might well make her almost unapproachable from behind, and for the greater part of her length upon either beam, and similar protection round the bow is not inconceivable. As soon as the problem is clearly apprehended—that is, how to make it impossible for a launch to approach without itself running foul of a torpedo before its own can take effect—we suspect it will not be long before a solution is found. The Whitehead torpedo itself might perhaps be utilised in this way, by treating it as a divergent towing torpedo until the launch approached, when directed and set in motion by electric communication through the towing line, it might be sent on ahead of the ship, or caused to assume any position likely to produce collision with the launch. It must be remembered that eluding the torpedoes themselves would not necessarily save the launch; if properly arranged she could hardly fail to foul one or other of their tow-ropes, and thus produce a collision with a torpedo. Not with any idea of suggesting an actual plan, but merely to show how readily such modes of defence suggest themselves, it may be worth while to ask whether two strings of torpedoes, towing one behind the other, might not be kept advancing in parallel lines with the ship, the leading one of each line being towed at a high angle of divergence from, say, the edge of a spar projecting beyond the ship's stem. It might not be too fanciful to conceive the line prolonged forwards on each side by self-moving torpedoes, controlled electrically from the ship. It is not easy to see how a launch could encounter such a defence without very grave danger. Whether any modification of this idea could give partial security against the submarine attack of the Whitehead torpedo might also be worth considering.

Another means of defence of more familiar kind is open to the attacked ironclad, viz., artillery fire. Of the value of this, as against a swift-moving launch, many authorities, including Mr. Donaldson, entertain a poor opinion. But amongst skilled gunnery officers this view is far from being universal, and the strongest advocate of torpedoes will admit that if the guns can only be fired often enough, while the launch is within range, there must be a good chance of destroying her. That the Devastation, with nothing but four 35-ton guns, would have little chance of hitting her pigmy foe, may be conceded, but all ironclads are not armed in that way, and there is no reason why any should be. Henceforth, every ship should carry as large a number as possible of torpedo guns upon the upper deck, expressly designed for accuracy and rapidity of handling. Since they would be required to fire projectiles of a small size, Gatling guns modified might answer the purpose. The object will be to fire as many shots as possible during the space of about three minutes, and it is obvious that, with proper appliances, a very large number might be got off in that time—of which one at least might find its mark.

Whether still more radical measures can be taken tending to make the ship itself torpedo-proof, is a question we are not now disposed to discuss, and which, indeed, we have small hope of seeing answered satisfactorily. Enough has been said to show that the navies of the world are not necessarily at the mercy of Messrs. Thornycroft's wonderful little vessels, while as to attacks by the fish torpedo, if the defence is as yet more difficult, the attack is more uncertain.

Of the launches themselves we can only repeat our high opinion of their value, and our regret that England is so far behind in this branch of preparation. As Mr. Donaldson says, "we have only one fast steam torpedo launch in Her Majesty's Navy, while other nations are providing them by the dozens for the defence of their principal ports."

The Lightning, which we presume costs about 6000*l.*, is an expensive implement to employ upon service of the forlorn hope kind, but it is clearly desirable to have some boats capable of keeping at sea for a few days. The distance at which they are to operate from their base must mainly determine the size, but we look upon all too large to be carried on board ship as valuable only for coast defence. For use with fleets we need something either smaller or larger than the Lightning, and, of course, we prefer the smaller type. As we remarked in a recent number of *Engineering*, the Italians are showing great foresight in naval matters, having the apparent intention of making the Mediterranean an Italian lake, and a very fair idea of how to set about it. In this matter of carried torpedo launches as in the construction of iron-clads of the very heaviest class, we have allowed them to get the lead, and the two Italian inflexibles, the Duilio and Dandolo, are each, we believe, to be fitted with two of the launches described by Mr. Donaldson. They are too large for hoisting up in the usual way, but are to be carried, as is well known, in a sort of dock in the after part of the ship—an arrangement of a very novel kind, perhaps open to criticism.

Messrs. Thornycroft's achievements need inspire no panic, but they require a more earnest attention than they appear to have received yet from the Admiralty—if English supremacy on the sea is to be maintained.

MOMENTUM AND VIS VIVA.

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Contributed to Van Nostrand's Magazine.

I.

To the mind of a student anxious to comprehend as thoroughly as may be the subjects of his inquiry, the treatment of *momentum* and *vis viva* by the books on physics and mechanics is extremely unsatisfactory. He rises from his study feeling that there is something left unexplained; and failing to find anywhere an adequate answer to his questions, he is fain to conclude that there is some mystery in the case which will never be quite solved. Several text-books, held in high esteem in our schools and colleges, say that the *momentum* of a moving body is its *quantity of motion*, and that it is equal to the mass of the body multiplied by its velocity. For instance, if a body equal to 10 units of mass have a velocity of 10 feet per second, its *momentum* is said to be equal to $10\times10=100$. But 100 what? Is it 100 feet, or 100 pounds, or 100 of something else? If *momentum* is a *quantity of motion* it certainly cannot be 100 pounds, and it is quite as difficult to say that this body has a *quantity of motion* equal to 100 feet. And here the majority of the books leave us without giving the first idea as to what the unit of the *momentum* is.

In the case of the *vis viva* of a moving body, represented by one-half the mass multiplied by the square of the velocity, an attempt is made to show what its unit is, but nearly all the books fail to point out the essential difference between this unit and the unit of momentum. Thus the result is a confusion of ideas, leading to vagueness and error. It is proposed in this article to attempt an explanation, as nearly complete as possible, of the exact meaning and nature of *momentum* and *vis viva*, as deduced from a few simple and fundamental mechanical principles.

Physicists all agree in calling the product obtained by multiplying the mass of a moving body by its velocity the *momentum* of the body; denoted algebraically by MV. But there is not the same agreement as to what that product really *means*. In order to understand what it does represent let us consider its relation to the force required to impart the velocity V to the mass M originally at rest.

It is a fact familiar to every one, that a body cannot be moved from rest without the application of force, and also that a body already in motion cannot be stopped without the exertion of force. This property of bodies, by virtue of which they resist any change of state, either of rest or motion, is called *inertia*. The common unit of force, with which the intensities of all other mechanical forces, whether producing motion or not, may be compared and measured, is the force capable of producing a pressure of one pound avoirdupois. This unit will be called briefly a pressure of one pound. Now it is clear that if a force equal to one pound pressure act for one second on a certain unit of mass, originally at rest, and impart to the mass a certain unit of velocity, say one foot per second, then a force equal to two pounds pressure will be required to impart the same velocity in the same time to twice the mass; or three pounds pressure will be required to impart the same velocity in the same time to three times the mass; and so on, the number of pounds pressure being equal to the number of units of mass to which in one second a velocity of one foot per second is imparted. Also, since any force acting on a body produces its own proper effect on the motion of the body, whether any other force act on the body at the same time or not, it is clear that in order to impart to the unit of mass in one second a velocity of two feet per second, by a constant pressure, the intensity of that pressure must be two pounds; or if the velocity given to the unit of mass in one second be three feet per second, the pressure will be three pounds; and so on, the number of pounds pressure being equal to the number of units of velocity given to the units of mass in one second. If, then, a certain pressure acting for one second on say five units of mass give it a velocity of one foot per second, the intensity of that pressure is five pounds; and the intensity of the pressure which could give to the same five units of mass in the same time a velocity say of six feet per second would be six times five pounds, or thirty pounds. And in general, the number of pounds in a constant pressure which can impart in one second to M units of mass a velocity of V feet per second, is equal to the number of units in the product MV.

These laws may be verified by means of Atwood's machine. But it is to be noticed that the pressure, by these laws, is supposed to be all applied in producing motion of the body through space, none of it being employed in producing compression or extension of the mass; since in the case of gravity, by which the laws are verified, the force is uniformly applied to all the particles of the body, and has no tendency to alter its shape.

If, then, any body having M units of mass, be moving with a velocity of V feet per second, no matter how long it may have been moving, or how intense the force which gave it motion, we know that the same velocity *might* have been given to it in one second by a constant pressure of MV pounds, uniformly distributed through its mass. It may further be proved by Atwood's machine that if a body move from rest by the action of a constant pressure for a certain time, and acquire any velocity, and the impelling pressure then cease to act, the body may be brought to rest again in an equal time by an equal constant pressure in the opposite direction. Therefore, in the case of any mass M moving with the velocity V, since the same velocity could have been imparted in one second by a constant pressure of MV pounds, that velocity may be destroyed and the body brought to rest in just one second, by an equal constant pressure of MV pounds, uniformly applied to its mass, in the direction opposite to its motion.

Let us now consider the value of M. By the standard adopted above, the unit of mass is the amount of matter which, acted on for one second by a pressure of one pound, will acquire a velocity of one foot per second. But this amount of matter is not the same as the amount which weighs a pound. In case of a pound weight falling freely in a vacuum we have a pressure of one pound exerted on the amount of matter which weighs a pound, and the velocity at the end of one second will vary slightly with the latitude of the place, and is usually represented in physical formulæ by the small letter g. But if a pressure of one pound acting for one second on the amount of matter which weighs a pound can give it a velocity of g feet per second, it follows from principles already explained that the same pressure of one pound can produce, in one second, a velocity of one foot per second in the amount of matter weighing g pounds. Hence g pounds of matter is our unit of mass. The number of units of mass, then, in any number of pounds of matter is equal to the number of pounds divided by g. Or if G be the number of pounds in the weight of a body, and M the number of units of mass, we have $M=\frac{G}{g}$.

If, then, in the expression MV we substitute this value of M, we have $MV=\frac{GV}{g}$; and the number of units in this expression represents the number of pounds constant pressure, uniformly distributed, by which the velocity V *might* have been imparted in just one second of time to the mass M, weighing G pounds, or by which the mass M, weighing G pounds *might* be brought to rest in the same time from the velocity V, if an equal constant pressure were similarly applied in the direction opposite to its motion. That is exactly what the product $MV=\frac{GV}{g}$, which is the *momentum* of the body, represents. And the *momentum* of the body has properly no other meaning.

The value of g, found by experiment, is approximately $32\frac{1}{6}$ feet. Substituting this value we have $MV=\frac{GV}{32\frac{1}{6}}$. If we say then that a body which contains ten units of mass, (and whose weight would therefore be $10\times32\frac{1}{6}$ pounds $=321\frac{2}{3}$ pounds), and whose velocity is equal to ten feet per second, has a momentum equal to $10\times10=100$, we mean that the same velocity *might* have been imparted to the same mass in just one second by a uniformly distributed constant pressure equal to 100 pounds; or that if the body were brought to rest in just one second by a constant pressure similarly applied, but exerted in a direction opposite to its motion, the intensity of that pressure would be equal to 100 pounds. And we do not mean anything else whatsoever.

Let us now consider the *vis viva* of a moving body. It may be shown by means of Atwood's machine that in order to overcome, by a constant pressure, the inertia of a given mass of matter at rest, and cause it to move with a given velocity, a certain definite number of *units of work* must be performed on it. And this is true whether the velocity be imparted in one second or any number of seconds, the number of *units of work* which must be performed to produce in the mass the given velocity is the same in all cases. By a *unit of work* is meant the exertion of a unit of pressure through a unit of distance, usually taken as a pressure of one pound through a distance of one foot; and the number of units of work, called usually foot-pounds, performed by any force, will be equal to the number of pounds pressure produced, multiplied by the number of feet through which it is exerted. Let us see if we can determine the number of units of work performed on any mass M in imparting to it any velocity V.

It is found by experiment that the velocity acquired by a a body, when acted on by a constant force, is proportional to the time during which the force acts. Hence, since a pressure of one pound, acting for one second on one unit of mass, gives it a velocity of one foot per second, the same pressure of one pound acting on the unit of mass for a certain time, say six seconds, will give it a velocity equal to six feet per second. But we know from the laws of uniformly accelerated motion that if the body start from rest, and move by the action of a constant force, the distance passed over in the six seconds is equal to one-half the distance which would have been passed over in the same time with a uniform velocity equal to the final velocity, which is six feet per second. The distance actually passed over by the unit of mass in acquiring the velocity of six feet per second from the action of a constant pressure of one pound, is then equal to eighteen feet. The pressure of one pound has therefore been exerted through a distance of eighteen feet, and the number of units of work performed is eighteen. If the same velocity be imparted to the same unit of mass by a constant pressure of two pounds, the time employed is equal to one-half the time employed before, or three seconds; and the distance passed over is therefore nine feet, and the number of units of work is two multiplied by nine, or eighteen, the same as before. If a pressure of three pounds be used the time is two seconds, and the distance passed over is six feet, and the number of units of work is three multiplied by six, or eighteen again. And in general to impart any velocity V to one unit of mass by a constant pressure, if T be the number of seconds employed, the distance passed over will be $\frac{T\times V}{2}$. But if the pressure be one pound, the number of units in the time is equal to the number of units in the final velocity; or $T=V$. And if the pressure be equal to P pounds we shall have $T=\frac{V}{P}$. Substituting this value of T we have the distance passed over by the unit of mass, while acquiring any velocity V, by the action of any constant pressure P, equal to $\frac{V^2}{2P}$. But the number of units of work performed by the pressure P will be equal to this distance multiplied by P; or, if W represent the number of units of work, we shall have $W=\frac{1}{2}V^2$.

This is the number of units of work performed in giving the velocity V to one unit of mass. Evidently, to give the same velocity to twice the mass, twice the work must be performed, or to give the same velocity to M units of mass, M times the work must be performed. The number of units of work, then, which must be performed by any constant pressure, on any mass M, starting from rest, to give it any velocity V, is equal to $\frac{1}{2}MV^2$; and this result is wholly independent of the time employed, the distance passed over, or the intensity of the impelling pressure, and dependent only on the number of units in the mass M and the final velocity V. We must, however, notice that if any work is performed in changing the shape of the mass, it must be in addition to the work represented by $\frac{1}{2}MV^2$.

In order to find the number of units of work performed by any constant pressure while increasing the velocity of a mass M, already having the velocity V, from this velocity to the velocity V', we have simply to observe that the work required to give the mass the velocity V' if it start from rest, is $\frac{1}{2}MV'^2$, and the work required to give it the velocity V, starting from rest is $\frac{1}{2}MV^2$; hence, the work performed while increasing the velocity from V to V', is equal to the difference between these results, or

$$\tfrac{1}{2}MV'^2-\tfrac{1}{2}MV^2=\tfrac{1}{2}M(V'_{2}-V^2).$$

We may now show that the work performed in giving the mass M, starting from rest, any velocity V, will be equal to $\frac{1}{2}MV^2$, whether the impelling pressure be constant or variable. For if it be variable we may suppose the time divided into very small periods, during each of which the pressure may be regarded as constant. Then, if, V', V'', V''', &c., be the velocities at the end of the first, second, third, &c., instants, we shall have the work performed in the first instant equal to $\frac{1}{2}MV'^2$. That in the second instant will be $\frac{1}{2}M(V''^2-V'^2)$; that in the third $\frac{1}{2}M(V'''^2-V''^2)$; and so on. Now the sum of all these elementary quantities of work is the total work performed, which is therefore

$$\tfrac{1}{2}MV'^2+\tfrac{1}{2}M(V''^2-V'^2)+\tfrac{1}{2}M(V'''^2-V''^2)+\dots\dots$$

If V be the final velocity the sum of this series will evidently be $\frac{1}{2}MV^2$, since each of the other positive terms will be cancelled by an equal negative term.

It is therefore a universal principle, that if any mass M be moved from rest, and acquire any velocity V, whether the impelling force be constant or variable, no matter what its intensity, or what the distance through which it act, or what the time employed, the number of units of work performed by the force in giving it that velocity is always the same, and is equal to $\frac{1}{2}MV^2$.

This work has been performed in overcoming the inertia of the mass; and by reason of its inertia the mass M, moving with the velocity V, has the power of performing just the same amount of work while it is coming to rest by the action of an opposing force, as was performed upon it in giving it the velocity V. For if any pressure give a mass any velocity in passing over any distance, and then cease to act, the mass, by reason of its inertia, is able to overcome an equal pressure through an equal distance, before being brought to rest, and thus to perform an equal amount of work.

The product $\frac{1}{2}MV^2$, therefore, represents the number of units of work required to impart the velocity V to the mass M, starting from rest; or the equal number of units of work which the mass M, with the velocity V, can perform before being brought to rest by any force, whether large or small, in whatever time or distance. This product, $\frac{1}{2}MV^2$, is very properly called the *vis viva*, or *living force* of the moving body, since it represents a definite power for performing work, or giving motion to other bodies; which power is exhausted, or may be said to die, at the instant the body comes to rest. The body can then impart no motion to other bodies at rest except it be first set in motion, or, as it were, brought to life again, by the action of some external power.

It should be remembered that our unit of mass here is the same as in the case of momentum; and thus, if G be the weight of the body, the *vis viva* is equal to $\frac{1}{2}MV^2=\frac{1}{2}\frac{G}{g}V^2=\frac{GV^2}{64\frac{1}{3}}$. If we say then that a body whose mass is 10, (and weight accordingly $321\frac{2}{3}$ pounds), and whose velocity is 10 feet per second, has a *vis viva* equal to $\frac{1}{2}\times 10\times(10)^2=500$, we mean that 500 units of work must have been performed, in order to give it that velocity, starting from rest, or that it will perform 500 units of work before being brought to rest by any force whatever.

The unit of *momentum*, then, is a force of pressure equal to one pound, and the unit of *vis viva* is the *work* performed in overcoming a pressure of one pound through a distance of one foot. With this understanding there is perfect harmony in all the results. For example, if a body whose mass is 10, moving with a velocity of 10 feet per second, be brought to rest in one second by a constant force, we know that the space passed through will be 5 feet. But the constant force which can bring it to rest in one second, is a pressure equal to its *momentum*, or $10\times 10=100$ pounds. The work which the mass will perform is then $5\times 100=500$ foot-pounds, or units of work; which is equal to its *vis viva*, or $\frac{1}{2}\times 10\times(10)^2$, as it should be. Or, if the same body fall freely from rest by the action of gravity, the distance it will fall in acquiring the velocity of 10 feet per second will be 1.554 feet. The work performed on it by the force of gravity in giving it that velocity is therefore $321\frac{2}{3}\times 1.554=500$ foot-pounds again. And a like harmony may be proved true in any other special case, or may be demonstrated generally.

It should now be observed that the *momentum*, represented by MV pounds, is of no special value for estimating the power of a moving body, or anything else whatsoever with regard to its motion, any more than any other arbitrary force we may choose. For in order that MV pounds shall be a measure of the pressure the body can produce, the body must be supposed to be brought to rest without change of form in just one second of time. Thus the product MV is not a measure of the pressure a moving body is *able* to produce, or of anything else with regard to it, except under a certain arbitrary condition; the intensity of the pressure a given moving body is able to produce, while coming to rest without change of form, being wholly dependent on the condition of time or distance in which it shall be brought to rest. For if we impose the condition that the mass M, whose velocity is V, shall be so brought to rest by a constant pressure in just four seconds, it is easily shown that the distance passed over before coming to rest will be equal to $2V$. Dividing the *vis viva* of the body, or $\frac{1}{2}MV^2$ by this, we have $\frac{1}{4}MV$ for the number of pounds constant pressure the mass will exert while coming to rest. And this, or any other number of pounds, is just as valuable for measuring anything with regard to the motion of the body, as MV is, if we impose the proper condition as to the time or distance in which it shall act. And we might, of course, have deduced the *momentum*, or the constant pressure required to bring the mass M to rest from the velocity V in one second, in the same way by which we have just obtained $\frac{1}{4}MV$ as the pressure required to bring it to rest in four seconds. For if the mass be brought to rest in one second, by a constant pressure, it will move a distance equal to $\frac{1}{2}V$, and the *vis viva* divided by this gives the intensity of the pressure equal to MV. This simple process would have been sufficient to show precisely the meaning of the product MV; but there is such a tendency to consider that product as in some way representing a *quantity of motion*, that it seems advantageous to establish its exact signification by two independent methods.

The product $\frac{1}{2}MV^2$, rightly understood, is, on the contrary, a valuable and definite measure of the accumulated energy of the moving body, without any arbitrary conditions whatever; and, as before remarked, is very appropriately named the *vis viva* of the body.

II. MOTION PRODUCED BY IMPACT.

We will now discuss the relation of momentum and vis viva to the case in which motion is communicated by impact from a moving body to another body previously at rest, both the bodies being supposed to be inelastic. In the case of one inelastic body in motion striking another inelastic body at rest, with its center of gravity in the line of direction of that of the moving body, so that after impact the two bodies shall move on together as one body, the ordinary mode of obtaining the velocity of the combined mass is by assuming that the momentum of the whole after impact will be equal to the momentum of the first body before impact. Thus, if M be the mass of the moving body, and V its velocity, M' the mass of the body at rest, and V' the velocity of the combined mass after impact, it is assumed that after impact we shall have $(M+M')V' = MV$, whence $V' = V\frac{M}{M+M'}$. The argument by which such an equality is inferred is given as follows in "Cooke's Chemical Physics," a work of deservedly high reputation:

"We shall consider the bodies as completely devoid of elasticity, and so constituted that after the collision they shall move as one body. Let us then inquire what will be the direction and velocity of the united mass after the impact. The mass M', being previously at rest, can have no motion save what it may receive from the mass M, and consequently must move in the same direction as the mass M moved in before the collision. Again, since bodies cannot generate or destroy motion in themselves, it follows that whatever motion the mass M' may acquire must be lost by the mass M; and also, that the total momentum of the united masses after the collision must be exactly equal to the momentum of the mass M before it."

In the work from which this is quoted, the true meaning of momentum, as previously explained, had once been given. But it seems to have been lost sight of in this argument. For it is here implied that momentum is either the supposed quantity of motion of a moving body, or at least proportional to such a quantity. If this were so, and if there were no modifying circumstance overlooked, the argument might be valid. But is momentum proportional to any such quantity? And is there no important modifying circumstance unnoticed? Let us examine it a little.

In the former article we arrived at a definite understanding of the meaning of the terms *momentum* and *vis viva.* We saw that *momentum*, which is equal to MV, represents the number of pounds pressure which the mass M with the velocity V is capable of exerting under a certain arbitrary condition; and that the *vis viva*, which is equal to $\frac{1}{2}MV^2$, represents the number of foot-pounds of work which the mass M with the velocity V must perform before being brought to rest by any force, constant or variable, without any conditions as to time, or the intensity of the force. Accordingly, if the mass M have its velocity reduced by any force, from the velocity V to a less velocity V', the work which the mass must perform during that reduction will be a definite quantity, viz:

$$\tfrac{1}{2}MV^2 - \tfrac{1}{2}MV'^2 = \tfrac{1}{2}M(V^2 - V'^2).$$

If, then, the velocity of any moving body be reduced by impact upon a body at rest, and the two move on as one, a certain definite amount of work must be performed by the first body during the impact, by reason of the reduction of its velocity. Now, since any body is capable of performing, before coming to rest from any velocity, the same amount of work as is expended in giving it that velocity, it would follow that in case of a mass M with the velocity V impinging on another mass M' at rest, so that the two should move on with the common velocity V', if all the work performed by the mass M, by reason of the reduction of its velocity from V to V', were employed in imparting the velocity V' to the mass M', the work which the combined mass after impact could perform before being brought to rest would be the same as that which the first body was capable of performing before impact. We should thus have the *vis viva* after impact equal to the *vis viva* before impact, or, $\frac{1}{2}(M+M')V'^2 = \frac{1}{2}MV^2$; from which we should derive $V' = V\sqrt{\frac{M}{M+M'}}$.

We arrive at this result by a logical argument from known facts and principles; but only under a hypothesis which is not true in fact, viz.: that all the work performed by reason of the reduction of velocity of the one body, be employed in imparting velocity to the other. In fact, whenever impact between two bodies occurs there is always more or less change of form, or compression of the mass of both bodies, either permanent or temporary; and a certain amount of work is employed in producing this compression. If the bodies are inelastic they remain permanently compressed, and in such cases there is always a certain amount of heat developed, which is the exact equivalent of the work performed in producing the compression. That is to say, ordinary motion through space is converted into molecular motion. And it is evident that if a body in motion strike a body at rest, none of the work which the moving body performs in the development of heat, or molecular motion, can at the same time be employed in imparting ordinary motion to the other body. Hence, in the impact of inelastic bodies, not all the work performed in the reduction of the velocity of the one can be employed in imparting velocity to the other; therefore, in the case supposed, V' must be less than

$V\sqrt{\frac{M}{M+M'}}$, instead of equal to it.

And we now see a very important modifying circumstance entirely unnoticed in the argument above quoted from Cooke. The argument says nothing about any destruction of ordinary motion, during impact, by its conversion into heat. On the contrary, the argument states that "bodies cannot generate or destroy motion in themselves;" which, though it may be true in the widest sense, is not applicable here when considered alone with reference to ordinary motion. For the bodies are supposed to be completely inelastic; and we know that in every case of impact of inelastic substances, heat or molecular motion is developed, and developed only by the destruction or conversion of ordinary motion. If, then, momentum, or MV, really represented or were proportional to a hypothetical quantity of ordinary motion, which is the only kind of motion it could in any way be conceived as representing, the momentum of the combined mass after impact, or $(M+M')V'$, could not possibly be equal to that of the body in motion before impact, or MV; since in every case of impact the velocity of the moving body will be partially destroyed by the conversion of ordinary motion into heat, or molecular motion, which the product $(M+M')V'$ could in no way include.

In what has now been said attention is simply called to the fact that the usual argument for deriving the equation, $(M+M')V'=MV$, does not give satisfactory proof of it. It has not been denied that the equation may be true. For if momentum, or MV, does not represent a quantity of ordinary motion, (whatever that be), and is not proportional to such a quantity, it may then be possible that after impact in the case considered, even though there be a conversion of ordinary motion into heat, $(M+M')V'$ will be equal to MV. If it be a true equation we have $V'=V\frac{M}{M+M'}$. But the velocity previously deduced under the supposition that there be no loss of vis viva is $V'=V\sqrt{\frac{M}{M+M'}}$. Hence, if the first of these equations be true it leads to the result that whatever the velocity of the moving body, or whatever the ratio of the masses, the loss of vis viva by the impact, or the work performed in producing molecular motion, is just sufficient to account for the reduction of the velocity of the combined mass after impact, from $V\sqrt{\frac{M}{M+M'}}$ to $V\frac{M}{M+M'}$. But have we any right to make an assumption which leads to such a result, without a demonstration of the proportion of work consumed in the development of molecular motion? or without a hint that there is any work so consumed? Although some of the books notice that the formulas as ordinarily deduced involve a loss of vis viva by impact, none of them, so far as observed, attempt to explain what that lost vis viva has been doing, or whether it has done anything. We are left to infer that a part of the work, which previous reasoning shows must be performed in any reduction of velocity, has been doing nothing. Vis viva has mysteriously disappeared, and that is all.

If then we are not at liberty to assume that in the case of impact here considered either the vis viva or the momentum after impact will be equal to that before impact, how can we correctly obtain the common velocity of the two bodies after impact? The following method is thought to be free from the objections brought against the usual argument.

We know that the velocities generated or destroyed in two masses, by the respective actions for the same time of two forces of equal intensity, are inversely proportional to the masses. In the case of impact, although the time of the action may be very short, there is nevertheless a certain amount of time spent before the velocity of the two masses can become the same; and the acting forces are subject to the same laws that govern in all other cases. Now, when one body at rest is struck by another body in motion, however the pressure may vary in intensity, or whatever may be the molecular disturbances of action and reaction produced, there will at every instant before the velocity of the two masses becomes the same be a total resultant pressure on the mass of the first body, in the direction of the motion. And by the principle that action and reaction are always equal and opposite, the resultant pressure on the mass of the second body must be all the time equal to that on the first, and in the direction opposite to its motion. The velocity generated in each instant in the first body and that destroyed in the second body will then be to each other inversely as the masses. This being true for each instant, must be true for the whole time of the action; so that the total velocity generated in the first body by the impact will be to that destroyed in the second body inversely as the masses. If M be the mass of the moving body, and M' that of the body at rest, V the velocity of the mass M, and V' their common velocity after impact, then $(V-V')$ will represent the velocity lost by M in the impact, and V' will represent the velocity imparted to M'. We shall thus have the proportion $M : M' :: V' : (V-V')$. From which we derive $V'=V\frac{M}{M+M'}$.

The velocity, V', is the common velocity of the two bodies at the instant their velocities have become equal. If the bodies were perfectly inelastic there would then be no tendency for them to separate, and neither of them would have any further power to change the velocity of the other. They would therefore move on together with the common velocity.

This velocity, $V'=V\frac{M}{M+M'}$, has now been deduced from a consideration of the acting forces, without any assumption as to the vis viva or momentum after impact. We may therefore have confidence that it is correct; and experiments with bodies so arranged as to be obliged to move together after impact show it to be the true velocity in the case considered. Knowing it to be true, and knowing also that if there were no loss of vis viva from the production of molecular motion the velocity of the combined mass would be $V'=V\sqrt{\frac{M}{M+M'}}$, we may feel sure that the difference between these velocities is due to the conversion of ordinary motion into molecular motion, or heat, and there need be no mystery as to what has become of the lost vis viva.

Let us now determine the loss of vis viva occasioned by the impact, and we shall thus have the mechanical equivalent of the heat developed. The vis viva of the mass M before impact is equal to $\frac{1}{2}MV^2$. That of the combined mass after impact is

$$\tfrac{1}{2}(M+M')\frac{M^2V^2}{(M+M')^2}=\tfrac{1}{2}MV^2\frac{M}{M+M'}.$$

The difference between them is

$$\tfrac{1}{2}MV^2 - \tfrac{1}{2}MV^2\frac{M}{M+M'}=\tfrac{1}{2}MV^2\times\frac{M'}{M+M'}.$$

The number of units then in this expression, $\frac{1}{2}MV^2\times\frac{M'}{M+M'}$, is the number of foot-pounds of mechanical work which, in the impact of bodies in the way supposed, is employed in the production of heat. And we see that with a given velocity of the mass M, the amount of work employed in the production of heat varies with $\frac{M'}{M+M'}$, or the ratio of the mass previously at rest to the combined mass after impact. Thus, if M and M' are equal, we have this ratio equal to $\frac{1}{2}$. That is, one-half the vis viva of the mass M before impact is employed during impact in imparting velocity to the mass

M', and the other half in the development of heat. If M' is very small compared with M, there will be hardly any loss of vis viva, from the production of heat. If M' is very large compared with M, nearly all the vis viva of the latter previous to impact will be consumed during impact in the production of heat. In the case of a small mass M falling upon the earth, for instance, its mass will be practically infinitesimal in comparison with that of M' in this ratio, $\frac{M'}{M+M'}$, which then differs from 1 only by an infinitesimal quantity. In this case all but an infinitesimal part of the vis viva of the mass M before impact is by the impact converted into heat; which we know is practically true when the motion of a body is suddenly arrested.

After having thus determined the loss of vis viva by the impact of two inelastic bodies, we may next proceed to compute the rise of temperature which the combined mass will undergo by the impact, supposing the heat to be uniformly distributed. We know that the expenditure of 772 foot-pounds of work is capable of raising the temperature of a pound of water one degree Fahrenheit. The relative capacity for heat, of different substances, has been determined and tabulated; and from these data, knowing the material of the bodies between which impact occurs, and the number of foot-pounds of work converted into heat, as above determined, we can easily calculate the rise of temperature under the condition supposed.

Let us now examine the momentum after impact. From the equation $V'=V\frac{M}{M+M'}$, or from the proportion by which it was obtained, we have $(M+M')V'=MV$; which proves that the momentum, or constant opposing pressure required to bring the combined mass to rest in one second from the new velocity V', is equal to that which would have been required to bring the mass M to rest in an equal time from the velocity V. But this does not prove momentum to be a quantity of motion, as might once have been thought. On the contrary, it proves most conclusively that it cannot be any such thing. For we know there has been change from ordinary motion to molecular motion during the impact, and yet the momentum remains unaltered.

There is then a compensation of errors in the ordinary argument for obtaining the velocity of the mass after impact; the first being the assumption that momentum can in some way represent or be proportional to the supposed quantity of motion of the moving body, and the second being another assumption that none of the motion supposed to be represented by the momentum can be lost by the impact; thus making it appear correct to place the momentum after impact equal to that before impact. And the fact that experiment confirms the truth of the equation, has probably prevented a clear view of the errors involved in the argument by which it was derived.

Ever since the time of Leibnitz there has been more or less discussion among mathematicians and philosophers as to whether momentum or vis viva ought to be taken as the measure of the quantity of motion of a moving body. And it is not difficult to see how the fact that momentum after impact of inelastic bodies is equal to that before it, might, without a clear knowledge of the laws of the correlation of forces, leave some chance for such discussion. The greater wonder is that after those laws have been so long established we still persist in defining momentum as a quantity of motion. Knowing that in every case of impact as here considered more or less change takes place from ordinary motion to molecular motion, and knowing also that after impact the product of the combined mass into its velocity is equal to the product of the mass and velocity of the body moving before impact, how can we posssibly admit that those products can in any way represent or be proportional to the supposed quantity of motion in the two cases? And yet, momentum is defined as a quantity or amount of motion, in our best dictionaries, general and mechanical; in such works as Bartlett's Analytical Mechanics, Peck's Elements of Mechanics, Silliman's Physics, and in most if not all the popular works on Natural Philosophy.*

May it not also be that a great part of the obscurity of this subject is owing to

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* Cooke's Chemical Physics does not so define it, although using the argument before quoted.

some logical absurdity in the very expression *quantity of motion?* Is not motion essentially a *condition* of bodies, rather than a *quantity* of anything which they can possess? Is it proper to say that one body has two or three times as much of that condition as another body? It is correct to speak of the *rate* of motion, or velocity of one body, as being twice as great as that of another, for their velocities can be compared in terms of the same unit. It is also correct to speak of the accumulated energy for overcoming and moving against resistance, or producing motion in other bodies, which a moving body possesses by virtue of its inertia, as being twice as great as that of another body; for here again comparison may be made in terms of a common unit, viz. the foot-pound. And this energy we know to be represented by the vis viva. What need or possibility is there of a *quantity of motion* different from one or both of these? And if there can be such a quantity, what is its unit?

The compound unit of vis viva is admissible, because a pressure of one pound exerted through a distance of one foot can be made to exert, and does exert in nature, the same pressure through the same distance, or if the pressure of one pound for a distance of one foot be neutralized by a pressure of one-half a pound the latter must be exerted through a distance of two feet; or if the pressure of one pound for a distance of one foot be neutralized by a pressure of two pounds the latter will be exerted through only one-half a foot; and so on, the product of the pressure multiplied by the distance through which that pressure acts, in neutralizing a pressure of one pound for a distance of one foot, being always equal to unity. But if we were to compare the motion of two bodies with different velocities, and assume that their quantities of motion could have such a complex unit as a unit of mass moving through a unit of distance in a unit of time, we should find it inadmissible in fact. For if a body could have a quantity of motion it ought certainly to be equal to that which the body could impart to another body by coming to rest itself, supposing there were no loss from the production of molecular motion in the transfer. But a unit of mass with one unit of velocity cannot possibly by coming to rest impart a double velocity to one-half the unit of mass. And if all the work which a unit of mass with a unit of velocity could possibly perform while coming to rest were employed in imparting velocity to one-half a unit of mass, its velocity would only be equal to $\sqrt{2}$. Whereas, if all that work were performed in imparting velocity to a double mass the velocity produced would be equal to $\frac{1}{\sqrt{2}}$. In neither case would the product of the mass acted on, multiplied by the velocity imparted, be equal to unity. The supposed unit could not then be admitted as a true standard of comparison of quantity of motion. Is it then possible to find a proper unit for such a quantity? And if not, is it not absurd to speak of a quantity that has no conceivable unit?

Should we not at all events restrict the word momentum, as applied to the product MV, to its proper and demonstrable meaning, or else discard it altogether? In fact that product is hardly more worthy of a special name than $\frac{1}{4}MV$, or any other fraction of MV, since it measures nothing except under the condition of acting for one second of time; and in practical cases it would be difficult, if not impossible, to apply a constant opposing force of just that intensity to any moving body. The fact that momentum before and after impact is the same, is not of so much value as the more general truth that the constant pressure which the combined mass after impact could exert while coming to rest in *any time*, is equal to that which would have been exerted by the body previously in motion, if brought to rest by a constant force in an equal time. This may easily be demonstrated by dividing the equation between the momenta by the number of seconds in the time. Each member of the equation will then represent the number of pounds pressure required to bring the respective masses to rest from their respective velocities, in the given time. And this fact may be worth knowing, although the work which the combined mass can perform after impact will be less than that which the body first moving was capable of performing, in proportion as the velocity is less.

NOTE ON "RECENT EXPERIMENTS ON FLOW OF WATER IN RIVERS AND CANALS."

By CLEMENS HERSCHEL, C.E., of Boston.

Written for Van Nostrand's Engineering Magazine.

We are told that the first time a diversity of language was shown to be very conducive to misunderstandings and other unpleasantness among men, was at the building of the Tower of Babel, and the experience of the ages that have passed since then, have presented some notable examples of the same causes, producing the same effect, so also in our own day and in our own limited circle of acquaintance.

To illustrate: I have been a diligent reader of the professional literature of this country, of England, France, and Germany, for the past fifteen or twenty years, and can recall an almost continuous series of such instances. Perhaps the latest, and a much to be regretted example, is the one in the last number of this Magazine, in which General H. L. Abbot gives expression to what would be righteous indignation, were the cause of it correctly represented, which it is not. In view of the eminent standing, professional and official, of all the parties concerned, no less than from a general desire to act in the interests of peace and good will, I have thought it proper to endeavor to clear up some misunderstandings; also to say a word to American readers, that may lead them to honor and respect the name of his Excellency, Dr. G. Hagen,[*] late (Ober-landes-baudirector) chief of the engineering department of the kingdom of Prussia and its provinces, as it is honored and respected by probably all who have ever read his works, or who know of his eminent services.

Gen. Abbot writes that the remark that Humphreys and Abbot's "results were altered in part to establish" (accommodate themselves to, or to fit, would have been a better translation)—to fit "the theory proposed," "implies a dishonest tampering with the field-notes—a suggestion which, whether made by Mr. Bornemann or Mr. Hagen, or by any other person is absolutely false and libelous." Such, however, is not my understanding of the phrase quoted, especially not in the original, where the word "data" would have been used in the German, instead of "results," had the charge of changing field-notes been implied or brought forward. But Dr. Hagen is not the man to bring any charges lightly or in a wrong spirit, and I shall endeavor to show now, just what he has said on the subject matter. I pass by anything said by Mr. Bornemann, for he clearly refers only to what had already been said by Dr. Hagen, and does not add anything new.

Dr. Hagen first wrote about the book—"The Physics and Hydraulics of the Mississippi River," in November, 1867, until which time, as he himself says, he had been unable to get a copy of the work in the original, and did not wish to use the translation. (See Z. fur Bauwesen, 1868, p. 63.) The parts of this work that have most attracted the attention of German hydraulic engineers, are the one that treats of the relations between the slope of a river, its hydraulic mean depth, and its mean velocity, and the one that describes the curve or scale of velocities in any vertical. Dr. Hagen does not accept, as *the final and true general law, applicable to all streams*, either of the formulae proposed, as representing these two relations. He does not think that these are the best formulae for that purpose that will ever be found, nor as good

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[*] Dr. Hagen must be now nearly, or a little more than, eighty years of age. In his youth he was a pupil of the celebrated astronomer Bessel; passing from this study to that of engineering, more especially of hydraulic engineering, it has been his life-work to introduce into the practice and study of the engineering profession the same accuracy, logical methods and careful weighing of scientific testimony, that have enabled astronomy to take the high rank she holds among the sciences. He must have been fully fifty years in the Prussia State service, and finally attained the position and rank above mentioned, that is, Chief of the whole Engineering Service of the kingdom, a position analogous to that of general of the army—the U. S. Army, and equally honorable. Upon his retirement from active service last year, the Emperor of Germany, by special decree, conferred upon him the title of "Excellency." That his mind has lost nothing of its original vigor, is plainly to be seen from his last work, "Researches on the Uniform Flow of Water," Berlin, 1876, containing a digest of all that has been done in this line of study to date, and offering to show, not so much a solution of the question, as the way in and by which the perfect answer, if at all attainable, is to be found in the future.

as can even now be proposed, and in fact, somewhat discourages the idea, by his later writings, of there being any one set of formulae hidden in Nature that will fit all streams large or small, steep and of gentle inclination, with equal accuracy. "But," says he, in the article above mentioned, "if these supposed theories are not to be accepted as such, this will nevertheless cast no doubts upon the observations (the field-notes) themselves; on the contrary, we must conclude from the lack of coincidence of these latter with the former, that they have been communicated to us, completely and without change. It can also not fail to strike the reader, from the description given of the methods employed, that the observations were conducted with thought, and in an expert manner. They rank unequivocably among the best hydrometric observations that we possess, and the circumstance of their having been made upon so mighty a river as the Mississippi, only gives them the greater value. But it is to be regretted that the observations (alluding now to those taken for determining the curve of vertical velocities, pp. 230–232, Humphreys and Abbot's book) were not printed as made, but are recorded by averages; also that some details were not described, which, nevertheless, are by no means without importance."

I think that enough has been said to prove that neither Dr. Hagen nor Mr. Bornemann have ever as much as intimated that the field-notes of the Mississippi Survey have been tampered with; as it happens they have asserted, on the contrary, that this evidently had not taken place. And as the remarks, whose misconstruction has led to the misunderstanding here spoken of, occur in the discussion of the curve of velocities in a vertical, and my present object is only to remove this misunderstanding, I shall have to confine myself to this one part of Dr. Hagen's review, however interesting and useful to any hydraulic engineer it might be, to get Dr. Hagen's views on the question of the mean velocity of a river also. In fact, the whole work mentioned in the foot-note ought to be translated into all the principal languages, as a standard guide for the profession, and so as to be within reach of hydraulic engineers the world over. But this is not the place to attempt anything of the sort, in however abbreviated a manner it might be done. I cannot even reproduce here, in translation, the whole of Dr. Hagen's discussion of the experiments, and the method of reducing them, that led to the finding of the curve showing the scale of velocities in a vertical, and its equation, as shown pp. 230–234, of Humphreys and Abbot's book. It occupies three quarto pages in the German. Suffice it to say, that Dr. Hagen gives good reasons for rejecting, first, the *methods* used for *reducing* the observations (which lead to what are called in the German the "results" of the observations) ; and secondly, these results themselves, as given by the parabolic curve and its equation.

After rejecting, further, any hypothesis, that certain observations were suppressed, or simply *not used*, because they did not agree with the theory proposed (a very mild form of sin in scientific research), and not even intimating the least suspicion of such a thing as altering a record once made (a gross form of sin in any sort of business), Dr. Hagen goes on and says (p. 69 of the article of 1867, above referred to): "There is, however, another and very probable explanation of this (remarkable) coincidence (of theory with the observations). Let any one try to draw curves of the described properties, so as to fit the plotted observations above given, and he will find that this procedure is one admitting of the greatest latitude. (The irregular observations shown in the plot are: the 4th, the series of Group I; 2d, series of Group II; and 3d, series of Group III; pp. 230–232, Physics and Hydr. Miss. River). Very dissimilar curves have all an equal validity. Each curve may be changed throughout its length, or only in part, without introducing any error. In this way it became an easy matter, to cause the observations to fit a presupposed form of curve within any desired degree of accuracy. It would not even have been difficult, to draw the curves in such way, that the mean values would have coincided with the calculation to seven places of decimals or closer still." And similar language, together with a reference to his previous writings, is used in Dr. Hagen's book of 1876. It is to one or all of these that Mr. Bornemann has referred, so that the remark in this Magazine, quoted at the beginning of this note, and replied to by Gen. Abbot, has reference to a little piece of self-deception, practiced by those who *reduced* the observations, upon themselves, and caused by using curves plotted by the eye, with only a limited number of points for a guide, as a means of deducing laws from observations, where the employment, in a rigorous manner, of the rigorous method of "least squares," would have avoided this difficulty, and in nowise referring to so much as a suspicion of any changes made in the field-notes.

Finally, I trust that my efforts will not prove the starting point for new misunderstandings; but if so, that their evil effects may be visited upon me alone. I feel that Dr. Hagen has had but a poor apologist in me; his own writings, in their entirety, are his best defence. It has been an ever recurring pleasure to me, for years, to read his writings, as they come from his pen, and I only wish that the honest, conscientious search after the truth, modesty of spirit, and clear conciseness of language which they contain, may some day be made accessible to the English reader, by a good translation.

INDIAN IRRIGATION WORKS.

From "The Engineer."

No one can look upon the vast remains of ancient works of irrigation in India and Ceylon without pondering over the causes which have led to their isolation in uninhabited tracts of country, where wild animals roam at will undisturbed by the presence of man. The question naturally suggests itself as to the object of its construction in such localities, and as to the cause of their total abandonment. It is not a question, however, which need remain long unanswered, for ancient records prove that at the time these works were executed these now desolate regions teemed with human life. The autocrats by whose will these great failures were carried out little reckoned of the cost at which such monuments had to be purchased. In their shortsightedness they saw nothing but the capabilities of the country for producing the staple food, rice, and altogether ignored those sanitary and other conditions which were involved in the massing together of great bodies of forced labor, unregulated by any system which should preserve them in health. Whole districts were, at the simple word of the satrap, laid under contribution for labor, and the large number of people collected were left to shift for themselves, unprovided with shelter or means of cleanliness, and almost without food. At least, it is certain that the latter necessary was doled out with such a restricting hand that the poor wretches, thus forced to hard and continuous labor, were scarcely kept alive by the pittance of food supplied them. The natural consequence of such shortsightedness followed. Imagine probably some twenty thousand men, women, and children huddled together under such conditions, and who can feel surprised that disease broke out among them to such an extent that, spreading from them as a centre, whole provinces became infected, and death swept off its tens of thousands of victims, until a country, once densely populated and carefully cultivated, became an arid, uninhabited waste, and the vast works which were designed to stimulate production were left to go to ruin uncared for and untended.

The size of these ancient works may be conceived from a few prominent examples. The Poonary tank, in the Trichinopoly district, is thirty miles in length, that of Weeranum ten, whilst the Giant's tank in Ceylon has a diameter of about twelve miles. Many of the Bunds constructed to secure these vast inland sheets of water were 40 feet high in places, and the size and girth of the trees which now cover them testify to the many centuries which have passed since their erection, and where nothing is now heard but the roar of the elephant and the sharp, short bark of the leopard. It is in the attempt to re-utilise these vast works of a bygone age that our Government has committed, we consider, one of their leading mistakes. There was the land, and there, with large outlay, would again be the water to irrigate it; but where was the population to cultivate it? It was conceived, and conceived erroneously, that given these advantages, the natives would leave those districts which were overcrowded by a teeming population and emigrate to avail themselves of them. But no being on earth is so conservative in his prejudices as the native of India. Generation after generation squat down under the vines and fig-trees of their forefathers, and their they would rather starve and die from diseases generated from over-crowding than leave the place where they were born. Consequently large sums have been thus fruitlessly expended which might, with a more judicious forethought—which needed scarcely, one would think, the teaching of sad experience—have been laid out upon the improvement and development of smaller works in localities where there already existed a nucleus of population. Noticeable instances of such failure are to be found in the large works at Rukam and Kanthali, in the eastern provinces of Ceylon, where many tens of thousands of pounds have been expended in their restoration, only to find the lands irrigated by them still untenanted, and with but little prospect of the Government ever being recouped its outlay by their sale. Steady application is a rare quality among many of the Indian castes, and they prefer, to the laborious occupation of rice cultivation, the ruinous practice of chena-ing—that is to say, the felling of a few acres of old forest, and the rapid exhaustion of the land by successive crops of dry grain, such land being abandoned when the soil has been made to yield until perfectly sterilized. To such an injurious extent has this practice prevailed, that whole districts have become denuded of forests, and many years must pass before the land so exhausted will produce anything beyond a miserable stunted jungle. The authorities have at last become awakened to the impolicy of permitting such proceedings, which would result in an ultimate scarcity of timber, and the still further depopulation of the country.

The mistakes above described are now pretty generally admitted, and more is being done to foster cultivation in peopled districts by small but judicious outlay on existing used works. In no way can expenditure be better applied than in improving and regulating the distribution of the water contained in the small tanks with which nearly every village is provided. From time immemorial the roughest means have been used for such distribution, and waste and litigation between the village populations have resulted. The exit for the water was usually of the roughest description, a well being sunk in the centre of the "bund," having tunnels below it to either foot of the base of the embankment; and the stoppage of the effluent water was effected by sinking in the well bundles of boughs tied up into a sort of rough fascine. How ineffective such means were under the pressure of a head of water often from 20 ft. to 30 ft., may be readily imagined. Again, when the water passed into the main distributing channels, the only method of its admeasurement to the several lands was by a log of wood crossing the channel, just above the surface level of the water, in which were cut notches, through which only it was able to flow, and the size of such notches bore a relative correspondence to the area of land to be cultivated by each proprietor. To remedy this state of things, amongst other innovators on the old system, Mr. G. L. Molesworth, now chief engineer of the State Railways in India, proposed to the Government of Ceylon the erection of small head and distributing sluices, having iron paddles fitted in wooden frames, which can be readily fixed by almost unskilled labor for a trifling outlay, which has, in every case, been soon recouped to the villagers by the saving effected. It is to such simple means as these that we desire to see the attention of Government directed —not to the total exclusion of more ambitious works, but for the fostering of the economical use of the supply now available to existing communities.

A further mistake is, in our opinion, committed by the over-strict adherence by irrigation engineers to the old methods of native construction. As long as brickwork is confined to foundations, and not exposed to the ravishes of the prolific vegetation of a tropical climate, it is scarcely possible to devise more simple or more efficient work; but it is a different thing when we come to consider the common use of brickwork in the superstructure of dams. The seed of the banian or Indian fig, is probably the most dangerous enemy to such construction, and once let it obtain a foothold in some unfilled joint or cracked plastering, and the destruction of the dam is simply a matter of time and that not of long duration. It is urged on behalf of using brick that a little care exercised—in fact, a stich in time—will prevent disastrous consequences; but no one acquainted with the *laissez aller* frame of mind of an Indian native would like to guarantee such watchfulness. Once let the seed develop, and rapidly a mighty force begins to exert itself, and the parasite increases in bulk, with a speed unknown in more temperate regions, and rends the work from top to bottom. We hold, therefore, that when conditions may admit—and they do so, in our opinion, in the majority of cases—brickwork should not be used in such erections, and even heavy masonry should be avoided when more durable means can be employed. Often have we seen huge solid masses of granite split from top to bottom by the growth of this insidious plant, and there is scarcely a piece of ancient masonry existing which has not been rendered valueless for useful purposes from this cause. We would suggest the freer use of iron in these works when they can be maintained under proper supervision. The use of this material has so largely extended for coffer-dams and other kindred purposes that there is little to be urged against its adoption as we propose, and if Professor Barff's new process can in time be made applicable to moderate sized castings, there is no reason why dams should not be formed of it which would be almost imperishable, whilst the foundations for such superstructure would be much less in extent than are now required. Concrete, too, might be far more used in the form of pisé work in such constructions. Its greater solidity would afford less opportunity for the growth of parasitic plants, and the excellent quality of the Indian limes would make this material especially adaptable. The method by which the Government now recoups itself from the cultivators by annual water rates is, among a people possessing the characteristics of the Indian native, open to many objections. Far from regarding it in its true light as payment for value received, the ryot looks upon it as a tax, and specially obnoxious to them, as are all direct taxes, a prejudice against which has rendered the adjustment of the Indian revenue at all times a matter of great difficulty. It is not easy, however, to point out a means by which this prejudice, which has largely militated against the value of these works, can be overcome in the case of irrigation being applied to lands already in native possession; but we do think it is a feasible suggestion, and one which would greatly tend to remove the difficulty of the present system, if all Government land sold in connection with such works were to have an upset price put upon them, which should include the value of the privilege of water supply. At present, such lands are sold under condition of payment by annual instalments, and the rate added for capitilisation of the annual payment would not, we consider, be severely felt; whilst the investor would feel that his land had the fully increased value, and as being held under a more unconditional and independent title. Colonel Arthur Cotton, whose name must ever be associated most honorably with the endeavor to extend the blessing of irrigation to the people of India, we cannot but consider to have weakened his cause, and led up to many disappointing results by his insistance on the superior merits of irrigation canals in India, as a means of transport. The result of such insistence has been in many instances, a sacrificing of the immediate object of irrigation, the success of which was guarranteed by known facts, to a prospective and visionary advantage; the fallacy of such anticipations having now been demonstrated by sad experience. It is but in few cases that such works, having combined objects, have proved remunerative, as they have generally been carried out on too magnificent a scale. Let our authorities take warning by the impressive lesson taught in the decayed grandeur of the attemps of past ages; and, whilst we rejoice to hear of instances such as are given in the last report of the Madras Irrigation and Canal Company, whose works last year irrigated no less than 90,750 acres, we trust attention will be paid to aiding more fully than hitherto the wants of small detached village communities, which have before been comparatively neglected for the prosecution of grand, but from a monetary point of view, unsuccessful, schemes.

STEAM BOILERS AND ENGINES FOR HIGH PRESSURES.*

By Mr. LOFTUS PERKINS, of London.

From "Engineering."

The object of this paper is to bring before the Institution plans for generating high-pressure steam, say from 250 lbs. to 1000 lbs. per square inch, and working it with great expansion and perfect safety, in conjunction with simplicity and durability. Sixteen years ago the author, conjointly with Professor Williamson, read a paper on this subject at a meeting of this Institution in 1861. The engine and boiler then described have been in use ever since, and recently became the property of a gentleman who for several years has had another boiler and engine on the same system at work. The boiler and engine of 1861 are to be re-erected at the new works of the Sub-Wealden Gypsum Company, at Battle, near Hastings, and are to form part of a steam plant consisting of three sets of boilers and engines, &c., on this system. Since 1861 many improvements have been effected, and are embodied in the engines recently constructed.

In generating steam of the high pressure required to realize a fuller benefit of expansion, it has previously been found impossible to combine in the boiler great strength and safety with durability; if the former are secured, by reducing the internal dimensions and capacity of the boiler, the impurities passed in are fatal to the latter. In working a marine engine which was designed to use water distilled from sea-water, the author found that, although extreme care was taken to separate all the impurities from it before it was introduced into the boiler, the internal surfaces were in the course of time seriously injured. In the same manner, ordinary marine boilers using surface condensation have been injured when there has been an insufficient supply of sea-water to form a protecting scale on the exposed internal surfaces. This led the author to seek for a remedy, which he succeeded in discovering, and adopted with absolute success. This was the use of nothing but fresh water, or distilled fresh water in the boiler, used over and over again, without any admixture of sea-water or the products of seawater, and this was easily accomplished, as the machinery in question had been designed to avoid any leak whatever, and the amount of waste that did take place from glands, &c., was so small in quantity, that no practical inconvenience was found in providing the small supply of fresh water required to make good the waste that did occur.

The means taken to secure the soundness of all the joints and parts of the machinery were the same as those which had previously proved successful in the manufacture of the high-pressure heating apparatus which the author and his firm have been making for upwards of forty-five years, and which has continued to work with the same water with which it was originally charged, without any destructive effect on the internal surfaces. Many sets of this heating apparatus have been working forty years without decay; and some specimens of tubes from the boiler that was described in the former paper in 1861, which were cut out of this boiler for the Admiralty Boiler Committee in 1874, were found to be in such a remarkably good state of preservation that the committee made a special report on the system, which was laid before Parliament, and the specimens referred to are now shown on the

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* Paper read before the Institution of Mechanical Engineers

table by the kind permission of the committee. The committee examined the condition of the boiler and cylinders of the engine at the writer's works, which were opened for the purpose, in the presence of the committee; and found the tubes of the boiler in a remarkably good state of preservation after having been in use nearly thirteen years, and the piston packing and valve rings made of the special metal were found in excellent condition after eighteen months' working without lubrication since last examined.

The possibility of using water which did not injure the internal surface of the boiler enabled the author to design the boiler on a system that combines maximum strength and safety. The horizontal tubes are $2\frac{1}{4}$ in. internal and 3 in. external diameter, excepting the steam collecting tube, which is 4 in. internal and $5\frac{1}{2}$ in. external diameter. The horizontal tubes are welded up at each end $\frac{1}{2}$ inch thick, and connected by small vertical tubes of $\frac{7}{8}$ in. internal and $1\frac{5}{16}$ in. external diameter. The firebox is formed of tubes bent into a rectangular shape placed $1\frac{3}{4}$ in. apart, and connected by numerous small vertical tubes $\frac{7}{8}$ in. internal diameter. The body of the boiler is made of a number of vertical sections, composed each of eleven tubes, connected at each end by a vertical one; these sections are connected at both ends by a vertical tube to the top ring of the firebox, and by another to the steam collecting tube. The whole of the boiler is surrounded by a double casing of thin sheet iron, filled up with vegetable black to avoid loss of heat. Every tube is separately proved by hydraulic pressure to 4000 lbs. per square inch, and the boiler in its complete state to 2000 lbs., this pressure remaining in some hours without showing any signs of leakage. Experience of a very extensive character has proved that this construction of boiler can be worked safely, with great regularity, and without priming, and that the steam produced is remarkable for its freedom from moisture. The area through the vertical connecting tubes is found ample for allowing of the free escape of the steam, and for the prevention of injury from overheating of the tubes in contact with the flame. Injury arising from a prolonged stoppage of the feed supply is a casualty to which all boilers are liable, but with this construction of boiler the small capacity of the sections reduces to a minimum any danger arising from such injury, and facilitates rapidity of repair.

The engine has three cylinders; the first is a single acting high-pressure cylinder, and the second also a single acting cylinder, four times the capacity of the first; these two cylinders are bolted together in the same straight line, and have a common piston rod. The third cylinder is double acting, four times the capacity of the second, and its piston-rod is connected to a crank at right angles to the other crank.

Having safely generated steam of high pressure at say 350 lbs. per square inch, a serious difficulty has to be overcome in using it, from the high temperature affecting the lubrication of the pistons and packing of the glands. This difficulty the author has succeeded in overcoming by introducing the high-pressure steam into the upper end of the first cylinder, where there is no gland, and where the piston is formed so as to require no lubricating material. The steam is cut off at about half stroke in this cylinder, and when it is admitted for the return stroke into the bottom of the second cylinder, of four times the area, the temperature is so much reduced as to cause no difficulty when brought into contact with the piston-rod gland. From the bottom of the second cylinder the steam expands into the top of the same cylinder, which is of larger capacity than the bottom, and serves as a chamber, and is in direct communication with the valve box of the third cylinder; this last is double-acting, and is arranged to cut off at about a quarter stroke, and at the termination of the stroke exhausts into the condenser, with a total expansion of about thirty-two times. All the cylinders are jacketted with wrought-iron tubes, which are cast in the metal, and supplied with steam direct from the boiler, the condensed water from the jackets being conveyed to the hot well. The whole of the cylinders and valve boxes, &c., are enclosed with a double case of thin sheet iron, filled in with vegetable black to prevent the escape of heat, and at the same time to maintain all the parts at a high temperature.

In working these high pressures with great expansion the ordinary mode of packing the pistons was found unsatisfactory, and to overcome the difficulty the compound piston was devised. The prevalent scoring and cutting of engine cylinders was effectually remedied by the discovery of the compound metal, of which the packing rings are made, which requires no lubricating material. Many cylinders fitted with piston rings made of this metal have been several years at work, and have been often examined, the cylinders showing no signs of wear, the wear taking place on the rings only, which may be easily and inexpensively renewed as required, and experience has proved that with these pistons, the longer an engine is worked the more perfect does the surface of the cylinders become, and the less wear results to the packing rings. This metal for piston packing rings is composed of five parts tin and fifteen parts copper, and has since been used by several other makers for ordinary engines with great success. When this metal is used, no oil or grease is required to lubricate the cylinders—a great advantage, particularly where the engines are fitted with surface condensers.

The high-pressure pistons in the steamers Atacama and Coquimbo of the Pacific Steam Navigation Company were fitted with these packing rings, and it was reported by the superintendent engineer that the cylinders, which were previously rough and slightly grooved, were in the course of two or three voyages, or about 10,000 miles' run, brought up to a beautiful smooth surface, and had since kept in capital order, giving no trouble whatever. After having been once brought up to a smooth working surface, the packing rings did not wear the cylinders; the wear of the rings was also very slight, and the friction greatly reduced, and one-third of the lubrication necessary for cast-iron rings was found sufficient. In the torpedo vessels made for the French Government, Messrs. Thornycroft found these packing rings for the engine pistons a great advantage, as there was no chance of the cylinders being scored; and they were enabled to run the two hours' trial easily, at the high speed of about 430 revolutions per minute, without using any oil or grease in the cylinders. In an engine at the Dorking Grey Stone Lime Company's works, the manager reported, after 2½ years' use of these packing rings for the piston, that they required no grease of any kind, and worked the cylinders to a polished face and needed no looking to until worn out; a set of rings lasted about 100 days, working at the usual high steam pressure of 400 lbs. per square inch.

The surface condenser used is constructed of a number of vertical tubes in such a manner as to be absolutely tight, so as to insure that the condensing water inside the tubes shall not mix with the water from the condensed steam outside them. The tubes are ⅞ in. internal and $1\frac{5}{16}$ in. external diameter, welded up at the top end and fixed securely in a tube plate at the bottom. These tubes are fitted with internal tubes, open at both ends, which are fixed in a division plate at the bottom, in order to cause the condensing water to circulate to their extreme ends.

A small still, worked by a steam coil, is used to distil water for replenishing any small waste that may take place in the feed supply. A duplicate apparatus forms part of the ordinary equipment of a sea-going vessel, to furnish steam from sea-water, for blowing the steam whistle, cooking, supplying distilled water for use of passengers and crew, and for all other purposes where distilled water is required.

In designing the machinery described, provision is made for passing any waste steam from the safety valves, &c., into the surface condenser, and the great strength of the boilers allows a margin of 100 lbs. per square inch or more to exist between the load on the safety valves and the pressure required to work the engines. When this system is fully carried out in steamships, the author would deem it quite safe, and more than ample for making good the waste of water from all sources, to provide, beyond the water in the boilers, a supply of fresh water in the proportion of ten gallons per twenty-four hours per 100 indicated horse power. As an instance of the practical feasibility of carrying out the system of machinery that has been described, it may be stated that a boiler containing only 300 gallons, and an engine working at 250 lbs. pressure and 250 indicated horse power, were worked night and day continuously thirteen days (one Sunday excepted) without requiring any addition to make good the waste of working, nor at the end of the trial was there any appreciable difference in the water level of the boiler.

In the indicator diagrams exhibited, the two upper diagrams are taken from the working of a pair of marine engines on this plan of seventy nominal horse power, and the coal consumed averaged 1.62 lbs. per indicated horse power per hour. In this case there was no vacuum and no low-pressure cylinder, and the terminal pressure was 21 lbs. per square inch above the atmosphere; the boiler pressure was 300 lbs. per inch. With the addition of a low-pressure cylinder and a vacuum, the author considers it may safely be estimated that this system, properly carried out, will realize an average duty of one horse power for each pound of coal per hour.

REVIEW OF THE FOREGOING FROM "ENGINEERING."

Every engineer who has had an opportunity of examining the high-pressure engines and boilers described in Mr. Perkins' paper must, we think, have been struck with the perseverance and ability which the author has brought to bear upon their construction. Steam at pressures of from 300 lbs. to 500 lbs. per square inch is not a fluid to be trifled with, and it is only by the adoption of many novel structural expedients and the employment of the very best workmanship that Mr. Perkins has been able to attain the results he describes in his paper. It may, however, now be taken for granted that Mr. Perkins has proved that engines of moderate size may now be made upon his plans and successfully worked with steam of the pressures named, and the chief points which now appear to us to be open to discussion are: What are the real advantages gained by the use of steam of excessively high pressures? and, second, Can the results attained with engines of moderate power be repeated on a larger scale and under the conditions as to weight, &c., which must be satisfied in the case of marine engines? This latter branch of the question, however, we do not intend to enter into here; and in the present article we shall, in fact, confine ourselves to discussing some of the points included in the first query.

What then are the advantages to be gained by the adoption of steam pressures of, say, 500 lbs. per square inch? The advantages claimed by Mr. Perkins may, we think, be summarized as follows: (1) That by resorting to such pressures we are enabled to use a type of boiler which could not otherwise be employed, this boiler being, it is said, durable, easily manufactured, and safe; and (2) that by using the high-pressure steam, and employing large ratios of expansion, the work can be done with a much less expenditure of fuel than is now required by our best marine engines. Let us consider each of these claims in the order in which we have mentioned them. First, then, as to the boiler. It may, we think, be at once conceded that with steam at moderate pressures it would be quite impossible to successfully work a boiler constructed on Mr. Perkins' system. It is only by employing very high pressures, and thus causing the steam generated to occupy a small volume, that the escape of the steam from the lower pipes to the steam space is rendered possible. But the volume occupied by the steam in the tubes depends not only upon its pressure but also upon the rate at which it is being generated, and it is easy to imagine conditions under which the rate at which the steam is generated may exceed that at which it can escape to the higher tubes, thus involving a risk of failure. It is in fact evident that the durability of the boiler must be largely affected by the rate at which it is worked, and hence the statement that a boiler has lasted so many years loses its value unless we are at the same time informed what work it has been doing. It is much to be regretted that Mr. Perkins' paper fails to give us definite information on these points. An ordinary marine boiler evaporates very commonly 5 lbs. of water per hour per square foot of heating surface, and very often does much more, and what we require to know is whether in the Perkins boiler the rate of evaporation considered desirable is higher or lower than this, and also how much water can be evaporated per pound of coal when working under what are considered by the designer to be proper conditions. Mr. Perkins will, we feel certain, add much to the value of the facts stated in his paper if he would furnish definite information on these points. To be of value the evaporative results per pound of fuel should be deduced from careful trials lasting at least ten hours, both fuel and water being carefully weighed, and precautions being taken to avoid loss of water by priming. We may add that in comparing Mr. Perkins' boiler with the ordinary marine type it must be borne in mind that the former is, from its small water capacity and other features, lighter than the latter in proportion to its exposed surface, and thus to do a certain amount of work it would be possible for Mr. Perkins to employ, without necessarily incurring excessive weight, a greater area of heating surface than is adopted in ordinary practice.

Next as regards the question of the economy alleged to be attained by the employment of steam at excessively high pressures worked with large measures of expansion; and here we may state at once that, to say the least, we consider this economy to be very generally vastly overrated. The statement made by Mr. Crampton in the course of the discussion on Mr. Perkins' paper, that in case of a certain pumping engine he had obtained with steam of 30 lbs. to 40 lbs. pressure, expanded about five-fold, as good results as with steam at higher pressures used more expansively, appeared to be received by some of the members present with a certain amount of incredulity, yet we believe the statement to be quite consistent with other experiences. To avoid misaprehension it will be advisable that we speak more fully on this point. It must be remembered that in Mr. Crampton's case the load on the engine was constant, and thus the mean effective pressure in the cylinder must also have been constant (neglecting any small differences of friction), whatever the pressure of steam employed might be. With steam at 35 lbs. pressure, or, say, 50 lbs. absolute, expanded five-fold, the total mean pressure would be 26 lbs. almost exactly, while to obtain the same total mean pressure with 70 lbs. steam, would require an expansion of a little over eleven-fold. Mr. Crampton's statement thus amounts to this, that in a single-cylinder condensing engine 35 lbs. steam expanded five-fold more economical—or at least as economical—as 70 lbs. steam expanded eleven-fold, and in making this statement we believe he is practically right. A confirmation of the view here taken is afforded by the experiments of Chief Engineer C. H. Loring and Mr. Charles Emery, on the engines of the U. S. Revenue steamer Gallatin, recorded in our number of February 18, 1876, and by those of Mr. Emery, on the engines of the Bache, of which we gave an account in our issue of January 1, 1875. In the case of the Gallatin trials, the engines were worked with steam at different pressures, and it will be seen on referring to the record of the experiments that when working with about five-fold expansion an increase in the steam pressure from 40 lbs. to 70 lbs. per square inch gave an economy of but about 10 per cent. only, while in the case of the Bache (when the engines were being worked non-compound) an increase in the ratio of expansion from 5.1 to 12.6 (the initial pressure being 80 lbs.) resulted in an increase of steam consumption by about 16 per cent. Coupling these facts we have good evidence in support of Mr. Crampton's statement, and we are warranted in concluding that with the steam of higher pressure the large amount of expansion employed would cause a loss more than counterbalancing the gain in other respects. Had Mr. Crampton been able to increase the load on his engine at the same time that he increased the steam pressure, he would no doubt have got better results at the higher pressures than at the pressures of 30 lbs. to 40 lbs. which he found most beneficial under the circumstances, while had the engine been of the compound type, he would have been able to use larger ratios of expansion with advantage. All recent researches into the action of steam in the steam engine tend to show conclusively that the extent to which steam can be benefically expanded in a single cylinder—even if that cylinder is steam jacketted—is far less than used formerly to be supposed, and that if we are to employ large measures of expansion we must divide the work between two or more cylinders, so as to limit the range of temperature in each. This view was maintained by Mr. Adamson during the discussion on Mr. Perkins' paper, and there can be little doubt of its correctness.

But while we have evidence that by the employment of the compound system we may benefically use measures of expansion not otherwise desirable, we are as yet without experimental data to show us the limit to which we can advantageously go under such conditions as Mr. Perkins proposes to introduce. All we know is that the economy actually obtained by the adoption of high pressures and correspondingly large measures of expansion increases far less rapidly than theory might at first lead us to expect, and reasoning from analogy we see good reason to anticipate that at pressures lower than those advocated by Mr Perkins the advantages derivable from the adoption of a large increase of expansion *per se* are more than counterbalanced by the losses due to increased radiation and variations of temperature in the cylinders, to say nothing of leakage past pistons and kindred matters. Under these circumstances it is we think very desirable that statements as to the economy derivable from the employment of steam of very high pressures should be founded on a sound basis. So far as we have been able to gather from Mr. Perkins' paper, and the discussion which succeeded it, no experiments have yet been made which justify this economy being stated as an absolute fact. Mr. Perkins appears to have calculated the consumption of steam per horse power per hour from indicator diagrams, but such a mode of calculation is entirely untrustworthy, as all who have conducted many engine trials well know. Considering that several of Mr. Perkins' engines are now at work, it ought not to be difficult to arrange an exhaustive trial which would afford data at present entirely wanting, and which it is necessary should be available before the merits of Mr. Perkins' system can be correctly judged. In conclusion, we may say that in making these remarks we in no way desire to criticise Mr. Perkins' system adversely. The skill with which Mr. Perkins has developed the use of high pressure steam deserves every recognition at our hands; but at the same time we believe that we are serving the best interests of Mr. Perkins, as well as of the public, by directing attention to some points connected with that system which are at present doubtful, and which should be cleared up without unnecessary delay.

RELATION AMONG THE ULTIMATE RESISTANCES OF MATERIAL TO TENSION, TO COMPRESSION, AND TO CROSS-BREAKING.

By JOHN D. CREHORE.

Written for VAN NOSTRAND'S MAGAZINE.

So long ago as 1842 the indefatigable Hodgkinson communicated to the British Association for the Advancement of Science, as one result of his experiments, the discovery of a "direct relation between the conditions of rupture by transverse and by longitudinal strain." The first three columns of the following table, contain a summary of his results as given by Mr. Hodgkinson himself, and may be found in many textbooks on engineering:

Material.	1	2	3	4	5
Timber....................	1000	1900	85.1	1531.8	1343.5
Cast-Iron..................	1000	158	19.8	356.4	323.6
Stone......................	1000	100	9.8	176.4	230.9
Glass......................	1000	123	10	180	269.7

Column 1 contains assumed crushing strength per square inch.

Column 2, mean relative tensile strength per square inch.

Column 3, mean relative transverse strength of a bar one inch square and one foot long, loaded at the center.

If we multiply each number in column 3 of this table, by 18, we derive the corresponding number in column 4, which is thus made to represent the relation of the "modulus of rupture" to the ultimate resistances to compression and tension, whose ratio is given in the 1st and 2nd columns.

To explain this change, let

W=breaking weight suspended at center of bar.

B=the modulus of rupture required.

Then, since each bar is one inch square and one foot in length between supports, we have the moment due the external forces,

$$M=\tfrac{1}{4}W\times 12=3\text{ W};$$

and the moment due the internal forces,

$$R=\tfrac{1}{6}B=M=3\text{ W}.$$

Whence $$B=18\text{ W}.$$

Column 5 contains relative values of the moment of rupture as computed by formula (5) below; from the assumed numbers in the 1st and 2nd columns of the same table.

It should be remembered that no one of the numbers in this table, necessarily represents the actual ultimate resistance of the given material, to tension, compression, or cross-breaking. They are abstract numbers, merely denoting ratios which the experimenter deduced from such trials as he had made. Other laborers have since wrought in the same field; and it is the object of this paper to give a formula which shall express the relation we are seeking, in accordance with a wider range of experimental facts than have been determined by any one observer; and by this means to clear up certain doubtful points, and remove some confusion which mars the pages of a few of our best instructors.

For instance, in the well known and most admirable work of Mr. Bindon B. Stoney on the Theory of Strains, we find at the 84th page, article 131, these statements:

"The student will naturally conclude that the formulæ investigated in the present and preceding chapters should give identical, or nearly identical results, when they are applied to the same girder; that, for instance, the breaking weight of a solid rectangular semi-girder, calculated by the equation $Wl=adS$, should closely agree with its breaking weight calculated by the equation $6Wl=fbd^2$; and, if our theory were complete, this would no doubt be the case. To test its accuracy, let us compare these two equations, when we obtain this result,

$$S=\tfrac{1}{6}f,$$

that is, the value of S for solid rectangular girders of any given material should equal one-sixth of the ultimate tearing or crushing strength of that material, according as it yields by tearing or crushing. In many instances, however, this will be found to be far from the truth; for example, the value of S for small rectangular bars of cast-iron = 3.4 tons, and 6 times this = 20.4 tons, far exceeds the tensile strength of ordinary cast-iron, which is about seven or eight tons per square inch. It must, indeed, be confessed that the law of elasticity ceases to be applicable when we approach the limits of rupture; and that the formulæ for *solid* girders investigated in the present chapter give their breaking weight much under what it really is for many materials, and this discrepancy will probably be found more marked in those whose ultimate tearing strain differs widely from their ultimate crushing strain. Greater confidence, however, may be placed in the formulæ relating to hollow and flanged girders.

Mr. Hodgkinson endeavors to explain this discrepancy by a change in the position of the neutral axis, as soon as the limit of elastic reaction of the horizontal fibres has been passed, and gives some reasons for this hypothesis derived from experiments on cast-iron, in his *Experimental researches on the strength of cast-iron*, p. 384. This seems a plausible hypothesis, for if the neutral axis of a solid rectangular cast-iron girder approach its compressed edge as the weight increases, and, after the limit of tensile elasticity has been passed by the fibers along the extended edge, we shall have a larger proportion than one half the girder subject to tension, and consequently the total horizontal tensile strain may exceed that derived from our theory, which assumes that the neutral axis always passes through the center of gravity of the cross-section.

These views of Hodgkinson are corroborated by experiments made by Duhamel, and also by similar experiments of the elder Barlow; but Mr. W. H. Barlow controverts these views, and gives the results of micrometrical measurements, on two cast-iron rectangular girders which he subjected to transverse strain, and infers from these experiments "that the neutral axis does not shift its position, and this view seems in accordance with experiments made long ago by Sir David Brewster," on polarized light transmitted through a rectangular glass girder.

I shall make no attempt to settle this dispute by preponderance of authority, but shall simply enunciate the law which seems to me axiomatic and inevitable *a priori*, and then apply it to the interpretation of the results of actual experiment, leaving the truthfulness of the law to be inferred from its accordance with observed facts.

I shall here consider only the case of a solid rectangular beam, and let us take

h=depth of beam,
b=breadth of beam,
l=length of beam,
x=distance of the neutral surface from the *compressed* side of the beam.
C=ultimate resistance of the material to crushing by direct thrust, in pounds per square inch.
T=ultimate resistance of the material to extension, in pounds per square inch.
B=the unit strain which, at the instant of rupture, would be developed at the upper and lower surfaces of a beam having its neutral surface midway between these outer surfaces; that is, B=the modulus of rupture, also in pounds per square inch.

Then we have, for the compressed part of any cross-section, moment of internal forces.

$$M=\tfrac{1}{6}Cbh^2;$$

and for the extended part of the same cross-section,

$$M=\tfrac{1}{6}Tb(h-x)^2.$$

Now since, in the nature of things, or upon the principle of sufficient reason, these two moments must be equal to each other, and each also equal to $\frac{1}{6}Bb(\frac{1}{2}h)^2$, we have

$$Cx^2=T(h-x)^2=B(\tfrac{1}{2}h)^2 \quad . \quad . \quad (1)$$

Whence,

$$x=h\frac{\sqrt{T}}{\sqrt{C}+\sqrt{T}}=\tfrac{1}{2}h\sqrt{\frac{B}{C}} \quad . \quad . \quad (2)$$

$$C=\frac{B}{\left(2-\sqrt{\frac{B}{T}}\right)^2} \quad . \quad . \quad . \quad . \quad . \quad (3)$$

$$T=\frac{B}{\left(2-\sqrt{\frac{B}{C}}\right)^2} \quad . \quad . \quad . \quad . \quad . \quad (4)$$

$$B=\frac{4C}{\left(1+\sqrt{\frac{C}{T}}\right)^2}=\frac{4T}{\left(1+\sqrt{\frac{T}{C}}\right)^2} \quad (5)$$

When, therefore, any two of the quantities C, T, and B, are given, the third may be found, and also the position of the neutral surface.

The following table shows the result of applying these formulæ to cast iron, wrought iron, steel, wood, and stone, using such experimental values for two of the three quantities, as are to be found in the works of Rankine, Stoney, Moseley, Mahan, Weisbach, Vose, Ure, Haswell, &c.

(*See Tables on following pages.*)

The value which has no initial annexed, is the one that has been computed by formula. The authorities are thus abbreviated:

H. Hodgkinson,
S. Stoney,
Ha. Haswell,
F. Fairbairn,
Bv. Bevan,
Bu. Buchanan,
B. Barlow,
D. Denison,
W. Weisbach,
R. Rankine,
N. Nelson,
M. Moore,
K. Kirkaldy,
Ro. Rondelet,
Mo. Moseley,
Re. Rennie,
C. Clark.

In the case of cast-iron, an empirical coefficient $(1+m)$ whose mean value seems to be about $\frac{10}{9}$, is required to make the value of B as determined by formula, accord with the results of experiment. This is equivalent to giving the same coefficient to C and T, and is supposed to be required by reason of the greater resisting powers of a cast-iron bar at its surfaces than in its central parts.

It will be observed that Stirling's Nos. 2 and 3, Devon No. 3, (hot blast), and Coed Talon No. 2 (cold blast), irons of high tensile strength, require no increment, but give B a value fully up to the extreme experimental number.

The first 9 cast-irons of this table yield Hodgkinson's proportionals almost exactly, with $\frac{10}{9}$ for the coefficient of B.

The names of the 16 irons whose mean strengths are given in the 13th line of the table, may be found in Mr. Stoney's volume, pages 228 and 289. But as I have nowhere found the moduli of rupture due those 16 irons, I insert Mr. Fairbairn's extremes beside the augmented value of B.

When I had advanced, in the application of these formulæ, through cast-iron, wrought-iron, steel, and wood, there fell into my hands Professor Thurston's article on the Flexure of Beams, appended to Du Bois's translation of Weyrauch; and here I learned that the law which I had called "axiomatic and inevitable *a priori*," is not now first discovered, but is "an early hypothesis of Navier, which seems to have been entirely abandoned by him subsequently, and which has not been accepted by subsequent writers on the subject." This last remark may partially excuse my ignorance of the existence of such an hypothesis; and the following words of Professor Thurston, together with the results of experiment which he brings to my aid, may mitigate the rashness of my endeavor to reinstate the rejected hypothesis:

		Ultimate Resistance to Crushing.	Ultimate Resistance to Extension.	Position of Neutral Surface.	Ultimate Resistance to Cross-breaking.	Increment given to B.		As given by Authorities.
	Cast-Iron.	C	T	$\frac{x}{h}$	B	m	$(1+m)$ B	B
1	Carron No. 2 (cold blast).	106,375 H.	16,683 H.	.28	34,240	$\frac{1}{8}$	38,520	38,556 H.
2	Carron No. 2 (hot blast) .	108,540 H.	13.505 H.	.26	29,520	$\frac{1}{4}$	36,900	37,503 H.
3	Carron No. 3 (cold blast).	115,442 H.	14,200 H.	.26	31,132	$\frac{1}{7}$	35,579	35.980 F.
4	Carron No. 3 (hot blast). .	133,440 H.	17,755 H.	.27	38,129	$\frac{1}{9}$	42,365	42,120 F.
5	Devon No. 3 (hot blast). .	145,435 H.	21,907 H.	.28	45,476	0	45,476	45,278
6	Buffery No. 1 (cold blast)	93,366 H.	17,466 H.	.30	34,044	$\frac{1}{10}$	37,448	37,503 H.
7	Buffery No, 1 (hot blast).	86,397 H	13,434 H.	.28	27,640	$\frac{2}{7}$	35,540	35,316 H.
8	Coed Talon (cold blast). .	81,770 H.	18,855 H.	.32	34,461	0	34,461	33,858 F.
9	Coed Talon (hot blast). . .	82,739 H.	16,676 H.	.31	31,771	$\frac{1}{20}$	33,359	33,145 F.
10	Means of these 9.	105,945	16,720	.29	34,046	$\frac{1}{9}$	37,829	37,695
11	Hodgkinson's Ratios.	1,000 H.	158 H.	.29	321	$\frac{1}{9}$	356.7	356.4 H.
12	W. H. Barlow	110,022	18,750 B.	.29	37,575	$\frac{1}{9}$	41,750 B.	41,750 B.
13	Means of 16 Irons.	86,284 H.	15,298 H.	.30	30,245	$\frac{1}{9}$	33,606	27,297 to 48,195 F.
14	Stirling's No. 3.	144,264 H.	23,461 H.	.29	47,658	0	47,658	27,297 to 48,195 F.
15	Stirling's No. 2.	122,395 H.	25,764 H.	.31	48,426	0	48,426	27,297 to 48,195 F.
16	Weisbach.	146,000 W.	19,592 W.	.27	41,980	$\frac{1}{9}$	46,644	30,000 to 46,900 W.
17	Rankine, means.	112,000 R.	16,500 R.	.28	34,466	$\frac{1}{9}$	38,295	38.250 R.
18	Stoney, means	87,490 S.	16,295 S.	.30	31,806	$\frac{1}{9}$	35,340	30,240 to 45,696 S.

	Material.	Compression.	Tension.	Position of Neutral Surface.	By Formula.	By Authority.
	Wrought Iron.	C	T	$\frac{x}{h}$	B	B
19	Plates	36,000 R.	51,000 R.	.54	42,524	42,000 R.
20	Bars	40,000 R.	68,848 K.	.57	51,522	51,341 C.
21	Stoney, means	40,320 S.	57,555 K.	.55	47,793	30,240 to 74,995 C.
22	Rankine, means	38,000 R.	65,000 R.	.57	48,815	42,000 to 54,000 R.
	STEEL.					
23	Hammered Bessemer	225,568 F.	83,391 F.	.38	128,998	128,083 K.
24	Rolled Bessemer	214,843	71,658 K.	.37	....	115,181 K.
25	Hammered Crucible	314,714	85,546 K.	.34	....	147,840 K.
26	Rolled Crucible	250,702	68,589 K.	.34	....	118,272 K.
	WOOD.					
27	Ash	9,363 S.	16,700 S.	.57	12,246	12,156 S.
28	Ash	9,000 R.	17,000 R.	.58	12,062	12,000 to 14,000 R.
29	Beech	9,363 S.	11,500 S.	.53	10,348	8,280 to 10,434 S.
30	Beech	9,360 R.	11 500 R.	.53	10,348	9,000 to 12,000 R.
31	Birch	9,032 S.	15,000 S.	.56	11,454	8,010 to 12,366 S.
32	Box	10,300 R.	20,000 R	.58	13,965	14,670 S.
33	Cedar of Lebanon	5,860 R.	11,400 R.	.58	7,948	7,400 R.
34	Deal	7,293 S.	12,900 S.	.57	9,505	9,372 S.
35	Deal, means	6,939 S.	12,900 S.	.58	9,237	9,372 S.
36	Elm	10,331 H.	14,400 Bv.	.54	12,113	11,820 DN.
37	Elm	10,331 H.	5,780 B.	.43	7,569	7,850 R.
38	Fir (Spruce)	6,499 H.	12,000 B.	.58	8,627	8,076 M.
39	Fir (Red Pine)	5,787 R.	13,000 R.	.61	8,328	8,320 R.
40	Fir (Larch)	5,570 R.	9,500 R.	.57	7,146	5,000 to 10,000 R.
41	Fir (Larch)	5,568 H.	10,220 Ro.	.58	7,372	8,010 B.
42	Fir (Larch)	3,201 H.	8,900 Bv.	.62	5,003	5,466 D.
43	Mahogany	8,200 R.	14,900 R.	.58	10,811	10,314 to 11,500 R.
44	Oak, means	7,923 S.	15,425 S.	.58	10,754	10,400 S.
45	Oak, means	7,900 R.	13,350 R.	.57	10,105	10,400 R.
46	Oak (average)	7,912	14,388	.57	10,429	10,400
47	Pine, means	6,584 S.	10,634 S.	.56	8,248	8,457 S.
48	Sycamore	7,820 H.	13,000 Bv.	.55	9,377	9,600 R.
49	Teak, Indian	12,000 R.	15,000 R.	.53	13,375	12,000 to 19,000 R.
	STONE.					
50	Granite, means	8,839 S.	586	.21	1,483 S.	1,584 Ha.
51	Sandstone	7,189 Bu.	1,158 Bu.	.29	2,359	1,100 to 2,360 R.
52	Marble, white	6,318 Re.	551 H.	.23	1,314	1,252 H.
53	Marble, black	14,833 S.	1,072	.21	2,664 Mo.	2,697 H.
54	Limestone	9,753 S.	857 W.	.23	2,039	2,007 S.

"The assumption that the resistances vary each way from the neutral surface proportionally with their distance from that surface, is, when coupled with a rejected hypothesis of Navier, nevertheless, not far from the truth in special cases, as may be shown by proper mathematical treatment and comparison with results obtained experimentally."

He then gives the results of experiments made by Mr. William Kent, upon cast and wrought iron and ash timber, at the Stevens Institute of Technology.

His results, reduced to conform to my notation, are as follows:

Material.	C	T	$\frac{x}{h}$	B	m	$(1+m)$ B	BEx.
Cast-Iron..........	96,000	16,000	.29	32,280	$\frac{1}{12}$	34,970	35,000
Wrought-iron......	60,000	60,000	.50	60,000	..	....	60,000
Ash..............	9,000	17,200	.58	12,120	..	....	12,000

The numbers harmonize sufficiently with those I had been able to gather from the various authors I have named; and, moreover, I suppose Mr. Kent's experiments were made by the same hand, and all upon material of the same quality. Such deductions, manifestly, are worthy of more confidence than should be given to ratios deduced from the tests of different observers, applied to different qualities of material; unless, indeed, the number of observations be very great, in which case the mean values of the observed ratios should agree closely with the requirements of the formula.

Whether that perfect and all-comprehensive formula which Prof. Thurston so finely pictures to the mind, shall ever be written, or not, it is evident that engineers need not be led very far from the truth in this matter, if they will correctly interpret the teachings already accumulated (and accumulating) by experiment.

When Mr. Stoney finds conflict between experiment and his equation $6S=f$, he should examine and see if f, *can*, as he has used it, represent anything but B, as I have defined it above. Giving f this value, his equation involves no wide departure from experimental data. Thus, if $S=3.4$ tons, then $f=20.4$ tons$=45,696$ lbs., which, though high, is quite within the limits of observation.

In regard to the permanence of the neutral surface in one position, I offer these queries, viz.

If the neutral surface *does* change its position, does it not also change its length? And if it changes its length, could Mr. W. H. Barlow's "micrometrical measurements" have detected its final position, or any change in its position?

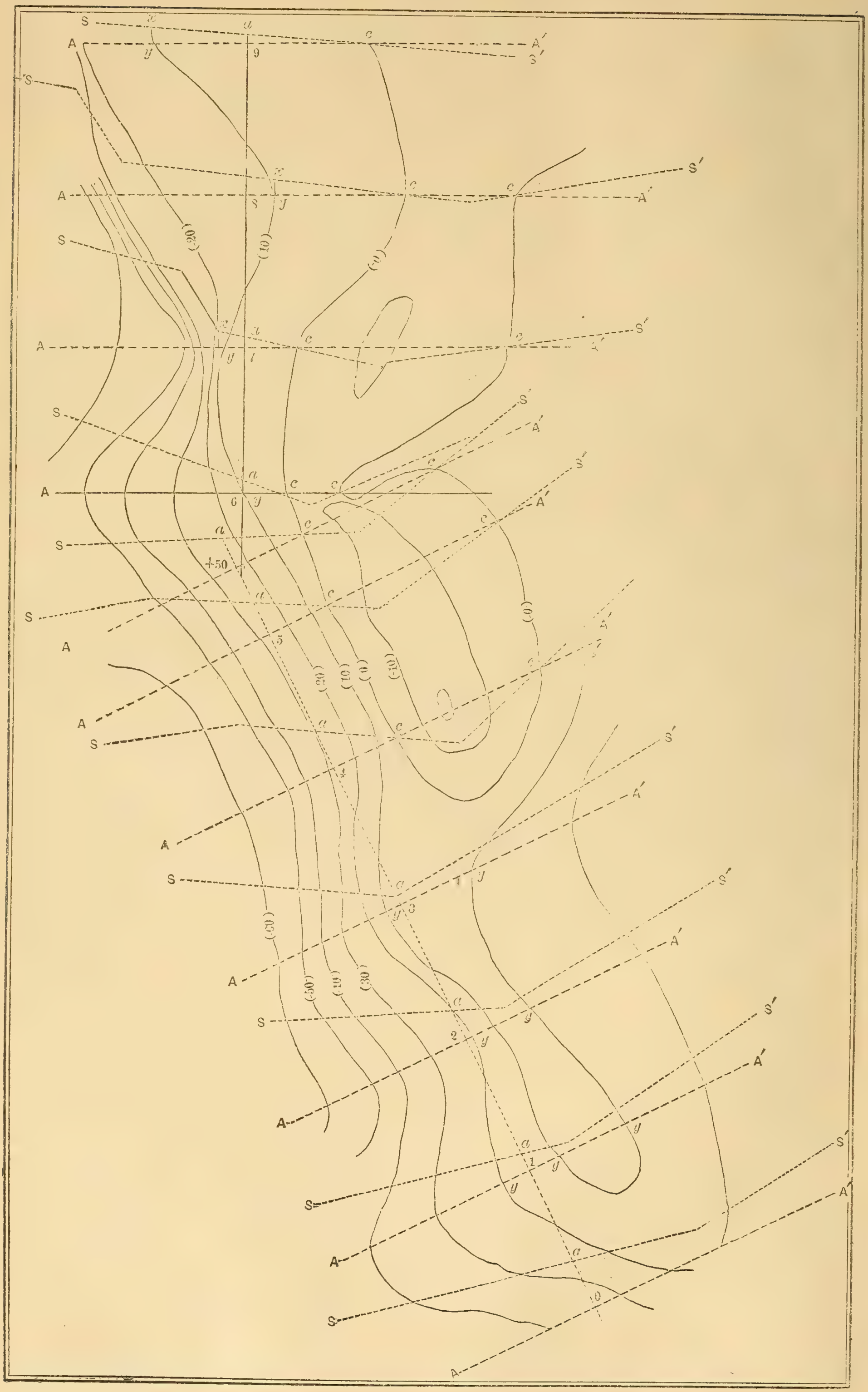
Is there any absurdity involved in supposing the neutral surface to change its position with every increment of strain in rectangular beams whose material yields unequally to tension and to compression? Or, rather, has not each value of the strain a neutral surface peculiar to its magnitude, regardless of the previous or subsequent values of the strain?

CONTOUR LINES IN RAILWAY SURVEYS.

By A. S. HARDY, Professor of Civil Engineering in Chandler Scientific School.

Written for Van Nostrand's Magazine.

Accuracy in the topographical features of the map may often be dispensed with, and on the other hand is often quite important. The art of sketching in these features by the eye may be carried with practice to a high degree of accuracy, so far as the general disposition of the surface undulations is concerned. But where the true position of contour lines is desired, *that is*, *the true relation between horizontal and vertical distances*, no sketch of this character is reliable. The writer has found this especially true with railroad parties in the course of practical instruction, where the topographer could not be depended upon for even general features, and where time was wanting for the delineation of contour lines by well known accurate methods. The following combi-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(-30)
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(-50)
+50

nation of clinometer and profile notes resulted from an effort to obtain from the preliminary survey a map reliable for location in the office. The principles are well known and their combination in the form below may have been before employed, although never having come to the writer's notice. Having plotted the preliminary line (see figure,) draw through each station as 0, 1, 2, 3, &c., lines AA′, AA′ &c. perpendicular to the axis, and lay off the distances $0a$, $1a$, $2a$, &c., along the axis and equal to the "height of surface" at the station in question above the datum plane, taken from the profile look, and at the points a, a, &c., thus determined, plot the cross sections SS′, SS′, SS′, &c., from the clinometer notes. These are taken in the field with a light rod and tape in the usual manner. Then are AA′, &c., traces *on the datum plane* of the vertical planes of cross-section, and SS′, &c., the revolved positions of the intersections of these planes with the surface. If then within the limits of the survey these perpendiculars and slope lines intersect, as at c, c, c, &c., the line ccc, &c., will be the contour line cut by the datum plane. If contours are to be drawn 10 feet apart take a distance $xy=10$ feet (stations 8 and 9) with the dividers between the slope SS′ and the line AA′ at every station. The points x, x, &c., are the revolved positions of points on the contour line 10 feet above ccc, &c., and in true position in plane are at y, y, &c., and yyy, &c., is the contour 10 feet above the datum. Take next a distance 20 feet and repeat. Contour lines may be thus sketched in very rapidly and with remarkable accuracy. The lines AA′ &c., and SS′ &c., may be drawn very lightly. As the same scale is used throughout, should any of the points a, a, &c., be over 100 feet above the datum plane, to avoid confusion when the stations are 100 feet apart assume any new datum plane above the former one. If, for example, the plane be raised 50 feet, the contour line which, referred to the former plane was (50), will be continued as the (0) contour for the new plane. Whenever the transverse slopes are uniform, the contours will be equidistant at that station—contours below the datum plane, should any occur, are located of course in a similar manner.

In the location of a few miles of road on the steep slopes of a river valley, where the exact topography was indispensable, the writer was surprised at the rapidity with which the contours were delineated, as well as by the accuracy of the location as subsequently proved in the field. Although more especially adapted to the topography of a narrow strip, as in a railroad survey, its extension to ordinary mapping is obvious.

INVESTIGATION OF THE QUESTION OF THE THRUST OF EARTH BEHIND A RETAINING WALL.

By J. ROMILLY ALLEN, A.I.C.E.

Written for Van Nostrand's Magazine.

PROBLEM TO FIND THE HORIZONTAL EARTH THRUST AGAINST THE BACK OF A RETAINING WALL.

It is assumed that the earth, being confined by the wall, acts as one solid mass and that if the wall were removed the earth would break away along a plane surface called the "Plane of Rupture."

On this supposition, therefore, the thrust behind the wall is produced by a wedge of earth, included between the back of the wall and the Plane of Rupture.

It must be premised that the position of the Plane of Rupture (which depends on the angle of repose of the earth and the batter of the back of the wall) has yet to be determined.

The Plane of Rupture does *not* coincide with the natural slope of the earth, as might be supposed at first sight; for the prism of earth, included between the back of the wall and the natural slope, as long as it is kept together in one solid mass, is just supported by the friction along the plane of natural slope, and consequently in this case no thrust will be produced.

For a similar reason, it is evident that the Plane of Rupture does not lie between the natural slope and the horizontal, and therefore it must fall in some position intermediate between the natural slope and the back of the wall.

Now if all prisms of earth produced the same amount of thrust against the back of the wall, the position of the plane of rupture would be indeterminate, since the earth would be just as likely to break away in one direction as another.

There is, however, one prism which produces greater thrust than any other, and this is obviously the one which will break away first.

Consequently the plane along which this prism slides is the plane of rupture.

It is, therefore, necessary to find the "Prism of maximum Earth Thrust," which is done as follows:

Note.—Friction against the back of the wall is neglected.

The portion of wall and bank dealt with is supposed to be one foot in length, so that this factor is left out in the calculations.

Fig. 1 shows the cross section of wall and bank of earth supported.

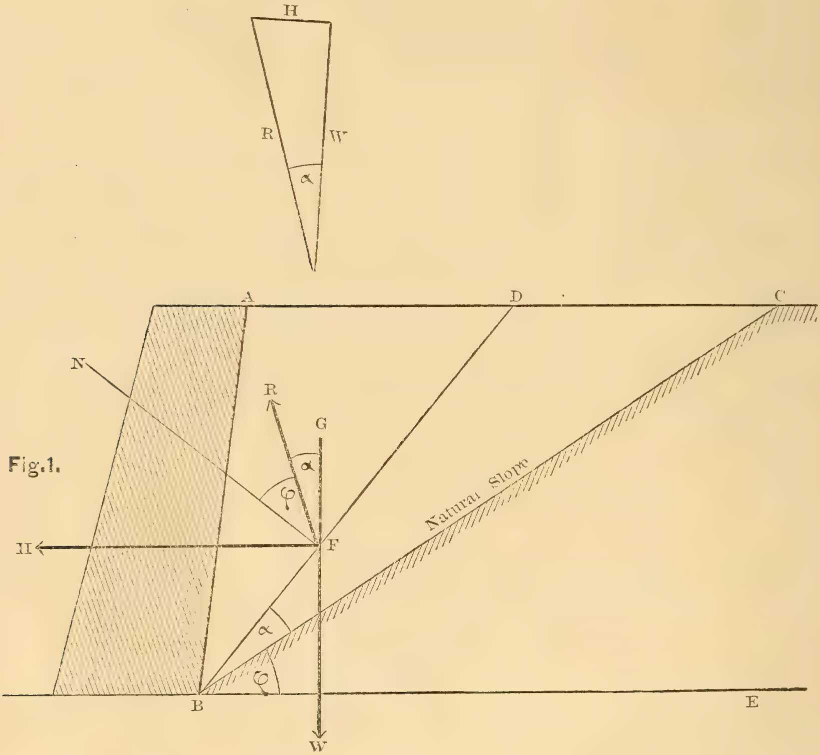


Fig.1.

AB is the back of the wall.

BC the natural slope of earth.

BE the horizontal.

Consider the condition of any prism of earth such as ABD, resting on the plane BD, which falls between the natural slope and the back of the wall.

The three forces which keep ABD in equilibrium are:

1. The weight W of ABD
2. The horizontal thrust H against AB
3. The reaction R of the plane BD

Let the angle of repose CBE, of earth $=\phi$ and angle DBC which DB makes with natural slope$=\alpha$.

Now the directions of the three forces W, H, and R, are as follows:

1. W acts vertically through G, the center of gravity of ABD.

2. H acts horizontally.

3. R acts at an angle$=\phi$ with FN the normal to BD.

Draw the Triangle of Forces, having its sides respectively parallel to these three directions, and make the side W of such a length as to represent the weight of prism ABD, to any fixed scale.

This construction gives the value of H to the same scale.

Now it may easily be shown, by producing the normal NF to cut BE, that angle RFG$=\alpha$, and, therefore, that the corresponding angle of the triangle of forces between R and W also$=\alpha$.

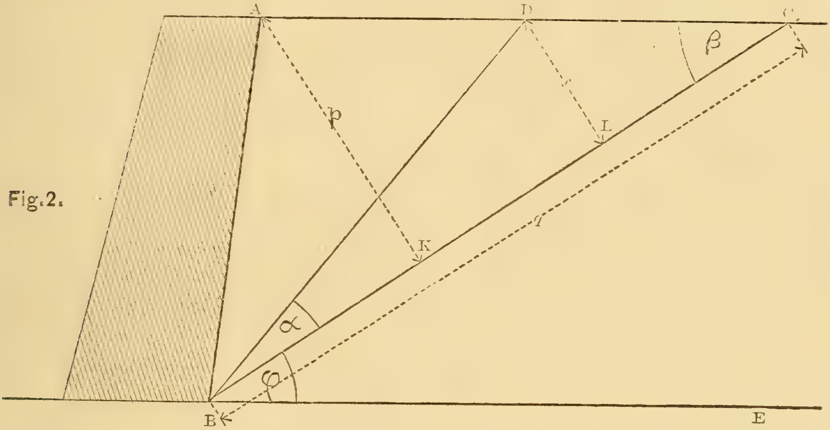
Therefore

$$H=W \tan.\ \alpha \quad . \quad . \quad . \quad . \quad (I)$$

But W, which represents the weight of a prism of earth ABD, one foot in length, is proportional to area ABD.

So that H attains its maximum value when area ABD$\times$tan. α is greatest.

Fig. 2.



In order to investigate this, in Fig. 2 let fall perpendiculars AK, DL, on natural slope BC.

Suppose Constant Quantities

p=AK, length of perpendicular from top of wall on natural slope.

q=BC, length of natural slope.

β=angle ACB, between surface of ground and natural slope.

Variable Quantities;

x=DL, length of perpendicular from D on natural slope.

α=angle DBC, which DB makes with natural slope.

Now

$$\text{Area ABD}\times\tan.\ \alpha=(\tfrac{1}{2}pq-\tfrac{1}{2}xq)\tan.\ \alpha$$

$$=\tfrac{1}{2}q\,(p-x)\left(\frac{x}{q-x\cot.\ \beta}\right)$$

$$=\tfrac{1}{2}q.\frac{px-x^2}{q-x\cot.\ \beta}$$

Differentiating the quantity $\frac{px-x^2}{q-x\cot.\ \beta}$ and putting the value thus obtained$=0$ for a maximum, there results the following equation

$$(q-x\cot.\ \beta)\ (p-2x)-(px-x^2)\ (-\cot.\ \beta)=0$$

or

$$x^2\cot.\ \beta-2qx=-pq \quad . \quad . \quad (II)$$

Putting this equation into another form

$$pq-qx=qx-x^2\cot.\ \beta$$

$$=x(q-x\cot.\ \beta)$$

$$=x\times BL$$

Area ABC$-$Area DBC$=$Area DBL

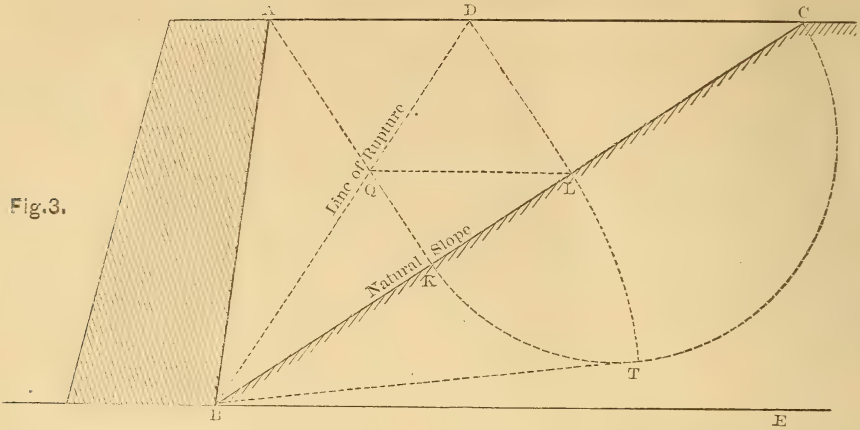
Area ABD$=$Area DBL . (III)

which equality expresses the only condition necessary, in order that ABD may be the "Prism of Maximum Earth Thrust" and BD the Plane of Rupture.

The following geometrical construction gives the position of BD, subject to the above condition.

Construction—From A (Fig. 3) let fall perpendicular AK on BC, and on KC describe a semicircle KTC.

Fig.3.



From B draw the tangent BT to semicircle.

In BC make BL=tangent BT.

Draw LD at right angles to BC, cutting AC in D.

Join BD, cutting AK in Q.

Now BD is the Line of Rupture: Proof—Since

$$BL^2=BT^2$$

$$=BC.BK$$

$$\frac{BL}{BC}=\frac{BK}{BL}$$

$$=\frac{BQ}{BD}$$

Join QL

$\therefore$ QL is parallel to DC

And since AK is parallel to DL

$$\therefore\ AQ=DL$$

$$\tfrac{1}{2}\,AQ.BL=\tfrac{1}{2}\,DL.BL$$

$$\therefore \text{Area } ABD=\text{Area } DBL$$

which was the condition insisted on.

The actual amount of maximum Earth Thrust is found thus:

From eq. (I)

$$H=(\text{weight of prism } ABD)\times \tan.\ \alpha$$

$$=(\text{weight of prism } DBL)\times \tan.\ \alpha$$

$$=w\times\tfrac{1}{2}\,DL.BL\times\frac{DL}{BL}$$

$$H=\tfrac{1}{2}wx^2\ .\ .\ .\ .\ .\ .\ .\ .\ .\ \text{(IV)}$$

where

H=maximum Horizontal Earth Thrust per foot forward

w=weight of 1 cubic foot of earth

x=DL

x may either be found by solving quadratic eq. (II) or may be thus expressed in terms of known quantities.

$$x=DL$$

$$=LC\ \tan.\ \beta$$

$$=(BC-BL)\ \tan.\ \beta$$

$$x=(BC-\sqrt{BC.BK})\ \tan.\ \beta\ .\ \text{(V)}$$

The above formulæ apply equally well whether the surface of the earth behind the wall be horizontal or not when the back of the wall is vertical of height=h and the surface of the bank horizontal

$$H=\tfrac{1}{2}\,wh^2\frac{1-\sin.\ \alpha}{1+\sin.\ \alpha}\quad .\quad \text{(VI).}$$

THE MECHANICAL THEORY OF HEAT.*

From "The Engineer."

The subject of this work is one which has become increasingly popular in proportion as it has been studied by the application of the sciences of mechanics and numbers. Like several other branches of science, it has become more interesting, and of wider importance, as the cobwebs of unsupported speculation have

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* "Treatise on the Mechanical Theory of Heat and its Applications to the Steam Engine, &c." By R. S. McCulloch, C.E., Professor of Mechanics in Washington and Lee University, Lexington, Va. New York: D. Van Nostrand; London: Trubner & Co. 8vo.

been removed under the light of experimentally ascertained facts. It is true that much of what we know of the nature of heat, as far as its analogy to light is concerned, was dimly foreseen by some ancient writers ; but, as the author of this work observes, imaginations are of no value, and of little merit, so long as they remain barren of positive results. The first chapter of this work is a brief historical sketch, which places before the reader the names and the nature of the work and claims of those to whom we are chiefly indebted for the development of our knowledge on this subject as we now understand it. Commencing with a reference to the celebrated "Traité de la Lumière," in which Huygens, in 1690, published his demonstrations of reflexion and refraction, regarded as phenomena of wave propagation, and commenting on the Newtonian hypothesis that light and heat are matter, one which obtructed the progress of knowledge for more than a century, the author passes on to the work of Young and Fresnel, laying much stress on the claims of the latter. The remainder of the chapter, and the largest part of it, is chiefly occupied by the result of the work of those who originated and developed the mechanical theory of heat—Rumford, Seguin, Mayer, Colding, and Joule, of course, occupying most attention. A spirit of fairness characterizes this chapter, in which, while giving full credit to Rumford, Mayer, and others, Joule is accredited with the merit of having caused the true theory of heat to meet with general reception. Perhaps a little too much is claimed for Huygens and Fresnel, for while admitting the splendid achievements of both, it must be remembered that until Rumford's experiments upon the mechanical equivalence of heat, his practical and indisputable proof by measurement, their work and discoveries remained so far in the condition of hypotheses, that they could be and were received by some and rejected by other equally eminent men. The proof of actual measurement by mechanical means, was necessary to remove the possibility of the existence of difference of opinion in equally competent minds, and for this proof we are most indebted to Rumford and Joule. The experiments of Hirn are not forgotten in this chapter, full reference being given to his determination of the quantity of work done by heat, the inverse of the problem solved by Joule, and which he determined by a series of experiments with 100-horse power engines. By these experiments it was proved that the efficiency of the Woolf engine was as much as from one-eighth to one-seventh, or about double that obtainable according to the calculations of Regnault, which led him to believe the steam engine a more wasteful machine than it really is. The apparent difference between Regnault's conclusions and actual measurement was removed by Rankine, who explained it in 1849 by showing that a certain quantity of work was done by the steam which had given up heat by condensation in the cylinder. For the purpose of demonstrating this Hirn had glass plates let into a pipe attached to engines working with a pressure of 75 lbs. per square inch, by which he was enabled to see the steam, which was transparent at 307 deg., become clouded with liquid vesicles during expansion.

It should be here said that Mr. McCulloch's book is an advanced scientific treatise designed principally for the use of students, and without any pretence to being a popular treatise. To be able to appreciate the mass of information, the unbroken chain in which the many different parts of the subject form appropriately arranged links, and the deduction from these, the reader must possess at least an elementary knowledge of the calculus and of analytical geometry. To enable the student to make intelligent progress in the study of the mechanical theory and equivalence of heat, it is, of course, necessary that he should well understand the fundamental laws of mechanics, and for this purpose the second and third chapters of the work before us deal with dynamics, in order that a proper conception may be formed of the ideas conveyed by the terms, "work—energy—potential and kinetic—force—conservation and dissipation of energy—transformation of energy, &c. &c., particularly in their thermal applications." Little need be said of these two chapters. As brief expositions of abstract dynamics they are lucid and careful, but in some instances they are too brief to be of any service to the student, and this is particularly noticeable with reference to the "work of heat" and "work of expansion," which are about sufficient explanation, and are dealt with in about one and a-half page. Chapter IV. is devoted to a discussion of the general laws of heat and thermodynamics, and Chapter V. deals with "airs and vapors," and presents able examinations of the experiments and laws of Gay Lussac, Marriotte, and Rankine, of the influence of temperature upon compressibility, expansion of vapors. In the three succeeding chapters, the most recent interpretations of the known phenomena of heat are examined and applied under the heads of "Internal Energy," "Air Engines," and "Thermal Laws" respectively. Part II. of the work deals with "applications," the first and second chapters of this part. Nos. IX. and X. form a carefully-considered digest of most of the laws relating to steam and vapors as bearing upon thermodynamic questions. "Steam engines, their defects and improvements" are the subjects of the XIth chapter, and Chapter XII. consists of miscellaneous applications. Want of space prevents our reference at any length to the former of these two chapters, or at all to the last. There are some expressions, however, which show that the author has not sufficiently considered his eleventh chapter to make it of service to either engineer or student. For instance, under the head of "Defective Expansion," the author says that "incomplete expansion is one of the principal defects of steam engines," and "the table of Clausius shows that between the temperatures of 150 deg. and 50 deg. C. the expansion of saturated steam is 25.7, or nearly 26 times its original volume. For such dilatation cylinders of enormous size would be requisite."

The latter sentence is practically the only one which suggests that there would be any difficulty in what the author terms complete expansion, and it is disappointing in such a work to find no mention of the principal difficulty to such ranges of expansion—namely, that very reduction of temperature which he has mentioned, or the refrigeration due to expansion, a question which could not be left out of sight with propriety, unless some perfectly non-conducting material had been found in which to conduct such expansion without positive loss. Again when considering the use of superheated steam, the author, in common with many other writers on the subject, observes, "the *chute de chaleur* or difference ($\tau \ \tau^\circ$) upon which the duty of an engine depends cannot, therefore, be much increased by making the boiler hotter," *i.e.*, because a pressure of ten atmospheres with a corresponding temperature of 180 deg. C. "cannot be much exceeded without danger of explosion." It would, no doubt, occur to most engineers that if the duty of an engine depended alone on the range of temperature in the cylinder, that the difficulty of making sufficiently strong boilers would soon be overcome. With a perfect ideal engine the *chute de chaleur* would undoubtedly be a measure of its efficiency, but in considering the applications of thermodynamics to practical engines, the reader might have expected that some inquiry would have been made into the economical limit to this range of temperature and the conditions which determine that limit. It may be said that this question is somewhat apart from the object of this work; but as practical applications are considered, it seems to us that this important practical question should have been discussed.

In this work Watt is of necessity very frequently referred to, and full justice is done to the magnificent results of his invention and work, but why the author should have designated him Sir James Watt is unexplained. This title was offered to Watt a few years before his death, but, although pleased with the kindness, he declined to accept it, a decision which if he had lived now, he would perhaps have arrived at without even consulting his sons on the matter, as he then did. It is also somewhat curious in a work of this kind, in which only ascertained fact is dealt with, to find the author, both in his preface and in his conclusion, writing about the docile acceptance of a faith, and commenting on "human perversity which seems to incline some to prefer the Arctic scepticism of negation to the genial warmth of Christian faith," in a manner which, in such a work, seems at least out of place.

AMERICAN WATCHES.

By Prof. JAMES C. WATSON.

Abstract of Report on Horology at the International Exhibition at Philadelphia.

In the examination of the quality of watches produced by the American Watch Company, it became necessary to consider first the mechanical contrivances by which the parts are executed, and then the manner in which these are brought together in the completed movement. While introducing so much that is novel in the way of machinery and processes of execution, there has been no attempt, in respect to the parts of the movements, at innovations which are not of recognized merit. There are certain well-established principles in reference to the proper construction of the train which have resulted from the application of the laws of mechanics, such as relate to the form of the teeth of the wheels, the leaves of the pinions, and proper numbers for each. It is well established also, that for the purposes of a watch to be carried in the pocket, the lever escapement is to be preferred. The results of careful experiments show that when the lever escapement, the duplex escapement, and what is known as the chronometer escapement, are equally well constructed, and placed in movements with equally good trains and equally well sprung, the performance is substantially the same. There being, therefore, no objection to the lever escapement, as regards the time keeping properties, and there being good reasons for its adoption in preference to the two others named, when the manner in which the watch is to be carried is considered, the propriety of this form of construction, even for the finest watches, is put beyond question. There is one innovation upon what was in this country and in England, until recently, regarded as an essential principle of watch construction, which the company ventured upon at the outset. This consisted in dispensing with the fusee and chain, and using in preference the going-barrel, thus reducing very much the size and the complexity of the watch. The Swiss manufacturers long ago ventured, so to speak, to adopt to a considerable extent this form of construction, but the poor quality of the watches which the careless greed of manufacturers and importers flooded upon the markets of the world, did much to strengthen the idea, among the dealers as well as among the purchasers, that a good watch must be provided with a fusee and chain, so that the ultimate force with which the power of the main spring acted upon the escapement should be as nearly constant as possible. Hence the popularity of the English lever watches in this country, and hence the tenacity with which in England they still adhere to the notion that no reliance can be placed upon any other form of construction.

But, wisely, the pioneers in the introduction of this industry into our country, decided to adopt the going-barrel, so as not only to reduce the size and expense of the watch, but also to obviate the great expenses for repairs made necessary by the frequent breaking of the chain. And besides, in the case of the breaking of the mainspring, the recoiling of the barrel, from the sudden removal of the pressure, creates the risk of injury to the teeth of the great wheel and to other important parts of the movement. Yet, in spite of these discouragements, the necessity of a fusee and chain seemed so apparent to those not versed in the knowledge of the action of the balance spring as regulating the movement of the watch, that this notion, so well fortified, did much to oppose the introduction of the watches in which these parts were wanting. Certain it is that many scientific sceptics upon this point had to be convinced by actual trials before yielding their assent to the possibility of the construction of an accurate timekeeper upon the simpler system. It is indeed true that the force of the mainspring is not constant, but if the difference be not too great the isochronal property of the hair-spring, in proper or even approximate adjustment, is quite sufficient to regulate the movement of the watch within the limits required. If the mainspring be of considerable length, and the number of turns when wound such that only comparatively a few turns are unwound during twenty-four hours, there will be very little variation of the force actually transmitted to the train, so that if the watch is wound regularly, the small differences which would result are counteracted so completely by even an approximate isochronal adjustment of the balance spring as to become practically insensible.

The safety of these simpler movements, from any injury which might result from the breaking of the mainspring, is most effectually provided for in all the watches made by this Company, by a device known as "Fogg's Patent Safety Pinion," invented in 1865. The arbor is tapped with a triple left-hand thread upon which the pinion screws. This pinion is toothed into the great wheel upon the barrel, and the action of the wheel upon it is in the direction of the tightening of the screw. Whenever, therefore, the mainspring breaks, the recoil of the great wheel unscrews the safety pinion from the arbor, and thus instantaneously releases the whole movement from the effect of the reaction.

It is well known that the teeth of the wheels should be epicycloidal in form, and the attempt is always made, in a properly constructed movement, to secure this form in the final process of finishing. Often, however, in the manufacture under the old system, partly on account of the difficulty of securing the proper form, and partly on account of the peculiar æsthetic notions of the workmen who conceive a dislike to epicycloidal teeth because they had in mind the resemblance of their form to that of the bishops' mitres, this important principle was disregarded. Another obstacle arises from the difficulty of producing in miniature with precision what it is possible to draw only upon a magnified scale. But by the application of the machines constructed for this purpose, not only in this instance, but in many other similar cases in the production of the various parts, is the difficulty obviated, and the requirements of theory rendered possible in practice. The forms of the cutters and polishers of the machines which cut the gearing are kept true by means of a machine which gives the true epicycloidal form, so that by proper attention to the depthing when the train of wheelwork is put together in the watch movement, the perfection of smooth and continuous action is secured. It is right here that the superiority of the American system comes into important operation. While it is possible by the ordinary methods, by patient work, to produce the wheels and pinions with the requisite degree of perfection, yet, manifestly, on account of the necessary expense, this cannot be done in the case of the common grades. But by the method under consideration, the action of the machines is precisely the same for all grades. There may be degrees of finish, but the form is unchanged, and the cheapest movements have this excellence in common with the best. The difference of the cost of watches not specially adjusted, will therefore depend upon differences in the number of jewels, the finish of particular parts, and the character of the balance.

The excellence of the train being beyond question, the next and the important consideration is, the character of the work in the construction of the parts connected with the escapement. These are all constructed by processes quite similar to those already mentioned, and hence attention may be directed particularly to the hair-spring and the balance. It is well known that it is upon the proper performance of these parts that the accurate time-keeping property of the movement depends. Any imperfection here will make manifest in a striking degree any imperfections in the train, and even when everything else is perfect, a failure here renders the movement practically worthless for the purpose for which it was intended. The balance spring must be of the very best material, must be evenly coiled, and must be so tempered as to secure a maximum degree of elasticity, and a continuance of this quality undiminished. There will then be a relation between its elastic force, the length to be brought into action, and the weight of the balance which it is to control, to be determined so as to make the vibrations of this balance isochronous whether they be long or short. An examination of the process employed in springing the movements showed that this is accomplished by means of instrumental appliances with all the precision necessary, even in the case of the commonest grades of movements. For higher grades, not including those specially adjusted, to be considered hereafter, the process gives results of wonderful accuracy. The symmetry of the action of the spring, as its coils are caused successively to contract or expand, is carefully tested by an indicator constructed for that purpose, and the condition that the center of torsion shall be coincident with the axis of the balance staff, is thus secured in advance. The elastic force of the spring for different degrees of winding is next determined, and thereupon it is possible to select a balance for an assortment of compensated balances prepared so as to fulfill the requirements for poise and compensation by identical processes, such that the conditions for isochronism are very approximately realized without any special adjustments after actual trials of the running movement. In this way, movements which are of low price, and adapted to the general market, are produced which perform often equal to the most carefully adjusted. An actual trial, by timing tests, of movements of this grade has shown generally only small errors in the several adjustments for temperature, isochronism and position, and in some instances they have been within the limits attainable by special attention to these adjustments in the usual way. Thus often for a very moderate price, indeed, may be obtained a movement of a very high order of excellence, such that if it had been adjusted by the usual method, on account of the attention required from the adjuster, its cost would have been necessarily increased ten fold.

The results of the examination of all the grades so far considered were, as stated in the report for an award, that for watches not specially adjusted, and including all the ordinary and medium grades as to price, this Company is enabled to produce, in the case of such movements, better watches than are produced by the other methods in use, and that these products are worthy of special commendation.

This Company also exhibited watches, the parts of which were also made by machinery, and highly finished, and which had been specially adjusted for isochronism, for position of the balance, and for temperature. One of these, selected at random, was taken down and all its parts minutely examined. The workmanship throughout was found to be excellent, and the materials the very best. The plates were of nickel, the wheels of gold. The holes were jeweled and capped, and the springs of excellent quality, and the balance spring of the form known as the Breguet spring. The workmanship being of a quality to warrant a movement of the highest order, it remained to determine by actual trial the accuracy of the adjustments, and the regularity of the motions from day to day. The limited time available, in the course of the examinations, for a work of this kind, made it difficult to give to these trials proper attention; but the requisite astronomical instruments being in position and available near the United States Government building, (being there as a part of the National exhibit) ten of these movements were taken from among those on exhibition, and subjected to trials. The first trials for errors of position were made for seven of these movements by Mr. Theodore Gribi, a member of the Swiss Commission, whose services as mechanical expert were of great value to the writer of this report in the course of the examinations in the department of horology. The trials made by Mr. Gribi were during days when the ranges of temperature were considerable, the average temperature in the building during the middle of the day often exceeding 100° Fahrenheit. The results of Mr. Gribi's comparisons during a period of ten days, as computed by him, are as shown by the following table : (*See Table on following page.*)

The numbers given show the difference of rate corresponding to a period of 24 hours.

The movement numbered 670052, which was also compared by Mr. Gribi, is, for reasons which will subsequently appear, omitted from this table.

Subsequently, during the trial of the Marine Chronometers, these movements were subjected to further trials, to determine their errors, and especially the errors of compensating for temperature. The results derived by the reporter, from his observations in connection also with the comparisons made by Mr. Gribi, were communicated to the Centennial

No. of Watch.	Dial Up and Dial Down.	Variation Between Pendant Up and Pendant Down.	Variation Between Hanging and Lying.	Variation Between Pendant Up and Pendant to Right	Sum of the Four Variations.
	s	s	s	s	s
670087	2.36	8.35	0.11	3.77	14.59
670081	1.40	1.50	2.65	3.23	8.78
670082	0.73	0.98	0.30	0.01	2.02
670083	1.76	0.44	7.01	5.01	14.22
670092	0.88	7.31	3.18	5.19	16.56
570044	3.74	1.45	0.28	0.46	5.93
Mean.....	1.81	3.34	2.25	2.94	10.34

Commission, in explanation of the award reccommended by the Judges for the production by this company of first-class pocket chronometers. These results are as shown by the following table:

No. of Watch.	Variation for ±1° of Temperature.	Difference between Long & Short Arcs of Vibration.	Maximum Error of Position.
	s		
670075	∓0 034		
670099	∓0.188		
670095	±0.024	s	s
670061	∓0.014	2.5	1.4
670087	∓0 038	0.4	8.3
670083	±0.012	2.2	0.5
670052	±0.064	1.3	6.7
670044	∓0.052	0.6	1.4
670082	∓0.064	0.1	0.8
670092	±0.240	0.4	7.0
The Mean of all gives..	±0.07	1.07	3.7

These numbers refer to the variations of the rate corresponding to a period of 24 hours.

In the reduction of the observations for the movement numbered 670052, included in the above table, it was assumed that an abnormal error, shown by Mr. Gribi's comparisons, was due to an error of observation, but subsequently it was found that the movement was not in proper running order, on account of clogging pieces of lint adhering to the escapement, and that it needed overhauling; and hence in the subsequent trials another movement was taken.

The results thus obtained were sufficient to show that the claim of the production of first-class pocket chronometers was well founded, and they served as the basis for the award to the company for that class of movements. Although sufficient to warrant the award made, it is true they do not depend upon a series of observations, sufficiently extended to be compared fully with the results for the trial of first-class pocket chronometers at the Swiss observatories where such trials are regularly made. And besides, in the temperature trials there was some uncertainty as to the position in which the watches had been placed in the refrigerator during a portion of the 48 hours that they were kept in the cold. In order then, to put the movements to a severer test, they were taken to the Observatory at Ann Arbor, and there subjected by the reporter to a rigorous trial. The whole period of this final trial was eleven weeks, which is five weeks longer than the first-class pocket chronometers are tested at the Swiss observatories. The trial began on September 9th, and ended on November 26th, 1876, and the results of the successive weeks are shown by the following tables (*next page*):

In these tables (p. 165) the watches are arranged in the order of their merit, as indicated by the range and fluctuations of the rate in all the changes of temperature from the beginning to the end of the trial—not taking into the account the errors arising from the change from the horizontal to the vertical position.

During the seventh week the watches were placed in different positions, with the dial up, in respect to the magnetic meridian, but no sensible errors, which might be attributed to the action of magnetic forces, were indicated.

In the ninth week of the trials the watches were kept during two days at a mean temperature of 36°.7, and then transferred to a warm room in which they were kept for two days at a mean temperature of 95°.1.

During the tenth week, the watches

TABLE SHOWING THE PERFORMANCE OF THE WATCHES DURING A TRIAL OF ELEVEN WEEKS.

Week of Trial.	*First.*	*Second.*	*Third.*	*Fourth.*	*Fifth.*	*Sixth.*	*Seventh.*	*Eighth.*	*Ninth*	*Tenth.*	*Eleventh.*
Range of temperature	57° to 69°	60° to 72°	47° to 69°	43° to 53°	35° to 50°	39° to 66°	40° to 61°	49° to 65°	31° to 99°	40° to 77°	55° to 69°
Mean temperature ...	62°.1	60°.5	53°.1	48°.7	43°.4	54°.8	48°.8	56°.3	61°.8	52°.7	61°.4
Position............	Horizontal Dial Up.	Horizontal Dial Up.	Vertical Pend. Up.	Vertical. Pend. D'n.	Vertical. Pend. Rig't	Vertical. Pend. Left	Horizontal Dial Up.	Horizontal Dial Up.	Horizontal. Dial Up.	Horizontal. Dial up & d'n.	Horizontal. Dial Up.
No. of Chronometer.	WEEKLY SUMS OF RATES.										
	s	s	s	s	s	s	s	s	s	s	s
670044	— 5.05	— 4.80	+27.55	+52.10	+ 42.05	+18.20	+ 2.05	— 3.50	+ 1.40	+11.35	— 5.55
670089	+ 3.65	+ 3.35	+47.40	+70.60	+114.85	+66.85	+11.90	+13.50	+12.65	+ 6.40	+10.05
670082	—13.65	— 7.60	+ 9.10	+15.85	+ 9.65	—10.25	— 7.90	—11.30	—11.30	—16.75	—21.95
670095	—20.85	—16.95	—18.15	+ 7.85	+ 1.40	+25.75	—20.90	—16.50	— 8.55	—18.90	—20.00
670087	—14.15	—17.20	+ 6.35	+79.20	+ 67.40	— 1.95	— 8.45	—14.35	— 3.55	— 9.50	—18.85
670083	+ 1.70	+ 7.80	+ 5.15	+57.60	+ 71.95	+42.45	+19.05	+27.95	+23.40	+10.20	+18.20
670090	+ 7.45	+ 6.45	+36.55	+27.30	+ 42.50	— 1.75	+24.90	+17.80	+29.45	+34.00	+29.80
670061	—27.70	—39.50	+ 1.25	—20.95	— 45.80	—32.50	—47.60	+49.40	—35.70	—36.05	—55.45
670099	—20.25	—26.15	+20.20	+66.15	+ 69.85	+53.25	— 6.25	—18.05	—10.50	— 0.35	—22.65
670092	—15.00	—11.35	—59.43	—31.62	— 63.05	—62.15	—32.45	—31.55	— 9.30	—22.35	— 7.20

These watches are all stem-winders. The duration of trial in each position indicated unmistakably the total effect of irregularities of motion, due to changes of temperature.

were kept three days with the dials down, in order to find the difference of rate between the dial up and the dial down.

By a comparison of the rates from day to day during the periods in which the position remained unchanged, the mean daily variation of the rate of each chronometer was obtained, and the variation of the rate, assumed to be uniform through a considerable range of temperature, corresponding to a variation of one degree of the thermometer, was derived by a comparison of the rates at the extremes of temperature to which they were subjected. In order to exhibit more fully the character of these movements the following tables are subjoined, the numbers in which have been derived from the data obtained in the course of the final trials now under consideration:

(*See Tables on following page.*)

Those who are familiar with investigations of this character will understand, without further explanation, what the numbers in these tables indicate; but for the purpose of making the results clear to the general reader, it is necessary to consider them more fully, commencing with the first of those which immediately precede.

The mean daily rate, as shown in the second column, is the average rate by which each watch gained or lost on mean solar time during the whole period of the trials, and the next column shows the average temperature during the same period. The fourth column exhibits the mean variation of the rate from day to day, excluding the differences which resulted from the changes of position. The amount of this mean daily variation for a particular chronometer is a fair index of the perfection of its manufacture, except so far as relates to the special adjustments next to be considered.

In reference to the numbers given in the fifth column for the variation of the daily rate corresponding to a variation of one degree in the mean temperature, it should be stated that they represent this coefficient as derived from the differences corresponding to a range of temperature from 36°.7 to 95°.1 Fahrenheit, disregarding the effect due to the irrationality of the compensation, for which, in marine chronometers, a secondary or auxiliary compensation is fre-

TABLE SHOWING THE MEAN DAILY RATE IN DIFFERENT WEEKS, AND THE VARIATIONS OF RATE FOR CHANGES OF POSITION.

Number of the Chronometer.	Difference Pendant Up and Pendant Down.	Difference Pendant Right and Left.	Difference of rate between First & Eleventh Weeks.	Difference before and after Vertical Positions.	Mean daily rate for Weeks in which position was horizontal. For temperature 60° Fahr.						
					Before exposure to Heat.				After exp'ure to Heat		
					First.	Second	Sev'nth	Eighth.	Ninth.	Tenth	11th.
	s	s	s	s	s	s	s	s	s	s	s
670090	—1.76	—5.18	+3.19	+0.87	+1.26	+1.56	+2.44	+2.17	+4.39	+4.68	+4.44
670089	+3.14	—6.40	+1.00	+0.51	+0.50	+0.74	+1.25	+1.78	+1.74	+1.38	+1.49
670082	+0.79	—2.38	—1.19	—0.74	—1.87	—0.83	—1.58	—1.77	—1.54	—2.02	—3.07
670095	+3.65	+3.64	+0.12	—0.81	—2.95	—2.33	—3.13	—2.31	—1.20	—2.85	—2.84
670083	+7.09	—3.19	+2.36	+0.01	+0.44	+1.69	+1.71	+3.67	+3.50	+2.35	+2.72
670044	+3.06	—2.27	+0.01	—0.61	—0.63	—0.12	—0.72	—0.83	+0.37	—0.71	—0.66
670082	+4.94	—2.30	+1.24	+0.53	—2.73	—3.04	—2.48	—3.69	—1.74	—1.23	—1.33
670087	+9.94	—9.33	—0.67	—0.18	—1.85	—1.92	—2.10	—2.35	—0.37	—1.93	—2.58
670099	+5.81	—0.43	—0.34	—0.06	—2.54	—2.67	—2.74	—3.21	—1.22	—1.45	—2.99
670061	—3.23	+2.09	—3.96	—1.40	—3.93	—5.55	—6.96	—7.11	—5.08	—5.45	—7.90
Mean, all	±4.34	±3.72	±1.41	±0.57	±1.87	±2.04	±2.51	±2.89	±2.12	±2.40	±3.00

In this table the watches are arranged in the order of their mean daily variations of rate.

The means given at the bottom, show the average accuracy of the performance of the whole group.

TABLE SHOWING THE MEAN DAILY RATE, AND THE MEAN DAILY VARIATION OF THE RATE, CORRESPONDING TO A PERIOD OF TWENTY-FOUR HOURS, IN DIFFERENT POSITIONS, ETC.

Number of the Chronometer.	Mean Daily Rate.	Mean Temperature.	Mean Daily Variation.	Mean Variation for ±1°.	Before and after Oven.	Difference between Hanging and Lying.	Difference Hanging and Pendant Left.	Difference Hanging and Pendant Right.	Difference Dial Up and Dial Down.	Sum of these Four Variations.
	s	°	s	s	s	s	s	s	s	s
670090	+2.76	59.5	±0.12	∓0.098	0.0	—2.98	—5.30	—0.11	—1.30	9.69
670089	+1.15	“	0.14	∓0.038	0.5	—5.75	+2.71	+9.25	—1.77	19.48
670082	—1.75	“	0.20	∓0.041	0.0	—1.83	—2.69	—0.31	—1.55	6.38
670095	—2.47	“	0.21	∓0.014	0.7	+0.26	+6.23	+2.53	+0.10	9.12
670093	+2.34	“	0.26	∓0.089	0.5	+1.27	+5.48	+8.67	—3.25	18.67
670044	—0.38	“	±0.27	∓0.094	0.2	—3.28	—1.16	+1.10	+3.85	9.39
670092	—2.57	“	0.30	∓0.214	0.7	+3.91	—0.74	+1.52	—1.25	7.42
670087	—1.82	“	0.31	∓0.005	0.1	—2.69	—1.13	+8.23	—0.20	12.25
670099	—2.47	“	0.39	∓0.164	0.3	—4.34	+5.01	+5.44	—0.37	15.16
670061	—6.08	“	0.54	∓0.014	0.4	—4.63	—4.75	—6.87	+0.45	16.70
Mean, all	±2.38	59.5	±0.27	±0.077	0.3	±3.09	±3 53	±4.40	±1.41	12.42

In this table the watches are arranged in the order of their mean daily variations of rate.

In trials of this character one of the conditions of competition, often imposed in advance, is that the difference between hanging and lying shall not exceed a certain limit, and the result has been an attempt to accomplish this at the sacrifice of the adjustments in other positions. The movements under consideration were not adjusted specially in reference to any such condition.

quently applied to the balance. In reducing the rates to a mean temperature, whenever such reductions were required, a small correction was introduced on this account, whenever its amount was clearly indicated by the observations.

The numbers given in the column headed "before and after the oven," exhibit the change of the daily rate found by comparing the rates on the days preceding and following the placing of the watches in a heated room, and show how far they are affected by sudden and violent changes of temperature.

In order to understand the significance of the numbers in the succeeding columns which relate to the differences due to changes of position, it is necessary to call attention again to the character of the final adjustments of a first-class pocket chronometer. The general nature of these adjustments and the methods by which they are approximated to by this company, in the case of even the cheaper grades of movements, have already been alluded to, but for our present purpose further consideration of this subject will not be out of place.

The parts of the escapement being supposed to be of proper construction and in proper adjustment in the movement, the attention will finally be directed to the balance and to its controlling spring. In reference to the balance it is often erroneously supposed that the office of the compensation by means of a bi-metallic segmental rim of brass and steel is to counteract simply the effect of the expansion of the balance itself by an increase of temperature, whereas in fact the change of rate arising from the loss of the elastic force of the balance spring, by an increase of temperature, is five times greater than that resulting from the expansion of the balance. And still further there is a change of rate due to the elongation of the spring from the same cause. The compensation of the balance must provide for all these changes, and they amount to a change of the rate to the extent of more than one minute in twenty-four hours for a change of only ten degrees in the temperature. This, however, is now so well understood, that within limits which are always possible to good workmanship, the compensation can be readily effected, and the poise of the balance with reference to the axis of its staff successfully arranged. But when the attention is finally directed to the adjustment of the balance spring, the difficulties to be encountered require the highest knowledge and the most skillful manipulation on the part of the adjuster. On account of the change in the amount of the friction in different positions, and on account also of different degrees of viscosity of the oil at different times, and on account further of inequalities of motion communicated by the train, the arcs through which the balance will vibrate, will be subject to considerable fluctuations even under the most favorable circumstances. When we add to these the interferences which are constantly operating as the watch is carried about in the pocket of the wearer, it becomes of the highest importance that the isochronism of the spring shall be as perfect as possible. The property of an isochronal adjustment, it is well known, is that the vibrations of the balance shall be performed in the same time whether they be long or short. This will require, in general, for a balance of a given weight and diameter, a certain determinate length of the spring in action, and a form such that this action shall be symmetrical in reference to the motion of the balance each way from the point of quiescence. When this adjustment is once secured, it is evident that any change of the acting length of the spring will destroy this important provision; and here comes in one of the difficulties in the adjustment of a pocket chronometer which is to be carried in different positions. In the case of a marine chronometer there is provision made that the instrument shall perform its functions always in the same position, and hence, when the balance is once poised, regulated to time, and the isochronal adjustment of the balance spring completed by comparing the times of vibration, or the daily rate, in long and short arcs, obtained by varying the acting power of the mainspring, the desired result is accomplished. But in the case of a watch, even if the poise be perfect, the isochronal adjustment made perfect for a horizontal position, will be found to be in error in other positions because of the modification of the action of the spring as its position is changed. It becomes a very difficult matter, therefore, to secure the perfection of the adjustments for a variety of positions when an extreme limit of precision is sought. It has already been mentioned how these adjustments are by instrumental appliances effected in all the watches made by this company, when once the required relation of the parts has been established; but the production of movements of the highest degree of excellence requires that further and special adjustments be made.

The elastic force of the spring, as its coils are contracted or expanded from the state of quiescence, must change its value in a ratio depending upon the weight and diameter of the balance, for the case of isochronal action, and this relation is found by careful trials. The weight of the balance is not affected by the temperature, but its diameter is thus changed, and here again a source of error creeps in. It is the accumulation of all these errors, infinitesimal almost in each vibration, which, in the course of the twenty-four hours, gives a finite error in excess or defect, and thus alters the rate of the watch. If the acting part of the spring be too short the tension as it is wound will be too great, and in the course of long arcs of vibration the rate will gain, while in short arcs it will lose. But if the acting part be too long the tension due to the elastic force will be too small, and the watch will lose during the long arcs of vibration and gain in the case of the short arcs. Any bend in the spring changes the effective length in action, and hence the final isochronal adjustments are effected by modifying the terminal curves of the spring without unpinning it from the collet or the stud.

It is evident further that when once these adjustments are effected, it is of the greatest consequence that the conditions remain unchanged. Hence the importance of a spring properly and permanently tempered. It has been established by experiment that the effect of the process of tempering the spring is to leave it, before it has been put into use, in a condition such that the relations of its molecules do not become permanent until it has been for some time in continuous vibratory motion, and further that the effect of such motion is to produce in the outset an increase of its strength and elasticity, and hence an acceleration of the rate of the movement which it controls.

The use of the flat spring in these chronometers requires that, for the best and most permanent adjustment, a terminal curve for the spring be brought out of and over the plane of its coil. This is not essential for a spring whose action shall be always in the same position, but when the positions are to be different the form of this terminal curve performs an important office in the adjustment. Sometimes both ends of the spring are turned into such terminal curves, and theoretically there would seem to be good reason for this arrangement; but the difficulties of finding, in practice, the proper forms for two such terminal curves, is a sufficient warrant for confining the attention to a single curve, as in the Breguet form of the spring adopted in the construction of the watches under consideration.

The accuracy of the adjustments depending upon the positions in which the watches are placed, are sufficiently indicated in the several columns of the tables. A reference to the table showing the weekly sums of the rates, will indicate the time allotted in the trials to each position in which the movements were tried. It will be observed that the periods were such as to make prominent the irregularities depending upon these changes of position, and further that the trials extended to six positions, besides tests for the influence of possible magnetic action. It is hardly necessary to add that the tables establish conclusively the excellence of all the adjustments. And it should be borne in mind, in a just determination of the merits of these movements, that they were subjected to great vicissitudes before they received the long trial of eleven weeks. They had been taken from the factory to the Exhibition before its commencement. They were not regularly wound and kept running, but only so kept during the progress of three distinct trials separated by considerable intervals, and they were exposed to great changes of temperature. After the partial trials at Philadelphia they were allowed to remain unwound and at rest, then they were again wound and started, carried to Ann Arbor, and after an allowance of only three days to assume their regular rate, the final trials began. These extended over the long period of eleven weeks, and through considerable ranges of climatic temperature, circumstances such as to test severely their running qualities. In order to do full justice to the exhibit, it would have been better to have extended the trials to a much larger number of movements, because in the case of so small a number involved in the means taken, a single watch going badly, or not very closely adjusted for errors of temperature and position, affects the mean results much more than if the number were greater. The limitation of the number was not the fault of the exhibitors, but was determined by the convenience of the reporter, who could not undertake, in connection with the other duties devolving upon him, the trial of a greater number.

It would be of benefit to the company if their finest productions could be subjected to trial every year in some astronomical observatory. Nothing has done more to stimulate the Swiss manufacture toward excellent workmanship and the most careful adjustments possible, than the competitive trials which have been made for a series of years at the Observatories at Neuchatel and Geneva. In this way, by comparison of results, can some idea be formed of the progress made and the degree of perfection attained. The trials of the first-class pocket chronometers at the Swiss observatories extend over a period of six weeks. They are tried in five positions and in the oven. The second grade of pocket chronometers are subjected to trial for four weeks, in two positions, hanging and lying, in the oven. It will be seen by reference to the parts of this report which refer to the English, Swiss and German exhibits, that the labors of the Judges were very much facilitated by the certificates issued from the observatories, showing the results of the actual trials of the adjusted movements which were on exhibition. And in a determination of the progress which has been made in America, there was thus provided a standard of acknowledged excellence. It is not the purpose of this report to make direct comparisons of the productions of different maufacturers. The reader who is curious in such matters will find, in most cases, the requisite data under the heads of the report relating to the exhibits in question. But in order to convey to the general reader an idea of what the system of manufacture under consideration has made possible in a newly developed industry in this country, it is proper to state here that the reporter has compared the results of the trials as heretofore given with those furnished for all first-class fully adjusted watches in the International Exhibition, and that it is clear beyond question that the chronometers numbered 670044, 670082 and 670095 are altogether superior to any others exhibited. The tables already given show clearly the excellence of adjustments, and the smallness of the mean daily variation of their rates. The best of these, taking everything into account, is No. 670044, for which the mean daily rate was only thirty-eight one-hundredths of a second, and for which the difference of mean daily rate between the first and eleventh weeks of the trial was only one hundredth of a second. The steadiness of the rates of these watches is best shown by placing in juxtaposition the average daily rate during several weeks separated by considerable intervals, and during which the determining conditions were very different, as already stated. The following are the results:

Week ending.	Mean daily rate of		
	No. 670044	No. 670095	670082.
	s	s	s
1876. July 20.	—0.50	—3.60	—2.80
Sept. 18.	—0.63	—2.95	—1.87
Oct. 30.	—0.72	—3.13	—1.77
Nov. 26.	—0.66	—2.84	—3.07

These numbers, for the sake of comparison, are reduced as nearly as possible to the temperature 60° Fahrenheit.

The performance of No. 670044 is extraordinary, considering the vicissitudes to which it was subjected and the long period of nineteen weeks during which its rate is considered. The mean daily variation of the rate is greater than that of each of the other two, but this may be due to the imperfect elimination of the error of the eccentricity of the second's dial, in the daily comparisons. Its temperature correction is also apparently somewhat larger, but that all the essentials for good performance were present, is best attested by the steadiness of the rate from week to week as it was under trial.

No. 670089 is hardly of inferior rank to the three here specially mentioned, although the sum of its position errors is greater than the general average.

The results thus developed show clearly enough that there is no lack, in this country, of mechanics skilled in the most abstruse principles of horology, and able to execute their work in accordance therewith. But it is a lamentable fact that the great majority of those who are to care for these productions after they are put into use, are in total ignorance of the fundamental principles upon which the performance of a watch depends. It would be of inestimable benefit to the public, from an economic point of view, if there could be established in this country schools of horology where young men desiring to become experts in the repairing even of watches, could be enabled to acquire that knowledge and experience which is essential to the proper performance of this kind of work. In every industrial or intellectual pursuit in life, it is desirable that men should be thoroughly educated in the principles underlying their profession, and in none more so than in that which we are considering. It is true that we find occasionally men in this profession, also, imbued with the desire to know the why and the wherefore, who have sought to acquire, and often have acquired such education; but too often indeed do we find men professing to be accomplished watch repairers who are in profound ignorance of the principles upon which the peculiar functions of these instruments are correctly performed, and who are liable to undo in a moment what has been achieved by the patience and skill of an accomplished manufacturer. The reader will already have in mind, from what has been under special consideration, that the greatest care is necessary in respect to fully adjusted watches. For needed repairs they should be entrusted to men who fully understand that the relations of the balance and the hair-spring should never be disturbed, even to the minutest bending or lengthening of the spring, or disturbance of the weights of the balance.

It is apparent, therefore, how important it is to the possessor of a good watch that it shall not be liable to be ruined by the ignorant manipulation of some professed watch repairer who undertakes to clean it. The desire to regulate such a watch to run in conformity with a worthless clock, with pretended compensation of the pendulum, has often been the source of mischief to the machine which should regulate instead of being regulated. And further, it should be borne in mind, that the use of watches has now become so general that the expenses of cleaning and repairs must amount to millions of dollars annually. These expenditures result partly from necessity, partly from the worthlessness of movements palmed off by irresponsible dealers upon a confiding public, and partly from the ignorance of those who undertake the repairs. It is one of the merits of the system of watch manufacture under consideration, that it reduces to a minimum the expenses to be incurred and the dangers to be encountered.

The general reader unacquainted with the results which are obtained from clocks and chronometers of the best construction, by those who are concerned with the accurate determination of time, may perhaps expect closer results than those indicated in the foregoing tables, especially when recalling to mind some of the extravagant statements often made as to the running qualities of watches in the hands of wearers whose comparisons have been made at long intervals, and often with uncertain standards, thus obtaining accidental coincidences. The trial of the movement from day to day, with exact standards of comparison, will necessarily reveal those errors which are compensated or concealed when the intervals of comparison are very long.

The fullness of this report on this particular exhibit has been made necessary, on account of the disposition manifested by certain foreign manufacturers to arrogate to themselves and their country the sole possession of that knowledge and skill which can render possible the higher achievements of horological art. While conceding, as they have been compelled to do, by the inexorable logic of the facts before them and the world, the superiority of the American system of manufacture in the case of all but the specially adjusted watches, they have been disposed to cling to the latter as of their exclusive control, and to look with suspicion upon any results which went to show the possibility of excellent production in these higher grades also. The final adjustment of a watch for position, for isochronism, and for temperature, requires only that the maker shall be able to produce the best quality of springs, of the desired form, and to make a balance of proper proportions, it being supposed that the escapement and the train have been properly constructed. The adjuster must understand the methods of springing, must know how to secure the poise of the balance, and how to modify the acting part of the spring so as to secure isochronal action in different positions and at different temperatures. It cannot be supposed that the science and skill which have achieved wonders in all of what might be called the inferior operations, should find an insurmountable barrier here. The results of the trials of the very few watches selected for that purpose, from among those of the exhibit under consideration, show that such is not the case. And the manufacturers may claim superiority for their finest productions, for the very simple reason that the machine to be adjusted, finally, for all the errors which interfere with accurate time keeping, is a better machine when constructed upon their system than when constructed upon the old system, and the better machine being equally well adjusted with the inferior one, in respect to these isochronal functions, must, in the nature of things, be of a perfection more enduring, and must satisfy better all the conditions for the highest productions of horological art.

CABLE-MAKING FOR SUSPENSION BRIDGES AS EXEMPLIFIED IN THE EAST RIVER BRIDGE.

By WILHELM HILDENBRAND, C. E.

Written for Van Nostrand's Magazine.

I.

The great interest, which American and foreign Engineers have taken in the construction of the East River Bridge, concentrates itself justly in the manufacture of the wire cables. It is acknowledged to be the most difficult part of the construction, an unusual engineering task and a work which, invented and patented by the late John A. Roebling, has been executed so far only by him and his son, the present Engineer. These facts, combined with the circumstance that the method is not generally known, have induced the writer to extend this article, originally intended to contain only some theoretical investigations, and to give, with the kind permission of Chief Engineer Col. W. A. Roebling, a detailed description of the cable-making machinery.

This will include a narrative of the course of construction, as far as finished.

He would feel satisfied, therefore, if he should succeed in contributing in the following pages something to the general information about this great work.

In manufacturing wire cables, there are three different methods in use:

First, the cable is composed of twisted wire ropes.

Secondly, it consists of parallel wire strands, previously made on shore and then hoisted or hauled in position, or

Thirdly, it is made right in place; each wire is taken over separately and adjusted; and all of them are by a wrapping combined into a cable.

The last method will be followed at the East River Bridge, and is the same which was used by the late Mr. Roebling for constructing the cables of the Niagara, Cincinnati, Alleghany and other suspension bridges.

Each method has advantages, which in

certain cases will recommend its application. A wire rope is easily handled and a cable of ropes can be formed quickly and without the aid of machinery. In small or light bridges therefore, this kind of cable is found to be most advantageous. But the tensile strength of a straight wire is ten per cent. larger, than of one twisted; hence, in large bridges a rope cable in the first place would not be economical, requiring more material and costing more per pound than one formed of straight wires. Secondly, the bulk of the former will exceed the latter by forty per cent., offering therewith so much more surface to the wind and to the corroding action of the atmosphere. This is very important, and would in case of the East River Bridge, which is exposed to great gales and to salt water air, alone decide in favor of a cable of straight wires. Finally there is great difficulty in making good attachments of heavy wire ropes with the anchor chain, which at best will always be clumsy and require more extensive masonry than the compact and neat wire connection. A cable of wire ropes for the East River Bridge, consequently must be rejected.

Considering now the second method (which, for instance, was used at the Wheeling Bridge), we perceive, that it is only applicable, if in the line of the bridge behind either anchorage there is disposable room of the length of the cable. To manufacture the strands somewhere else and transport them in place is not possible, because a strand of straight wires can not be handled like a rope, and coiling it up would render it useless.

It is evident, that in the present case, where the bridge connects two crowded cities, such available room does not exist, and, therefore, that necessity already compels us to exclude this method. But even if the location would favor it, there are other reasons, which speak against strand-making on shore. When a number of wires, laid up straight without tension, are tied together and suspended in a curve, those in the lower part are considerably higher taxed than those in the upper. This difference of tension in the single wires may amount to a loss of twenty-five per cent. in total strength. Through the elasticity of the material a certain equalization will take place, but nobody can tell to what extent, and the determination of tension in a single wire can only be hypothetical. Furthermore, it is no easy matter to handle a strand of fifty tons weight, which will exert a strain of forty tons, while being pulled over the towers and put in position. All points so far are decidely in favor of the third plan. But one great disadvantage is connected with it, namely, the loss of time involved, from the fact that towers and anchorages must be finished, before cable making can commence. This is not the case with the first two methods; wire ropes or wire strands can be manufactured while the masonwork goes on, and after completion of the latter, they can be put in place at once. The advantages of making the cables in place are, however, as we have seen, so predominant, that there could be no hesitation in adopting this method at the East River Bridge. It is hardly necessary to mention here the impossibility of moving the entire cable, finished, in its place, which opinion seems generally to prevail among non-professional people. Not to speak of the impracticability of its manufacture, the consideration—that it is 3577 feet long and weighs 870 tons, shows, that insurmountable obstacles would resist the locomotion of such a mass and that no tower could withstand the side thrust caused by the friction in taking the cable over.

Before going into details, I will premise by a description, in general outlines, of how the cables are made, using therein the dimensions and names of the East River Bridge. This will facilitate the understanding of the cable machinery and will also be applicable to any other suspension bridge by changing dimensions and names according to its size and location.

The floor of the East River Bridge, which is eighty-five feet wide, and destined to carry all kinds of traffic including railway cars pulled by endless ropes, will be supported by four cables. These are suspended in three spans : a middle or river span of 1595.5 feet between centers of towers, and two side or land spans each 930 feet from center of tower to face of anchorage or 954½ feet to point of cable attachment. Each cable consists of nineteen strands of 332 parallel steel-wires and contains therefore altogether 6308 wires, which represent a total ultimate strength of 10,730 tons. Each strand is secured with a 7 inch pin of iron to two anchor bars $1\frac{1}{2} \times 9$ inches. The wires do not pass around the pins directly, but around a cast iron shoe, which rests against the pin and which increases the curve of bending from 7 to 17 inches diameter. The last link of the anchor chain, to which the strands are attached, is arranged in four tiers. Each of the three lower ones is destined to hold five strands, the upper only four. While the strand is being made, it does not occupy the position it ultimately will have in the cable, but it hangs at an elevation considerably above. This difference in height, which in our case amounts to 55 feet in the center of river span, is produced in two ways : first, on the towers the strand rests on rollers above the saddle and at the anchorages, the above-mentioned shoe is temporarily secured 10 or 12 feet behind the anchor pin to a casting called "the leg", which is specially designed for this purpose.

After the strand is finished, the shoes are relieved from their seats on the legs and let forward into their places on the anchor bars; at the same time the strand is lowered from the rollers on top of the saddle into the saddle, which double operation causes the vertex to sink in the correct position, previously determined upon by calculation.

There are various reasons for making the strand in a more elevated position. It is clear out of the way of the main cable; the latter does not interfere with regulation, which otherwise would be the case and which would delay the operation considerably. The flat curve, in which the wires are suspended, facilitates the tying together, and the separate attachment of the shoe gives ample working room for laying the wires into it. But the main advantage is derived from the fact that the tension in the wire is nearly doubled, amounting to about three quarters of the maximum tension to which it ever will be subjected in the finished bridge. This tests the wire to a certain extent, takes out all waves and bends, and leads to the easier detection of a defective wire or splice. It gives, therefore, to the engineer more or less assurance that all wires, worked in the cable, come up to the requirements and that the latter will obtain its calculated strength.

The running out of the wires takes place from the Brooklyn anchorage, where all wire rings are received. A number of them, spliced together, are wound on wooden drums, which are large enough to contain about 12—14 wires of the length of the whole cable. The end of a wire, taken from the drum, is now temporarily fastened to the leg, and the loop, formed in this way, hung into a grooved wheel called "traveling sheave," which is firmly attached to an endless rope stretched from anchorage to anchorage. The latter, called "traveling or working rope," passes at each anchorage around horizontally placed wheels which, connected with a steam engine, give motion to it. The traveling sheave with two wires are carried over by the rope. One wire, which is fastened to the leg appears to stand still, while the other, which unwinds from the drum, runs with twice the speed of the working rope. After their arrival at the New York anchorage, the wires are taken from the sheave and laid around the shoe in such a manner, that all standing wires occupy one side of it, and all running wires the other. These two wires are now regulated according to a "guide-wire" which previously has been suspended and adjusted to the desired deflection. The same operation is repeated 166 times, until all wires for one strand are stretched. The regulating is done by men standing on light platforms called "cradles," which, supported by wire ropes, are erected at different places along the line of the cable. It is therefore necessary, that these cradles should be at such an elevation, that the wires of the strand hang about breast high to a man standing in them, so as to enable him to compare the relative deflections of the wires.

In order to reach the cradles without difficulty and otherwise to facilitate general communication between all parts of the work, there is a narrow and light "footbridge," spanning the river and the spaces between towers and anchorages, which serves for all purposes. The wires of a strand are tied together with a wire lashing about every sixteen inches, and then lowered in their final position. A second strand is made in the same manner, and so on until with the nineteenth the cable is ready for wrapping. The strands occupy in the cable a certain fixed order, and are regulated among themselves in shortening the longer ones by the insertion of an iron segment between shoe and anchor pin. All cables of the large suspension bridges, built so far, never contained more than seven strands; these of the East River Bridge are the first, which will be formed of nineteen strands. The number 7 or 19 is chosen for the reason, that the cable shall form a cylinder and a section through it shall reveal the strands in an order, that a circle can be drawn around them as tangent, (see Fig. 5.) Between 7 and 19 there is no number, which makes this possible.

After all the strands are regulated, the temporary lashings, which held the wires together, are removed, powerful screw squeezers press them in cylindrical shape, and a wrapping of No. 10 wire is put on, which finishes the cable. Figs. 1 and 2 show the position of cradle ropes, cradles and foot bridge, in elevation and ground plan; also the position of the wire-drums, anchor links, working rope and a wire, while traveling across. The guide wires, or the strands in the high position, are only indicated in the ground plan in order not to confuse the drawing. In the elevation they would hang parallel to and a little above the cradle ropes.

The general description given above, shows us that for making a wire cable the following permanent and auxiliary structures, appliances and materials are necessary:

I. Anchorages for attachment and towers for support of the cables.

II. Auxiliary structures and appliances:

1. Means and machinery for taking wires over. Traveling rope, etc.
2. Certain places along the line of the cable, from where the wires can be adjusted. Cradles, etc.
3. Means of access to these cradles. Footbridge.
4. A preliminary, carefully adjusted wire to regulate the others by,—guidewire.

III. The cable wire and method of working it into a cable.

1. The material and its treatment.
2. Regulation of wires and strands.
3. The wrapping of the cable.

All under heads I and II must be completed before cable-making can begin. The work at the East River Bridge was commenced on Dec. 26, 1869, when ground was broken for building the caisson for the Brooklyn tower foundation.

In August of 1876, after various interruptions, towers and anchorages were completed, and the first rope (one of the traveling ropes) was taken across the river. During the previous winter all machinery pertaining to traveling ropes, etc., was erected on the Brooklyn anchorage. Building cradles and footbridge, suspending the guidewires and other preparations required three-quarters of a year longer, until on May 29th, 1877, the first wire for actual cable-making was stretched. It is estimated, that it will require from 2 to 2½ years to complete the four cables.

I. TOWERS AND ANCHORAGES.

The building of the lofty stone towers and the massive anchorwalls, works of unsurpassed magnitude, has already been extensively described in various reports and scientific journals, so that I can confine myself to the repetition of some principal dimensions, especially as far as they have bearing on cable-making.

The towers, containing 40,000 cubic yards of masonry each, are built throughout of granite, coming from over twenty different quarries of Maine. Their dimensions are at high water line 140 by 59 feet, and on top below the cornice 126 by 43 feet. The summit is 272 feet, and the saddles in which the cables rest, 267 feet 6½ inches above high water. The cast iron saddles, (see Figs. 39 and 40), weighing 12 tons each, rest on 43 iron rollers of 3½ inches diameter, which are moveable on a cast-iron bed-plate, 4½ inches thick and weighing 11 tons. On the flanges of a saddle, six short stands are cast, serving as bearings for three rollers, in which the strand rests during its construction. The middle roller is placed 6 inches outside the center of saddle, the saddle

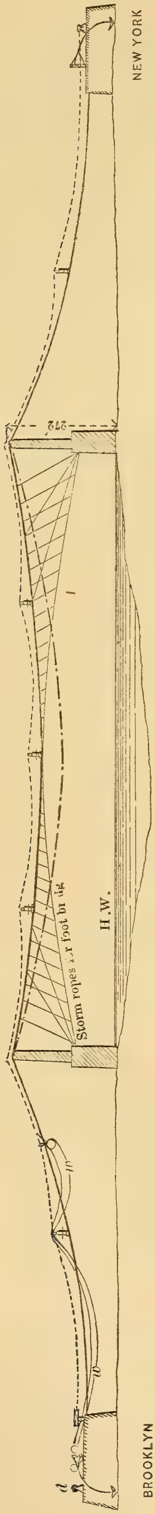


Fig. 1

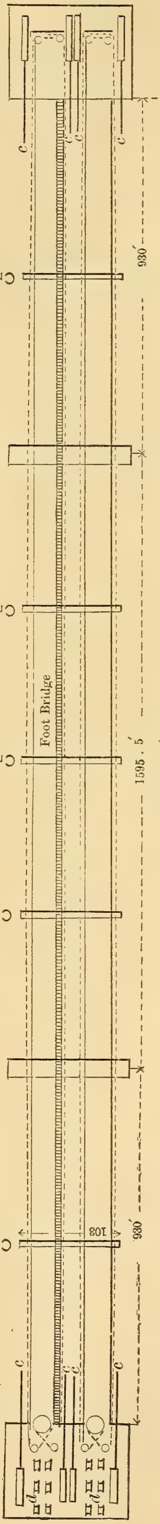


Fig. 2
Ground plan

Scale 1 inch 500 feet

Transverse Scale of Ground plan 1 inch 200 feet

fulldrawn lines, Cradle and foot bridge Ropes
dotted lines, Traveling Ropes
final position of Cable in Centre span.

d, wire drums
c, the guide wires
w, Wire, while traveling across

7 inches beyond center of plate, and the latter 12 inches outside the center of tower, all in direction towards the land.

This was done as a precaution, to provide for the probable motion of the saddle towards the river, and to prevent the resultant of pressure from intersecting the tower base at foundation further outside its center than it naturally does, owing to the difference of inclination of river and land cable. It will subsequently be shown that this motion does not exceed two inches.

The dimensions of the anchorages, built of limestone with granite corners, are at the foundation 132×119 feet 4 inches, at groundline 124×111 feet and at top 117×104 feet. In front the given widths are 10 feet less. Two arches run lengthwise through the masonry, dividing it in three piers, in which the anchor chains are bedded. The total height of the anchorage above high water is at the face 89 feet from where it falls towards the rear with $3\frac{1}{4}$ per cent.

The anchor chains consist of a double set of links, placed over each other. Each set contains ten links, arranged so, that the first two, starting from the anchorplate, are vertical, the next six form a quarter circle and the upper two are horizontal. In each link there are alternately 9 and 10 bars of sizes varying from 3×9 inches for the upper to 3×7 inches for the lower links. The pins also diminish proportionally from 7 to 5 inches in diameter.

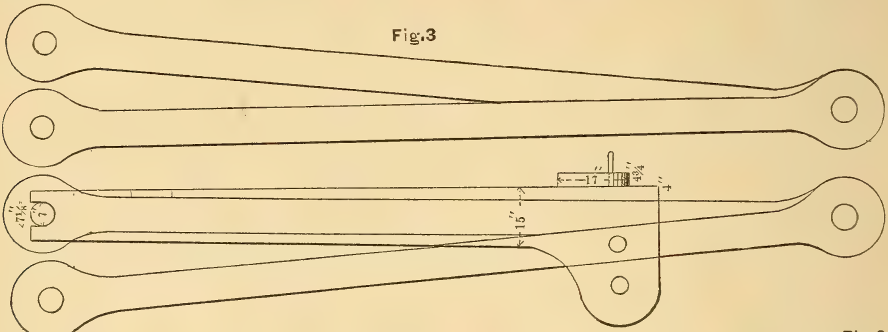


Fig.3

Scale ¼ inch=1 foot

Fig.8

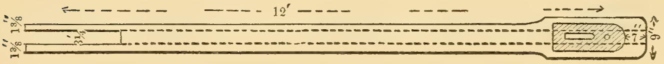


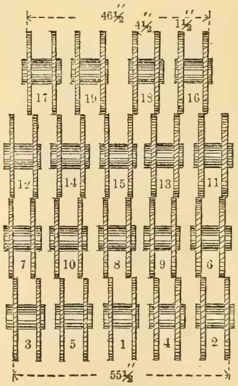
Fig.9

Fig. 3 shows in side elevation the upper set of anchorlinks to which the strands are attached. The figures (Fig. 4) written between the bars indicate the order in which the strands are made in succession.

About 10 or 12 feet forward of the shoe, the two halves of a strand are combined into one, and all strands, before leaving the masonry, are squeezed into a round cable, in which they occupy the following position: (see Fig. 5).

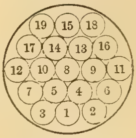
The cables as solid cylinders emerge from the anchorage eight feet below grade line of bridge.

Fig.4



Scale ¼″=1′

Fig.5



⅛″ = 1 foot

Fig.7

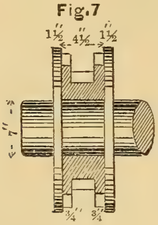
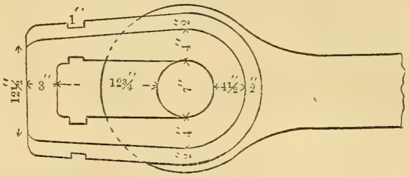


Fig.6



Scale ⅛ inch=1 foot

An important part in anchoring the strands is the "shoe" (see Figs. 6 and 7). It should combine strength with the greatest compactness in order to reduce the width of the chain to a minimum. To resist the squeezing pressure of the strand the shoe is stiffened in front by a connecting piece; but enough length should be given to enable regulation of the strand. The advantages of the shoe are various. It increases the diameter of the curve around which the wires are bent, and it holds the latter between its flanges firmly, preventing any slipping after a wire is once regulated. In removing the strand from its temporary to its final position, the shoe especially is of great advantage, as all connections for hauling are made with the shoe, saving thereby the wires from injury or displacement.

The shoe in its first position rests on the so-called "leg," of which a description may be in place here, as part of the anchorage, though being only of temporary use:

The "leg" is a trough-shaped casting about 12 to 13 feet long. It is held in front by the anchor pin and has in the rear a half round block cast to it for the shoe to rest against. Fig. 3 shows the side elevation of the leg in its position on the anchorbar, and Figs. 8 and 9 give ground plan and rear view. In order to prevent its rear end from rising through the tendency of the strand to draw upwards, a pin connects the two sides which straddle the bar.

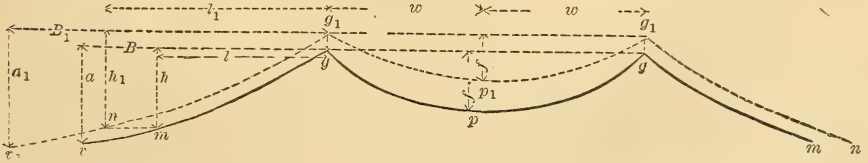
An important point in designing the leg is the determination of its length. This can be done best and surest by actual trial, in manipulating with the guidewire reversedly as afterwards with the strand. The guidewire is lifted from the saddle into the saddle rollers and both ends are pulled back until it has reached the elevation in which the strands shall be made. The distances for which the ends were pulled back give the length of legs. However, it is often desirable to have the legs cast before all other appliances are in readiness, in order not to delay the work. In such case this length must be determined by calculation.

The problem therefore is the following:

A wire, fastened at *mm* and supported at *gg*, is suspended with a deflection *f*. It is supposed that no friction exists at points *g*; hence the three parts hang in perfect equilibrium, and the tension in the wire on both sides of *g* is the same. Now the supports *g* are raised to g_1 and the points *m* moved horizontally to *n*, which causes the wire in Fig. 10 to take the position of the dotted curve with a fixed deflection f_1. The distance *m n* shall be determined. It will first be necessary to calculate the length of the whole wire in its first position, or, as the curve is symmetrical to the center line, half its length : $\overline{mg+gp}$.

If the wire were to consist of perfect homogeneous material, it would hang in a catenary. This not being the case, we have a right to assume that the weight is equally distributed over the

Fig.10



horizontal projection of the curve, or, in other words, that it forms a parabola. This assumption is the more justified by the consideration, that the future cable is uniformly loaded so heavily that the differences in own weight, calculated per foot of its horizontal projection, are vanishing.

Hence point m is a point of a parabola, which, if prolonged, would have its vertex in v. Taking point v as origin and calling the coordinates of g : a and B, those of m : $(a-h)$ and $(B-l)$, we have as first condition :

$$\frac{B^2}{a}=\frac{(B-l)^2}{a-h} \qquad (1)$$

The equality of the tensions on either side of g, gives the second condition :

$$\frac{\sqrt{B^2+4a^2}}{2a}Bq_1=\frac{\sqrt{w^2+4f^2}}{2f}wq$$

q and q_1 signify the weight of the wire per unit of length in center and side span. The actual tension being of no importance, we assume q equal to 1 and q_1, in consequence of the steeper curve equal to 1.01.

w=half of center span=799.65 ft.

f=deflection=121 feet.

The right side of above equation therefore is equal to a constant T, and we can write :

$$\frac{\sqrt{B^2+4a^2}}{2a}Bq_1=T \qquad (2)$$

Through the solution of equations (1) and (2), we find the unknown distances a and B.

It follows :

$$a=\frac{B^2h}{2Bl-l^2}$$

$$B=\frac{l^3}{2(l^2+h^2)}\pm\sqrt{\frac{\frac{h^2\,T^2}{{q_1}^2}-\frac{l^4}{4}}{l^2+h^2}+\left(\frac{l^3}{2(l^2+h^2)}\right)^2}$$

The values for l, h and T in our case are :

$$l=952.55,\ h=188.3\ \ T=2760.8,$$

which, if introduced, give :

$$a=188.48\ \ B=983.3$$

With the position of the vertex v, established through a and B, we are now able to express the length s of the curve $\overline{mg}+\overline{gp}$ in a formula :

$$s=w\left\{1+\tfrac{2}{3}\left(\frac{f}{w}\right)^2-\tfrac{2}{5}\left(\frac{f}{w}\right)^4\right\}$$
$$+B\left\{1+\tfrac{2}{3}\left(\frac{a}{B}\right)^2-\tfrac{2}{5}\left(\frac{a}{B}\right)^4\right\}$$
$$-(B-l)\left\{1+\tfrac{2}{3}\left(\frac{a-h}{B-l}\right)^2-\tfrac{2}{5}\left(\frac{a-h}{B-l}\right)^4\right\}^*$$

$a-h$ being very small, the last member simply can be called $B-l$ without committing a great error. All the values introduced, we find

$$s=1787.74 \text{ feet.}$$

The wire in the upper portion forms the curve $n\,g_1\,p_1$, the length of which also is equal to s (neglecting thereby the elongation to which the wire is subjected under the greater tension), hence:

$$w\left\{1+\tfrac{2}{3}\left(\frac{f_1}{w}\right)^2-\tfrac{2}{5}\left(\frac{f_1}{w}\right)^4\right\}$$
$$+B_1\left\{1+\tfrac{2}{3}\left(\frac{a_1}{B_1}\right)^2-\tfrac{2}{5}\left(\frac{a_1}{B_1}\right)^4\right\}-(B_1-l_1)=s \qquad (3)$$

In this equation are three unknown values a_1 B_1 and l_1, consequently, to solve the problem, we need two other equations. They are found by expressing in symbols, that the tension of both curves must be alike, and that n is a point of the parabola $\overline{v_1g_1}$:

---

* The above formula gives the length only approximately. The mathematically correct formula is the following :

$$s=\frac{p}{2}\left\{\sqrt{\frac{2f}{p}\left(1+\frac{2f}{p}\right)}\right.$$
$$\left.+\text{log. nat.}\left(\sqrt{\frac{2f}{p}}+\sqrt{1+\frac{2f}{p}}\right)\right\}$$
$$+\frac{p_1}{2}\left\{\sqrt{\frac{2a}{p_1}\left(1+\frac{2a}{p_1}\right)}\right.$$
$$\left.+\text{log. nat.}\left(\sqrt{\frac{2a}{p_1}}+\sqrt{1+\frac{2a}{p_1}}\right)\right\}$$
$$-\frac{p_1}{2}\left\{\sqrt{\frac{2(a-h)}{p_1}\left(1+\frac{2(a-h)}{p_1}\right)}\right.$$
$$\left.+\text{log. nat.}\left(\sqrt{\frac{2(a-h)}{p_1}}+\sqrt{1+\frac{2(a-h)}{p_1}}\right)\right\}$$

$$\text{in which } p=\frac{w^2}{2f}\quad p_1=\frac{B^2}{2a}=\frac{(B-l)^2}{2(a-h)}$$

For flat curves both formulas are almost identical. In our case the error is less than $\frac{2}{1000}$ of a foot.

$$\frac{\sqrt{B_1^2+4a_1^2}}{2a_1}\,B_1q_1=\frac{\sqrt{w^2+4f_1^2}}{2f_1}\,wq=T_1 \quad (4)$$

$$\frac{B_1^2}{a_1}=\frac{(B_1-l_1)^2}{a_1-h_1} \quad (5)$$

In the last three equations is :

$f_1=66$ft., $h_1=h+\overline{gg_1}=190.42$ $T_1=4909.8$ w_1, q and q_1 have the former values.

From the introduction of these values, follows :

$$B_1=1406.05, a_1=210.07\ l_1=964.65.$$

hence

$\overline{mn}=l_1-l=964.65-952.55=12.1$ feet, which represents the length of leg. Owing to a somewhat larger stretch, than was anticipated, in the cradle ropes, the above deflection of 66 feet was a little increased, in order to hang the strand most conveniently for the regulator.

This necessitated a shorter length of leg than calculated, which finally was fixed to 11 feet $10\frac{3}{4}$ inches.

II. AUXILIARY STRUCTURES AND APPLIANCES.

1. *Traveling rope.*—The "traveling or working rope" or "traveler" consists of a $\frac{3}{4}$ inch steel wire rope, which forms an endless line around certain wheels placed on each anchorage.

Fig. 11

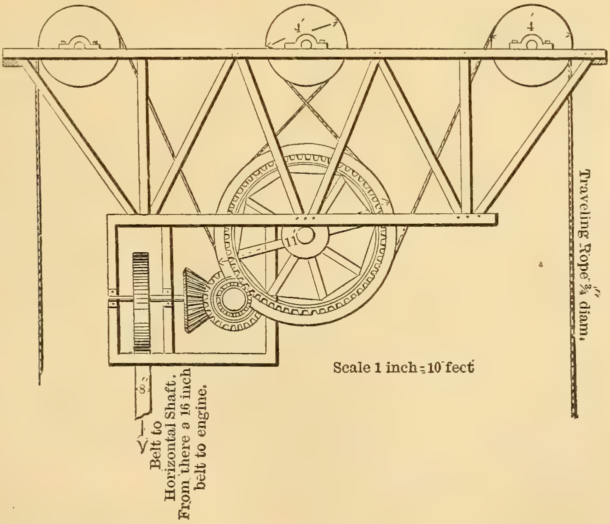
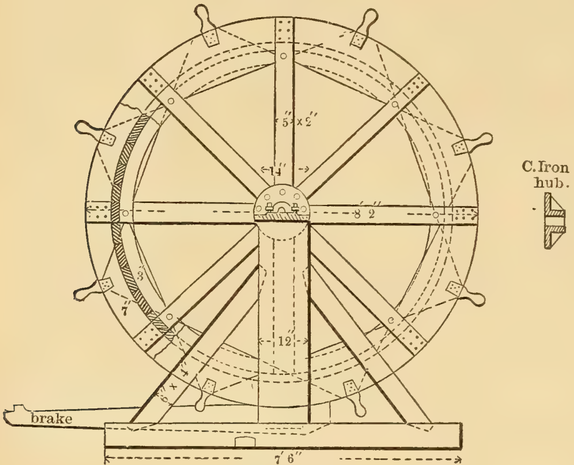


Fig. 11 represents the arrangement on the Brooklyn side. The rope makes first two turns around a double grooved wheel 11 feet in diameter, called "driving wheel" and then passes around two smaller ones, called "guiding wheels," which keep the ropes in the desired distance from each other. On the New York side two similar wheels do the same service, which are fixed in a movable frame, so as to enable taking up slack in the rope. The driving wheel has a gearing on its circumference in which a pinion works of 15 inches diameter. On the pinion shaft is a mitre wheel in which are matched two others, both worked by a lever to throw them in or out of gear. The motion of the driving wheel and working rope can by these means be reversed. A 3 feet pulley on the shaft of these bevel wheels receives its motion by an 8 inch belt from another horizontal shaft which runs along the front face of the anchorage and which, by a 16 inch belt, is in connection with a steam engine located in the yard below. The 8 inch belt runs loosely on the pulley and receives adhesion by tightening pulleys which enable the working rope to be set in motion or stopped, without stopping the engine. The engine has a 12 inch cylinder with 24 inches stroke, carries from 60—75 lbs. of steam and makes about 70 revolutions per minute. This corresponds to a speed of $4\frac{2}{10}$ feet per second of the traveling rope, which therefore requires 14 minutes for a trip from anchorage to anchorage. The whole machinery, described above, is double; each set doing the work of carrying wire for two cables. The driving power is derived from the same steam engine. During its passage from anchorage to anchorage, the working rope is supported on the towers and on each cradle by properly placed sheaves. In close connection with the working rope is the "traveling sheave."

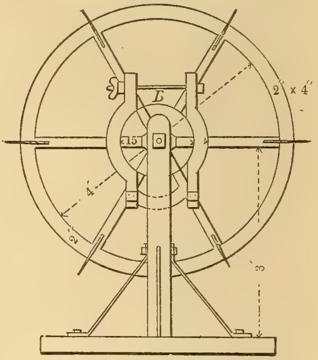
This is a light wooden wheel 5 feet in diameter with a grooved rim of zinc, in which the wire hangs, while traveling across. It is secured to the traveler by an iron, bent in the shape of a gooseneck, so as to allow free passage of the rope over all supporting shears. An iron rod with a weight at the end, fastened to the hub of the wheel, keeps it in vertical position and prevents it being upset by the wind. Each working rope carries two traveling sheaves placed so, that one is at the New York anchorage, while the other is at the Brooklyn anchorage. When, for instance, the left sheave carries a wire from Brooklyn to New York, the right one moves empty in opposite direction. When the latter arrives at the Brooklyn anchorage, the driving wheel is stopped, a wire placed on the same, and the motion of the working rope is reversed.

Fig. 12



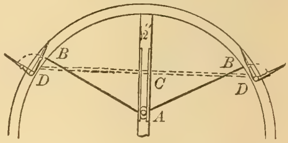
Wire Drum
Scale ¼" = 1 foot
Clear width of drum = 15" feet
width over all = 21" feet

Fig. 13



Scale 1 inch = 3 feet

Fig. 14

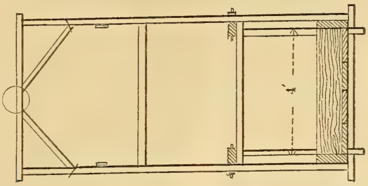


Other appliances in the service of stretching wire are the wire drums. There are altogether 32 drums, 8 for each cable, the chief object of which is to serve as reservoirs of wire ready for being worked into strands. Each drum is 8′ 2″ in diameter, 15″ wide and can hold about 50,000 feet wire, which is enough for six to seven trips of the traveling sheave. It is provided with a brake by means of which the sag in the running wire can be regulated. It is necessary that the wire is wound tight on the drum, else the brake is ineffective. The wire ring therefore is placed first on a smaller wheel (Fig. 13), from which it is unwound on the large drum under considerable tension achieved by the brake *R* on the arms of the small drum. Small movable flanges (Fig. 14) admit putting the wire on and hold it afterwards in place.

2. *Cradles and Cradle Ropes.*—The cradles are necessary for various purposes. Their chief object is, as stated in the general description, to serve as platforms for men to stand on, who regulate the wires. They also are of value as intermediate supports of the traveling ropes and as a check for the lateral displacement of the wires by wind. Finally they serve as support to a few men, whose duty it is to throw the running wire on the other side of the strand.

There are altogether five double cradles, three in the river span and one in each land span. Owing to the fact, that the cables in the land span do not hang parallel to the axis of the bridge,

Fig. 16



Cradle

Fig. 15

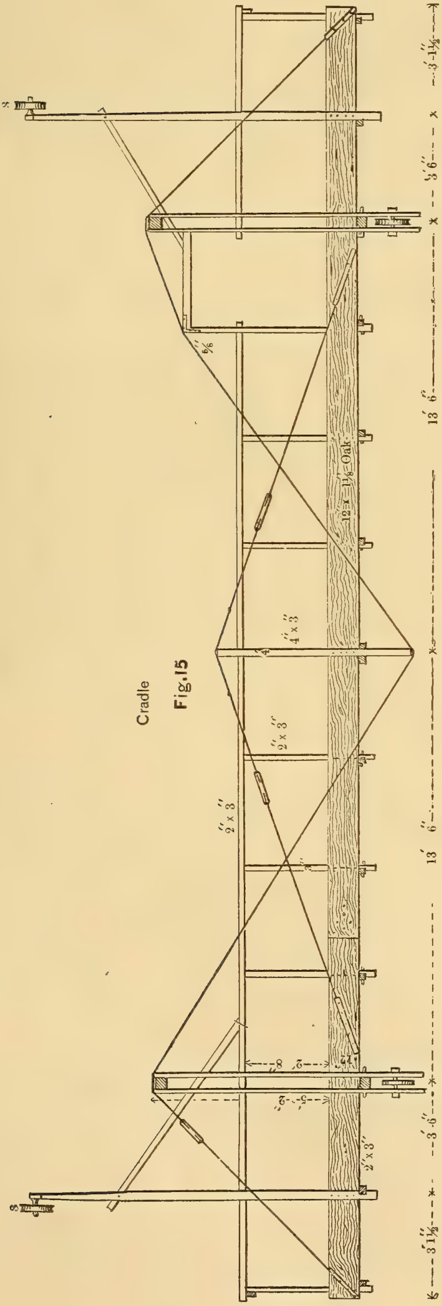
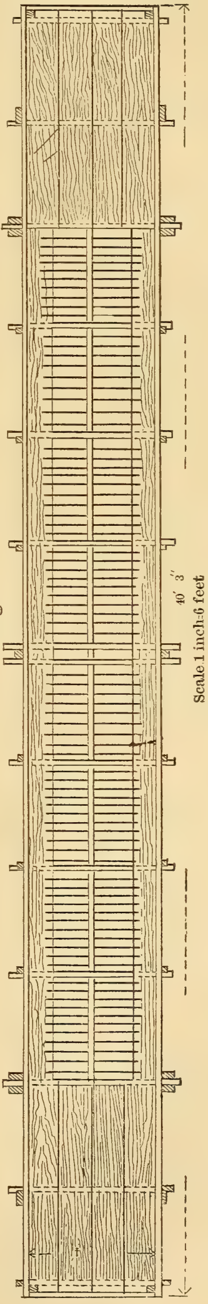


Fig. 17



Scale 1 inch-6 feet

but spread from anchorage towards tower, the single cradles have different lengths, those in the main span being 7 feet longer than the land cradles. On the opposite page one of the land cradles is illustrated in ground-plan, front and side elevation.

They are built throughout of oak and strengthened by light trussrods. The larger part of the platform consists of an iron grating for the free passage of the wind, to which they are dangerously exposed in their lofty position.

The *cradle cables* which support the cradles, consist of 2⅝ inch crucible cast steel ropes with an ultimate strength of 180 tons each. One of the four ropes, which at the same time supports the footbridge is 2⅝ inches in diameter, and able to sustain 240 tons. The following is the weight on each cradle rope:

Its own weight (9 lbs. per foot).....	14,580 lbs.
One half of three cradles...........	6,000 lbs.
Working rope.....................	600 lbs.
About 6 men......................	960 lbs.
	22,140

It is suspended with a deflection of 73′3″ hence the largest tension in the rope is:

$$22140\frac{\sqrt{800^2\times4\times73.25^2}}{4.73.25}=61000\text{lbs.}=30\tfrac{1}{2}$$

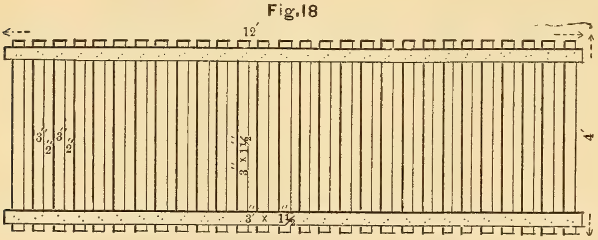
tons. This gives a margin of safety of 5.8 times.

On top of towers the cradle ropes rest on wooden blocks, and at each anchorage they are attached to separate anchor bars by means of wrought-iron sockets and screw stirrups, similar, only differently proportioned, to the connection shown in Fig. 21 and 22.

3. *The Footbridge.*—The footbridge has no direct service in cable making, but its indirect values are so great, that for the manufacture of large cables its erection may be deemed an absolute necessity. All the footbridges used by the late Mr. Roebling were built after the general plan of suspension bridges, namely, they consisted of a platform suspended to two cables. The writer of this, while working out the plans, suggested laying a platform directly on the cables, guided by the consideration, that a bridge parallel with the strand would enable the regulator to sight it at all points, that access to the cradles would be easy and that it would form a stiffer platform, which probably would suffer less under the violence of terrific gales, which rage almost weekly on the East River. Partially by these reasons, but especially for the accommodation of shipping, the Chief Engineer adopted this plan, and it was executed accordingly.

The floor of the footbridge (Fig. 18) is formed of oak slats 3″×1½ inches, 4 ft. long, set two inches apart, and nailed to two longitudinal strips of equal size. It is made in sections of twelve feet length and rests on the main footbridge ropes, to which it is secured with small screw stirrups (see Fig. 19).

Fig.18



Scale ¼ inch= 1 Foot

Fig.19

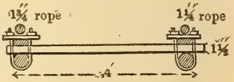
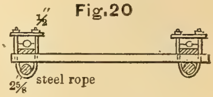


Fig.20



The main footbridge cables consist of 2⅝″ steel ropes, each having an ultimate strength of 240 tons. They are assisted by two auxiliary ropes of 1¾ and 1¼ inches in diameter; the heavier rope being on the side which also supports the cradles. The connection of these ropes with the floor is shown in Fig. 20.

The anchorage of the footbridge ropes is illustrated by Figs. 21 and 22.

The socket for fastening the rope in, is made of the toughest wrought iron, contrary to the usual cast sockets of this kind. This precaution was taken to exclude any danger of bursting, which, in a case like the present, would be followed by the greatest calamity.

The principal means for guarding the footbridge against gales, are two 1¼ inch ropes which, fastened at the towers, are,

Fig.21

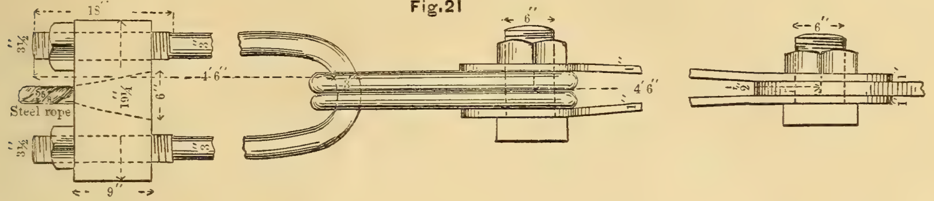
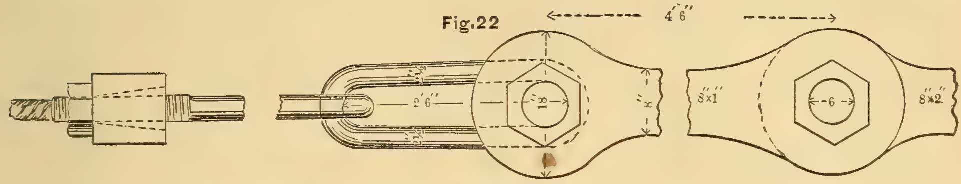


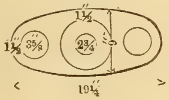
Fig.22



Scale ½ inch=1 foot

in shape of inverted parabolas, suspended from the footbridge by inclined suspenders (see Fig 1). These "storm ropes" are assisted by a number of storm stays, which reach from the tower out as far as the first cradles. In the landspans the storm guys are anchored in the ground. This whole system of guying has proved very successful, as even in heavy gales the motion of the bridge is confined to a small lateral displacement.

Fig.23



The maximum load coming on one footbridge cable is 62123 lbs. creating a tension of 86 tons. This is resisted by the aggregate strength of the $2\frac{5}{8}''$ and $1\frac{3}{4}$ inch rope amounting to 318 tons; hence the margin of safety is 3.7 times.

All the ropes used for the footbridge, cradles, etc., were manufactured in the works of John A. Roebling's Sons, Trenton, N.J.

The footbridge ropes were first suspended with a deflection of 64.4 feet and sunk to 74.2 feet after the load was on (a foot more than anticipated). This amounts to a stretch of 2.26 feet in the whole ropes, or to $\frac{1}{11500}$ of the length, per square inch of section, per ton of strain.

The erection of all these structures was a difficult and perilous task, considering that the lowest point is 200 feet above the water, and that about 100 craft of all kinds cross the line of the bridge every hour.

The first rope, taken over, was one of the travelers. Coiled on a reel, it was placed on a scow at the foot of the Brooklyn Tower. Its end was taken over the tower to the anchorage and temporarily fastened. Then the scow was towed across and the rope allowed to sink to the bottom of the river. The remaining part of the rope was taken from the reel and the end after passing over the tower, fastened to the drum of a steam engine. By observation it was ascertained, that most every day an accidental coincidence of circumstances keeps for 6–7 minutes the line under the bridge clear of vessels. Of such a chance advantage was taken, and in the forenoon of Aug. 14th, 1876, a cannon shot gave the signal for cutting the lashing on the Brooklyn dock and setting the engine in motion. Four minutes later the rope came out of the water and in six minutes hung clear over all masts and formed the first connecting link between the cities of New York and Brooklyn, destined never more to be broken. Simple as this operation was, it created considerable excitement and interest among the population, probably caused by a feeling of historical importance for the day, which practically should unite the two cities. Thousands of spectators lined the shores, who greeted with loud cheers the appearance of this little rope, as it rapidly ascended high up in the air. It seemed that all doubts, hitherto entertained as to the erection and subsequent safety of the bridge, had vanished from that moment. The East River Bridge was considered an established fact.

The other half of the traveling rope was taken over in the same manner and then the ends of both were taken to the New York anchorage and spliced together, making the rope endless.

In stretching the second traveller, use was made of the one already in position. The new rope was lashed to it and pulled over by the engine. Afterwards these lashings were cut away, which was performed by men running out on the traveler in a boatswain's chair or a "buggy." The latter (Fig. 26) consists of a platform four feet square, suspended by four iron rods to a grooved six inch roller, which runs on the rope.

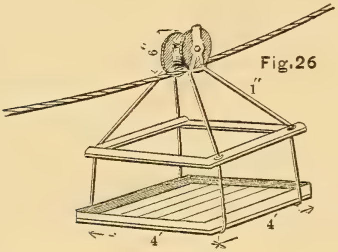


Fig.26

An auxiliary, so called carrier rope, of 1¾" diameter was the next one stretched across. Its object was to carry the load of the footbridge and cradle ropes which were too heavy for the working rope. This carrier was hoisted up from out the water after the first method. The footbridge and cradle ropes were put in place in the following manner. Each rope was landed at the Brooklyn tower, its end hoisted up, passed around a sheave attached to the traveler, and lashed to a hemp rope which was connected with the drum of an engine at the New York anchorage.

As the rope moved on, it was supported by a "hanger" (see Figs. 24 and 25) every 50—60 feet, which consists of two hooks, for the rope to rest in, attached to a roller that runs on the carrier rope. These hangers were afterwards removed, leaving the rope suspended clear from the carrier.

After all ropes were in place, the cradles were hoisted on the towers and allowed to slide down to their proper positions, in which they are secured by heavy lashings.

Fig.24

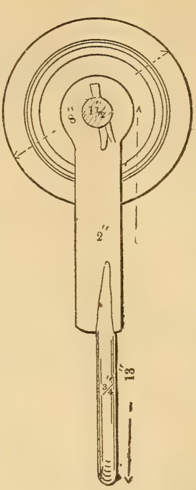
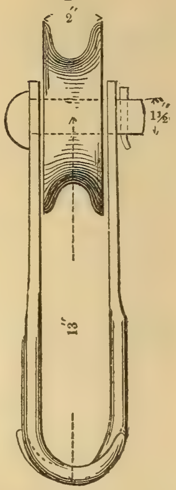


Fig.25



Scale ⅛ inch = 1 inch

Laying the footbridge floor was commenced simultaneously from both anchorages and the center of main span. It occupied but few days. Two light ropes, serving as handrails, lastly put in place, completed the footbridge, which since has been crossed by thousands of people.

REPORTS OF ENGINEERING SOCIETIES.

American Society of Civil Engineers.—The following papers are to be found in the Society's Transactions for June and July :

Connected-Arc Marine Boilers; a demonstration of the Principles of their Construction, by C. E. Emery.

On the simultaneous ignition of thousands of Mines, and the most advantageous grouping of Fuses, by J. H. Striedenger.

Improvement of Entrance to Galveston Harbor, by C. N. Howell.

The July number also contains an important discussion on the failure of the Ashtabula Bridge, by Messrs. A. P. Boller; E. Warren; E. S. Philbrick; C. Hilton; Thomas C. Clarke; Robert Briggs; C. Shaler Smith; J. Cooper.

The Institution of Civil Engineers.—The session which ended in May has been one of more than usual activity both in respect of the ordinary operations of the society and of the interest aroused by certain changes proposed to be made in its constitution. The meetings began on November 14 last, when a Paper was read on "The Lighthouse System of Japan," which elicited much valuable information on the best form of structure calculated to withstand earthquake shocks. The discussion also afforded an opportunity for the former presentation of an interesting collection of specimens of the mineral products of Japan received from the Imperial Government. On this occasion Mr. Wooyeno, the Japanese Minister, was present, and personally responded to the vote of thanks passed to his Government. The next Paper, on "The Fracture of Railway Tires," sought to reduce to theory the difficult problem of the strains generated in tires by changes in the molecular constitution of the materials of which they are composed. Following upon this, and initiated by a Paper on the "Chalk Water System," was a discussion which lasted three evenings, the great question of metropolitan water supply being considered from widely different standpoints. The statutory annual general meeting of the members of the corporation was held on December 19. The presentation of the medals and premiums having been concluded, an important debate ensued upon the proposals of the Council for the establishment of another grade, intermediate between the existing members and the associates, and—much diversity of opinion existing—it was agreed to consult the whole body as to the title recommended by the Council for the new class. In the sequel, the difficulties of making any radical change in the case of an old-established corporation were fully realized, for though a majority supported the Council, a large and influential minority deprecated any change, and at a special general meeting, held subsequently, it was resolved that the object sought could be better attained by a literal interpretation of the existing bye-laws than by the enactment of new ones.

The ordinary meetings were resumed after the Christmas recess by the reading of a Paper "On the Repairs and Renewals of Locomotives." This was followed by one "On the Combustion of Refuse Vegetable Substances for raising Steam," a subject of considerable though indirect interest to the general public, for in it is involved the question of the cost of importing flour from the great corn-growing countries of south-eastern Europe, where coal for the threshing engines is not to be had, and wood is getting dearer and dearer. Next ensued a discussion upon "The Sewage Question," which occupied five evenings, thus testifying to the interest felt in the subject and to the eagerness with which the solution of its difficulties would be hailed. Attention was then directed to "Transmission of Power," in which various systems and agents were alluded to. A Paper "On the River Thames" could not fail to excite attention at the present time, though the discussion rather turned upon the "regulation" of the river than upon the causes of late floods. To this succeeded an account of a "Deep Boring for Coal at Scarle in Lincolnshire," and a Paper "On Street Tramways," and these gave place to one "On the History of the Modern Development of Water Pressure Machinery," in which was traced the growth of the present system of applying hydraulic power to numberless purposes formerly supposed only capable of being met by steam. A rather significant account of an improvement in detail affecting the cost of making gunpowder charcoal completed the ordinary business of the session, which was closed, in accordance with a time-honored custom, by a conversazione given by the president this year at the South Kensington Museum on Thursday evening. The papers noticed will, with matter from various other sources, form material for four octavo volumes of "Proceedings," two of which have already been issued, and the others will follow in August and November respectively. This account of the sessional work of one of the Institutions may be fitly closed by a few particulars respecting its position. The Institution of Civil Engineers was established in 1818 "for the general advancement of mechanical science, particularly civil engineering," and now numbers upwards of 3,104 members of all classes.

It is the custodian of trust funds amounting to more than £14,000, the interest upon which is devoted to premiums for meritorious original communications made to the society. It also possesses funded property available for the general benefit of the society of £22,500, has an annual income of between £10,000 and £11,000, and possesses an unique and unrivalled engineering library of 13,000 volumes.

IRON AND STEEL NOTES.

Homogeneous Iron.—A general meeting of the members of the Institution of Mechanical Engineers took place on Thursday, the 31st May, at the Institution of Civil Engineers. A discussion took place on Mr. Kirk's paper "On Homogeneous Iron and the degree of Homogeneity to be expected in Iron produced by various systems of Puddling and subsequent Working." Mr. Kirk made some preliminary remarks, in the course of which he stated that the opinion expressed in his paper that the Danks furnace did not give much promise of homogeneity in its products, because of the difficulty of expelling the large quantity of thick cinder usually present, had recently been to some extent modified by the results of some experiments. He thought that rotary furnaces were adapted to the production of a steely iron, though he had found that the two ends of a bar produced from iron puddled in a Danks furnace had given analysis which showed the percentage of sulphur to vary between about 0.3 and 0.23 per cent. He also remarked on the general superiority of crystalline iron, saying that most of the bar iron which will stand bending and doubling cold is of a crystalline character, and that usually this class combines to a greater degree the properties of toughness and elasticity than that which breaks with a fibrous fracture. The latter characteristic he attributed to the presence of cinder, and suggested that cinder having practically no tenacity, the strength of iron in which it existed must be reduced in proportion to the quantity contained. Dr. Siemens agreed generally with Mr. Kirk that a crystalline structure was not antagonistic to toughness, and that it generally indicated a freedom from cinder. Cinder, he said, could be of no service in the composition of iron. He then referred to the papers which he read in 1868 before the British Association dealing with the question of boiling in the puddling furnace, and containing his suggestion that as much weight of puddled bar should be obtained as there was of iron put into the furnace. The iron is not so much oxidized in the rotating gas puddling furnace as in the ordinary fixed furnace, the flame being more of a neutral character. Reference was then made to the very successful working of the gas furnaces at Messrs. Nettlefolds, Wellington Works, the results of their operation throughout eighteen months being of a most satisfactory character; 12,384 tons of bars had been made, and the loss of iron in the furnaces was only 3.8 per cent. Dr. Siemens had more recently turned his attention particularly to the production of wrought iron from the ores. It was, he said, a difficult problem, but he believed he might consider it now practically, though not commercially, solved. He was using Northamptonshire ores, containing 30 per cent. of metallic iron and about 2.5 per cent. of phosphoric acid, equal to about 4 per cent. of the iron in the ore, though only about 0.1 per cent. of phosphorus was found in the iron produced. The iron was produced by means of a considerable admixture of silex in the rotary gas furnace, and the explanation of the absence of phosphorus was referred to the neutral character of the flame, which admitted of its being carried off with the slag, instead of being combined with the silicon, and thereby with the metallic iron, as was the case in the blast furnace. Mr. Bell could not see in what way the presence of cinder influenced the strength of iron, as, assuming the iron and cinder to be in layers, the iron would still be in a condition to exercise its resisting properties, and cinder could not render iron brittle. He could not agree with Dr. Siemens with reference to the possibility of even an approach to a neutral flame, as carbonic oxide was introduced into the furnace with the gas, and a very small quantity of carbonic oxide would oxidise a considerable weight of cast iron in an ordinary furnace. He was inclined to think that the good results obtained from rotary furnaces was due mainly to the mechanical regularity and quickness of the apparatus, and that the removal of the phosphorus described by Dr. Siemens was not at all due to the nature of the flame, but that it was entirely a question of temperature. In the Siemens furnace the temperature was kept below that at which phosphorus combines with iron, and as iron oxidises so much more readily than phosphorus or sulphur, the latter in the Siemens furnace have no opportunity of combination with the former. Hence the freedom from these described by Dr. Siemens. Mr. Crampton spoke particularly with reference to the good results obtained by Price's furnaces, over 1000 tons of iron having recently been obtained, which was within one per cent. of the quantity put into the furnace. Mr. Crampton was disposed to attribute most of this success to the quickness with which the operations were conducted, and to the fact that the fuel was passed into the furnaces in an incandescent condition, and that no cold air was at any time admitted. In many cases the excess of yield was due to the fettling. Mr. Jeremiah Head thought that the meaning attached to the word homogeneous, as now so often used with reference to the properties of iron and steel, should be defined, and that it would be regretted if any misapplication should lead to such words having different meanings in different languages, though originally they had one common derivative. He thought that the experiments of Tyndall on the adhesion of surface plates suggested that the presence of cinder must certainly tend to the loss of strength in iron. One surface plate supported the lower one by attraction of cohesion; the attraction of magnetism decreased as the square of the distance between the objects, and from these facts it might be argued that the presence of any impurity which kept the particles of iron asunder reduced the strength of the whole, and cinder acted in this way. He referred to the fact that the difference between the strength of rolled iron with and against the grain was most apparent in long narrow plates and bars, and that the Lowmoor Company are now putting down rolls 11 ft. to 12 ft. long, so that the rolling action may act orthogonally, and the plates produced may be equal in strength in all directions.

RAILWAY NOTES.

New Zealand Railways.—The New Zealand Government has ordered surveys to be made for a railway between the Thames and the Waikato. The new line is expected to prove one of the most successful railways in New Zealand.

Canadian Pacific Railway.—Mr. Sanford Fleming has reported on the surveys and preliminary operations carried on in connection with the Canadian Pacific Railway. Mr. Fleming states that the surveys, commenced in 1871, now extend from the valley of the Ottawa, west of the capital, to that portion of the Pacific cost lying between Alaska on the north and the Straits of San Juan de Fuca on the south. The cost of the surveys to the close of December, 1876, had been $3,136,165. From Burrard Inlet to Montreal will be 633 miles less than the distance from San Francisco to New York. It is estimated that the Canadian route will bring New York, Boston, and Portland from 300 miles to 500 miles nearer the Pacific coast at Burrard Inlet than these cities now are, with San Francisco as the terminal point of the Pacific Railroad. The distance from England to China will be more than 1000 miles less by the Canadian than by the American line. Ten routes for the Canadian Pacific line have been opened for consideration; they terminate on the coast of the main land at seven distinct harbors, and they all converge to Yellow Head Pass. The line has been located with sufficient accuracy to admit of the construction of an overland telegraph.

ENGINEERING STRUCTURES.

Active preparations are going on for the immediate commencement of the long-projected work of draining the Zuyder Zee. A dam of 40 kilometers—24 miles 1504 yards—long, and 50 metres broad at its base, is to be carried across the gulf, but up to a height of ½ meter above the ordinary level of high tide. Upon this pumping machines of 10,000-horse power will be erected, capable of pumping up from the enclosed sea and discharging on the outside of the dam 6,500,000 cubic meters of water daily. Taking the average depth of the water at 4½ meters, it is estimated that the work of pumping will be completed in about sixteen years from its commencement. The total cost of reclamation is set down at 335,000-000f., but, huge as this sum is, the undertaking is confidently looked upon as likely to prove a most remunerative speculation. The success of the scheme will add to the kingdom a new province 195,300 hectares—or nearly 500,000 acres—in extent. Judging from previous experiences in connection with the Haarlem Sea, it is reckoned that at least 176,000 hectares of the land thus won will be applicable to agricultural purposes, which, at an average value of 4000f. only per hectare, will richly repay the enterprise and treasure lavished on the gigantic undertaking.—*The Farmer.*

Passage of the Suez Canal by Deep Draught Ironclads.—Mr. Latimer Clarke, C E., read a paper on the 11th inst. before the Royal United Service Institution, "On the Employment of Clark and Stanfield's Floating Docks at Naval Stations and the means they afford of transporting heavy Ironclads through the Suez Canal." Mr. Donald Currie occupied the chair. Mr. Scott Russell, Captain J.C.R. Colomb, R.M.A., Mr. Rennie, and other well-known gentlemen took part in the discussion which followed, and which was prolonged to an unusually late hour, the subject being one of both pressing importance and great interest, the paper containing many startling facts which it would be well to calmly consider in time of peace to prevent ourselves being found unprepared in time of war. The real economy of providing in time, and at our leisure, the necessary means of repair to our complicated fighting vessels at our distant vulnerable points, was forcibly pointed out, and indeed there can be no doubt that few are really aware of the helpless state into which our naval forces at long distances from home could be speedily thrown for want of a few coaling stations, cheap and portable docks, and the necessary telegraphic connections.

The transport of our heavy ironclads through the Suez Canal has always been considered a sheer impossibility, but these floating docks, provided with air cushion blocking, acting somewhat similarly to the well-known Russian air camels, appear to efficiently overcome the difficulties presented by the problem; it is very obvious that in time of war the practical value of our most powerful ships would by these means be enormously increased.

ORDNANCE AND NAVAL.

A New Military Invention.—The German General Berden has devised a new distance-calculator, or, in military language, "range-finder." When closed up ready for moving it looks like a sort of primitive chariot, mounted on two large light wheels and drawn by one horse. Two men ride upon the comfortable seat, the driver the operator, and for the latter, so simple is the instrument that no special training is necessary, however desirable it may be that he should also understand the principle of trigonometrical science. When the range of any point is to be taken, before a battery begins to play, for instance, or a regiment to fire, the horse is slipped out of the shafts, and the body of the "chariot" then turned completely over on its axle. The frame on which is built the seat, then serves as a firm and steady support for the instrument. The body of the vehicle is revealed as a box or case one metre wide, nearly two long, and about a foot deep; and when its two opposite ends are thrown open, the instrument is ready for use. This consists, loosely described, of two parallel telescopes, about 1½ metres long, and very powerful, affixed to a frame which swings on a common pivot. The sight ends are just one metre apart, and this is, of course, the base line of the calculation. One of these telescopes, the one at the right hand, is movable only with the frame, of which both form a part. This is first sighted on the object, and the frame is made fast, the first step being thus completed. The other telescope is adjustable further, on a pivot of its own, and by means of a small wheel is turned to the right angle of convergence with its companion—that is to say, until it too covers the object. Now, having the base line and the angle of convergence, any surveyor could, of course, calculate the distance. But in General Berdan's instrument the wheel which adjusts the second telescope is marked off into meters, centimetres, and milometres, is covered by a little hand or pointer, and when the object is brought within the focus the pointer indicates exactly the distance. This is absolutely all there is of it. In two minutes the instrument can be unlimbered, put into readiness, and a distance found, less time therefore than a gun or a battery is made ready. Then the two ends are closed, the body swung around into its place, the horse put into the shafts, the driver and operator mount the box, and away they dash to some other point.

BOOK NOTICES.

British Industries. Sea and Salmon Fisheries. By E. W. H. Holdsworth and Archibald Young. London: Edward Stanford. For sale by D. Van Nostrand. Price $2.25.

The two authors treat respectively of Sea Fishery and Salmon Fishery.

The book is descriptive only of methods pursued in Great Britain. The description of apparatus is made clear by good diagrams.

The contents are: Sea Fisheries; English Fisheries; Scotch Fisheries; Manx Fisheries; Irish Fisheries; Salmon Fisheries.

The growing interest in this latter subject, under the stimulus of the Commissioners of State Fisheries in this country, will doubtless create a demand for this little book.

Common-Sense for Gas Users. A catechism of Gas-Lighting. By Robert Wilson, C.E. London: For sale by D. Van Nostrand. Price $1.00.

This book is designed for consumers of gas rather than for Gas Engineers.

It contains much good advice regarding selection and testing of burners, care of meters, etc., put in the plainest possible style. The illustrations are numerous and good.

An appendix treats, in a brief and satisfactory way, of the utility of gas engines.

A General Classification of Railway Rights, Realities, and Personalties. By George T. Balch, C.E. New York: J. J. Little & Co.

This work is designated to facilitate the labor of taking an inventory of railway property. It is illustrated by the example of the Receiver's Inventory of the property of the Erie Railway Company, as prepared for the Supreme Court of the State of New York. Not a widely useful book, but invaluable in certain cases.

Handbook of Natural Philosophy: Mechanics. By Dionysius Lardner, D.C.L. New edition. Edited by Benjamin Loewy, F.R.A.S. For sale by D. Van Nostrand. Price $3.00.

The original work of Dr. Lardner has been used so far as the elementary exposition of principles is concerned, but the whole has been revised by the new editor, so as to bring it in accord with modern scientific terminology.

The present issue is the first of five proposed volumes, to treat respectively of Mechanics; Hydrostatics; Pneumatics; Heat; Optics; Electricity; Magnetism, and Acoustics.

The work has always deservedly enjoyed a high reputation for clearness of exposition of the most abstract principles of physics.

VAN NOSTRAND'S SCIENCE SERIES, NO. 30.

The Magnetism of Iron Vessels. By Prof. Fairman Rogers. New York: D. Van Nostrand. Price 50 cts.

The readers of the Magazine have already been made familiar with this able work, through the pages of the numbers for May and June last.

It is rare that so much science is found in so compact a form as is now offered in this little treatise.

It is designed for the use of all engaged in navigation, especially to yachtsmen and scientific travellers, as a simple introduction to the subject, and a guide in such observations as they may feel disposed to undertake.

An Elementary Treatise on the Integral Calculus. By Benjamin Williamson, M. A. Second Edition, Revised and Enlarged. London: For sale by D. Van Nostrand. Price $5.25.

This work, the first edition of which we noticed last year, appears to have satisfied a wide-spread want on this side of the Atlantic. An examination of the table of contents will reveal the fact that applications of the Calculus occupy considerable space, nearly half the book.

I. Elementary Forms of Integration. II. Integration of Rational Fractions. III. Integration by Successive Reduction. IV. Integration by Rationalization. V. Miscellaneous Examples. VI. Definite Integrals. VII. Areas of Plane Curves. VIII. Lengths of Curves. IX. Volumes and Surfaces of Solids. X. Moments of Inertia. XI. On Mean Value and Probability.

Water Supply Engineering. By J. T. Fanning, C. E. New York: D. Van Nostrand. Price $6.00.

This is an elaborate treatise relating to the hydrology, hydrodynamics, and practical construction of public water supplies.

Our author says in his preface, "This work is intended more especially for those who have already had a task assigned them, and who as commissioners, engineers, or assistants, are to proceed at once upon their reconnoissance and surveys, and the preparation of plans for a public water supply."

A perusal of the contents reveals a systematic, progressive method of treatment of the general subject, that leads the engineer confidently forward from his reconnoissance in the field to the delivery of water through the arterial net-work of distribution pipes and fire hydrants, amid the households and warehouses of a town or city.

The book is divided into three sections, and treats upon—1st. The Collection and Storage of Water, 160 pages; 2d. The Flow of Water through Sluices, Pipes, and Channels, 172 pages; 3d. The Practical Construction of Water Works, 259 pages.

An introductory chapter discusses the physiological and financial influences of a liberal distribution of wholesome water throughout a city, and points out their values, especially those incidental values that do not appear upon the balance-sheet of the City Treasurer, but which are invariably of far greater importance respecting the welfare of the city than the construction, maintenance and income accounts.

The introductory chapter is followed by statistics and tables of water supplied and consumed in various American and foreign cities, and of the ratios of consumption during the different seasons, days of the week, and hours of the day, and of the reserve and conduit capacity necessary to provide water for the uses of the fire department.

The useful statistics here collated, and illustrative diagrams, will prove of exceeding value to engineers and commissioners who have to estimate quantities of water that must be provided in new water supplies, since the quantity of water required is a controlling element in the preliminary investigations, in the proportion of conduits and distribution pipes, and in the capacities of reservoirs, filters, and pumping machinery.

The hydrology of water supplies is discussed in the several chapters upon *Rainfall, Flow of Streams, Storage and Evaporation of Water, Supplying Capacity of Water-Sheds,* and upon the supplies from *Springs and Wells.* These chapters relate more especially to the hydrology of the United States.

The rainfalls are first classed into Western or Pacific coast, Central or Mississippi valley, and Easter or Atlantic coast systems, and the influences controlling each examined. The rainfalls of the principal river basins of each main system are then independently examined, and the variations in precipitation from source to *debouche* are shown in a table convenient for reference. There are tables and diagrams of grouped rainfall statistics, and discussions of the monthly and secular variations in precipitation, and of the local and physical meteorological influences which are not to be neglected in an estimate of the rainfall that may be expected, and that may be made available in any given locality.

The vast current that sweeps across the tropical Atlantic, and impinges upon our Atlantic coast, and from which branches a broad stream that passes up through the Caribbean Sea and circuits through the Gulf of Mexico, yields a flood of warm vapor that is wafted by prevailing winds up the great trough between the Alleghany and Rocky Mountain ridges and over the Atlantic slope of the Appalachian range. An equally vast current sweeps up past the coast of China and the Aleutian islands and impinges upon our Pacific slope, and the prevailing westerly winds waft its floods of accompanying vapors up the westerly slopes of the coast range, the Sierras and the Rocky Mountains, and a portion of this latter vapor, after the passage of the great ridge, is gathered into the storms that sweep down across our northwestern States and the lake region.

The influences of the great ridges upon the flow of vapor from the two great ocean currents is marked, and a study of them leads easily to localization of dependent rain systems of uniform characteristics. And more detailed study leads easily to the special characteristic of precipitation in each of the great river basins. The statistics in the chapter upon rainfall are classified upon the system above indicated, and include reports from stations in nearly all sections of the State and territories, so that the reader will be able to determine from the data given, with close approximation, the mean rainfall in any of the principal river valleys, and by comparison, also, in any lesser valley of the tributaries of the main streams.

The chapter closes with descriptions of some of the great storms that have moved over the Atlantic slope and New England, and with instruction for the selection of pluviometers, and their management, for reliable gaugings of rainfalls.

The elaborate discussion of rainfall forms a fitting introduction to the succeeding chapter upon the *Flow of Streams.* The flow is dependent not only upon the periodic fluctuations in precipitation, but largely upon evaporation and other influences. The causes that produce steady or flashy streams are duly considered, and the periodic divisions of natural flow are classified, and the monthly ratios of flow are developed from statistics of actual measurements. Several tables and formulas to facilitate estimates of minimum, mean and flood flows of streams, are presented.

Next follows a brief chapter upon the artificial *Storage of Water*, and the losses of water by *evaporation, percolation,* and other means, incident to storage. This chapter gives numerous valuable statistics and tables relating to evaporation from water, earth, and vegetation, and their practical effect upon storage.

The succeeding chapter upon the *Supplying Capacity of Water Sheds*, is a practical application of the deductions of the three previous chapters just alluded to. This discussion will prove invaluable to all those who are interested in the gathering of water for power, domestic use, or for irrigation, or who have to guard against the disasters of floods, and sufferings incident to severe drought.

The next two chapters treat upon the *Impurities of Water,* and upon the qualities of *Well, Spring, Lake, and River Supplies.* Our author, recognizing the vital influence of one of the three essentials to human life, *air*, *water*, and *food,* has examined minutely the composition, solutions in, and properties of water, and the physiological effects of its impurities. Its usual mineral and organic impurities, and their sources, whether belonging to the surface, sub-surface, atmospheric, terrestrial, aquatic, animate or vegetal classes, are duly pointed out, so that their presence or absence may usually be predicted without the aid of qualitative analysis. The qualities and deadly effects of agricultural, manufacturing, and sewage impurities, and the effects of impure ice in drinking water, from which much sickness results in cities, are vividly pictured and an energetic protest entered against the admission of such filthy dregs into the drinking waters of towns, and by it into the human circulation. Some valuable hints are given for the selection, test of quality, and preservation of domestic water supplies. A great deal of the information here condensed is invaluable in every household, for its own protection, for it is a lamentable fact that in many towns the subtile poisons of impure water are flowing day by day unsuspected into the life-blood and weakening, especially, the systems of the growing children. A knowledge of these impurities and their sources will lead to their avoidance.

The opening chapter of the second section is devoted to the consideration of the *Weight, Pressure, and Motion of Water,* more especially to those molecular properties that give to water its mobility, its possibility of equal pressure in all directions, and the possibility of the instantaneous conversion of the pressure among the particles into motion of the particles. It considers also the theoretical effects of pressure due to the combined weight of the particles and the theories of the resultant pressures upon plane and upon curvilinear surfaces, and gives useful formulas and tables to facilitate calculations of pressures in reservoirs, chambers, pipes, and upon sluices, or coffer-dams. This chapter is a concise presentation of those philosophical principles applicable to the pressure of water with which the student in hydrodynamics must needs be familiar before he attempts the design of hydraulic structures or machines, and which will make easily comprehensible the theory of flow of water as discussed in the next five chapters relating respectively to the *Flow of Water through Orifices, Flow of Water through Ajutages, Flow of Water through Pipes under Pressure, Flow of Water in Open Channels,* and to *Measuring Weirs and Weir Gauging.*

The second section of the book exhibits by plain language and pictorial illustration the methods by which the constant force of gravity is applied to the determination of the volume of flow of water through orifice, ajutage, weir, pipe, and channel. Gravity acts upon the individual particles as it acts upon a solid, but when the particles fall through an orifice, or through an ajutage or a weir, the volume is modified by the contraction of the jet at the issue, and when they fall, or, as it is usually expressed, flow through a pipe or channel, the constantly accelerating force of gravity is modified by the reactionary resistances from the pipe wall, or channel sides and bottom. In the cases of pipes and channels, the effect of the accelerating force is directly as the ratio of inclination, and the retarding force is directly as the area of reactional surface of pipe or channel, the square of velocity of flow, and inversely as the sectional area of stream, modified by a coefficient. The flow becomes uniform when the accelerating and retarding forces balance each other, and the flow under this condition becomes subject to closely approximate measurement. From the conditions as above stated, it is seen that an equation may be formed for any given case of uniform flow, in which the velocity and the coefficient will be the only unknown quantities. Now if a great variety of experimental measurements of flow in pipes, for instance, with varied velocities and varied sectional areas be collated, and their coefficients determined and systematized by plotting, and thus a full table of coefficients applicable to all common cases be deduced, then a simple equation, having but one unknown quantity, may be constructed for each given case, and that unknown quantity—the velocity, can be easily computed. Tables of coefficients are presented in these chapters, with simple formulas to which they are applicable, and adapted to the several classes of orificed jets, and to streams, and adapted to compute the velocity, the head to which the velocity is due, diameter of jet or pipes, and volume of flow. It is here shown that the series of coefficients applicable to smooth pipes is not applicable to masonry conduits, or to open channels, neither are the plotted curves of the several series parallel. Hence the confusion, when the attempt has been made to make some one of the old standard formulas for flow universally applicable. In the discussion upon the flow of streams, the most approved forms of hydrometers are illustrated, and their methods of use described.

The third section of the book, which includes about one-half the entire work, is devoted entirely to practical engineering construction, and is very fully illustrated with plates and cuts of suggestive type-forms of complete structures, and details. The first practical subjects taken for descriptive discussion are *Reservoir Embankments and Chambers, and Canal Banks.* The author seeking for underlying principles, considers not merely the metallic and cohesive natures of the earths of which he would build an embankment, but also their interstices and the combinations of earth grains that will reduce the voids to the smallest microscopic proportions, and shows clearly why one bank of earth is impervious and stable, and another is constantly liable to be swept away. The proportion of waste-ways, the safety-valves of embankments are more fully discussed than they have heretofore been in any publication, and formulas and tables of their dimensions are given.

The construction of *Waste Weirs* and *Dams* of masonry and timber crib-work is fully exemplified by varied illustration and description. Then follows a chapter devoted to the theoretical proportions and practical construction of masonry partitions and retaining walls to sustain earth or water, and of wharf and pier revetments. The theoretical profiles of retaining walls have been favorable subjects of analysis in technical treatises, and in the class-rooms of technical schools, and favorite subjects for the application of mathematical gymnastics, but the instructive judgments of intelligent, practical mechanics often rebel against the acceptance of the theoretical deductions, because erroneous or unpractical bases have been assumed. The present discussion shows that theory and good mechanical practice are really in harmony, as developed by one familiar with both theory and practical construction.

The next three chapters are devoted to the proportions, construction, and laying of conduits of masonry and mains and distribution pipes of metal, and the valves, hydrants and appendages of distribution systems. Many valuable suggestions evolved from the author's extended practical experience are herein contained, and accompanied by tables that will greatly facilitate the preparation of plans of pipe systems.

The next chapter is devoted to the *Clarification of Water.* The various sediments and impurities which the water brings in suspension or in volution are duly considered, and then the processes of treatment by infiltration, precipitation and filtration are described. The methods of construction of filter-beds and basins are illustrated and described, including instructions for their management and maintenance.

A chapter is devoted to the approved methods of pumping water, the various types of pumps, and prime movers, and to the discussion of the efficiencies of the various parts, such as turbine and boiler, steam cylinders condenser, pump-cylinder, air-vessel, etc., that form the combination of a pumping engine.

The concluding chapter is a brief discussion of the several *Systems of Water Supply,* and includes a review of the methods of gathering and delivering water, the choice of water, and systems of pumping with reservoir reserve and pressure, and with direct pressure, and by either steam or hydraulic power. An appendix is added, giving numerous tables, equivalents, and formulas, of value to hydraulic and mechanical engineers.

The work covers 650 pages, octavo, and contains 32 full-page plates, 190 illustrations, and 130 tables. It has been carefully indexed, both alphabetically and by subject, to facilitate convenient reference to the details of subjects treated upon.

While professedly intended as a manual for water supply engineers, and water commissioners, it is really the most thorough, complete, and systematic elucidation of the elementary principles of hydrology and hydrodynamics, with their application to practical hydraulic constructions and water supply systems that has yet appeared in the English language. The writer's long experience and reputation as a successful designer and constructor of hydraulic works and machinery, should give weight to what he has to say on these subjects, and his exhaustive treatment of his subject will undoubtedly give to this book the position of a standard treatise on hydraulics.

The publisher has not spared expense in the production of the work, and has given it that mechanical and artistic perfection which its merits deserve.

A Manual of the Mechanics of Engineering and of the Construction of Machines, with an Introduction to the Calculus. By Julius Weisbach, Ph. D. Vol. 1. Theoretical Mechanics. Translated from the fourth augmented and improved German edition. By Eckley B. Coxe, M.A. London: Trubner & Co.; 1877. New York: For sale by D. Van Nostrand. Price $10.00.

When a book has reached four editions it may be taken for granted that whatever its merits or demerits may be, it supplies a want, and is appreciated by those for whose use it is intended. The first edition of the work before us was published in March, 1846; the second in May, 1850; the third in July, 1856; and the fourth in May, 1863. The present translation seems to have been finished in December, 1876, in America, and was published in this country in the present year. To a certain class of engineers Weisbach's "Mechanics" has been well known for years; but the rising generation have little acquaintance with it. We venture to hope that this will not much longer be the case, for it is indisputable that the work is one of the most able and complete ever written on the mechanics of engineering. To the non-mathematical reader, however, its appearance is singularly repulsive, as its pages fairly bristle with formulæ. In this it has not a little in common with Rankine's works. On examination, however, it will be found while it is no doubt "tough reading" in some respects, the style is so good and the drawings and diagrams so excellent, that a great deal of valuable information can be obtained from it by anyone possessed of so much mathematical knowledge as will enable him to read a formula. The method of the author is very different from that of Rankine. Instead of contenting himself with giving a formula or a rule, Dr. Weisbach has, in most instances, appended an example to show how the formula is to be applied. Nothing can be devised which is more likely to assist the student than this. It throws light on difficult points; clears equations of letters, and replaces them by figures which always appeal more powerfully to the practical mind; and in not a few instances reduces apparently very difficult problems to a rule of three sum. The method of treatment adopted by our author is, however, essentially mathematical. He saw plainly that a multitude of questions have to be dealt with by engineers which can only be handled readily by the aid of the higher mathematics; but he also discovered that the utility of his first edition was seriously impaired by the fact that large numbers of engineer students knew nothing of the calculus. To get over this difficulty he added to the second edition an introduction to the calculus, which in lucidity and comprehensive grasp of the subject, has never been surpassed. He thus, in a measure, supplies the key by which the storehouse of knowledge contained in his volumes can be thrown open. The book is so rendered complete in itself, and can be read without extraneous aid by any one who can master the first chapters either with or without the assistance of a teacher.

The book is of such great magnitude—containing, as it does, no fewer than 1,112 pages, and 902 woodcuts—that we may be excused from doing more than giving a general idea of the subjects with which it deals. We have first the introduction to the calculus, to which we have just referred. Next comes Section I., which deals with phoronomics, or the purely mathematical theory of motion. The first chapter treats of simple motion, and the second of compound motion. Section II. is devoted to mechanics, or the physical science of motion in general. This also is divided into two chapters. The first deals with the fundamental principles and laws of mechanics, and the second with the mechanics of a material point. Section III. is devoted to the consideration of the statics of rigid bodies. It contains five chapters on the general principles of the statics of rigid bodies, the theory of the centre of gravity, the equilibrium of bodies rigidly fastened and supported, equilibrium in funicular machines, and the resistance of friction and rigidity of cordage. Section IV. deals with the application of statics to the elasticity and strength of bodies. In it are five chapters on compression and shearing, elasticity and strength of flexure or bending, the action of shearing elasticity on the bending and twisting of bodies, on the proof strength of long columns, and on combined elasticity and strength. Section V., on the dynamics of rigid bodies, contains four chapters on the moment of inertia, centrifugal force, the action of gravity, and the theory of impact. Section VI., on the statics of fluids, has four chapters on the eqilibrium and pressure of fluids, the equilibrium of water with other bodies, the molecular action of water, and the equilibrium and pressure of the air. Section VII., on the dynamics of fluids, has nine chapters on the efflux of water from vessels, the contraction of the vein when issuing from an orifice in a thin plate, the flow of water through pipes, the resistance caused by sudden enlargement and contraction in the pipes, the efflux of water under variable pressure, the efflux of air and other fluids, the motion of water in canals, hydrometry, and the impulse and resistance of fluids. In an appendix, we have the theory of oscillation fully considered.

The preceding statement will serve to give an idea of the enormous range of the book, and it will show that scarcely a road which the engineer can travel has been left unprovided with finger-posts by our author.

Before concluding, we must raise our voice to protest against the unwieldy form in which the book has been issued by the publishers. It is in dimensions, just such another volume as Clark's "Rules and Tables," to the size of which we have already taken exception. It weighs no less than 3¼ lbs., and is far too thick to be read with comfort. Such voluminous works would be much more easily used if they were issued in two parts. If this be deemed objectionable, then a larger page should be used, which would give a thinner and handier book. For the rest—the type, printing, and paper, are all good, but we should like to know who is responsible for the grave defect that in numerous instances commas have been used instead of decimal points.—*The Engineer.*

MISCELLANEOUS.

Novelty in Bridge Construction.—A most remarkable bridge, on a new principle, was opened on the Tyne some time ago. It is connected with the Scotswood and Wylam Railway, and crosses the Tyne near Wylam. It is on the bowstring girder principle. The girders are made from wrought iron, and the roadway is suspended from these girders. The bridge has a very light appearance, and there was a general impression that it would collapse when tested, but the severe tests applied to it have shown that the principle of the bridge is good, and also that the material and workmanship are of the highest class. Enormous weights, in the shape of engines, tenders, &c., were placed upon the bridge in various positions, and the deflection was so small that the utmost confidence is now felt in the safety of the structure. These experiments were conducted by the Government Inspector, Mr. Alfred Hannen, Mr. Haswell, and others. The final experiment was as follows—Three engines, coupled, were started from the south end of the bridge, and the same number from the north end, and they were suddenly stopped at the center, and even with this test the deflection and vibration were scarcely visible.

The Turkish Navy.—In addition to the particulars given in a previous issue (No. 211) the following information with regard to the present condition of the Turkish Navy will be interesting at this time. There are at present fifteen ironclads ready for active service, two nearly completed and two building. In addition to these vessels, Hobart Pasha has under his command fourteen monitors for coast and river service, which will be able to do good work in hindering the Russian army from crossing the Danube. Of the fifteen ironclads, seven are frigates and eight corvettes; they are all armed with Armstrong breechloaders. The particulars of the fleet are as follows:—Frigates: Massoudieh, 14 guns, 7200 h.p.; Musreetieh, 12 guns, 800 h.p.; Azizieh, 16 guns, 3050 h.p.; Osmanieh, 16 guns, 3050 h.p.; Orkanieh, 16 guns, 3050 h.p.; Mah-mudieh, 16 guns, 3050 h.p.; Ashar-i-Tefzik, 8 guns, 3568 h.p. Corvettes: Mahaden-Hair, 4 guns, 3250 h.p.; Feth-i-Bulend, 4 guns, 3250 h.p.; Idaladieh, 5 guns, 300 h.p.; Aoni-Illah, 4 guns, 400 h.p; Aschar-Chef-kit, 5 guns, 300 h.p.; Neduh-im-Shefket, 5 guns, 350 h.p; Luft-i-Dchelik, 5 guns, 200 h.p.; Hifs-e-Rhaman, 5 guns, 200 h.p. The crew of each of the frigates has recently been raised to the war strength of 640 men. The corvettes have each about 250. These men are obtained from the Turkish coast provinces, in which naval service is now obligatory. The term of service is seven years, supplemented by five years in the Rediff or Reserve, which is now called out. The total number of seaman is stated at 50,000. The above may be considered a fair statement of the active portion of the Turkish Navy. Though small in numbers, it must be remembered that it is compact. The situation of the Turkish empire is such that this small, well-armed fleet will be able to hold its own in the Black Sea without any chance of the Russian squadron in those waters being able to obtain any reinforcements, as the Dardanelles may be said to hermetically seal the entrance.

In addition to its fighting ships, Turkey has a long list of vessels available for the transport of troops. She possesses three old wooden liners, mounting an aggregate of 254 old-fashioned guns; five wooden frigates and seven corvettes, with some 300 guns, besides twenty-one smaller craft with about eighty guns; all of these are screw-steamers. There are four paddle corvettes, mounting four guns each; three large sailing cruisers, with an aggregate of eight guns; and twenty-two avisos, or dispatch boats, carrying sixty-four guns in all. Of these last, however, a large number are very old, and hardly seaworthy. There are also three Royal yachts of great speed, which might be utilised as dispatch boats, and five old transports mounting two or three guns each. In addition to this, Turkey has twenty-nine large steamers belonging to companies or to private individuals, which would, no doubt, be made use of should Abdul Kerim think fit to carry out the idea of landing a Turkish force in the Caucasus, and calling upon the wild tribes there to revolt against the iron rule of the Czar. The Turkish fleet will have its work to do in the Black Sea, as Russia has spared neither trouble nor expense in its fortifications; but stone walls and torpedoes will be the only enemies it will have to contend with, as the Russian fleet is so manifestly its superior that it will never be able to meet it in fair fight.—*Iron.*

VAN NOSTRAND'S
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ENGINEERING MAGAZINE.

NO. CV.—SEPTEMBER, 1877.—VOL. XVII.

CABLE-MAKING FOR SUSPENSION BRIDGES AS EXEMPLIFIED IN THE EAST RIVER BRIDGE.

By WILHELM HILDENBRAND, C. E.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

II.

4. *The Guidewire.*—The guidewire serves as a guide, parallel to which the single wires of a strand are suspended and adjusted. This is the work of a few minutes, while the regulation of the guidewire requires days and sometimes weeks; but once adjusted it guides all wires in the cable. With the commencement of every new strand it is lengthened an inch or two as a precautionary measure to assure sufficient lengths for the former. For too short a strand there is no remedy, while one longer than necessary is easily shortened in the manner already described.

The cable, which at first hangs parallel with the guidewire, occupies in the finished bridge a considerably lower position, caused by the weight of superstructure, etc. This last position must be determined before hand, and in the case of the East River Bridge depends upon the following considerations:

The law demands that the lowest point of the bridge in the center of the river shall in no case be less than 135 feet above mean high water. An indiscriminate raising of the cable however, would not only increase its tension, but also the grade of floor which already amounts to 3¼ feet in 100′.

The latter difficulty might be overcome by lengthening the suspenders, but this again deprives the bridge of the advantage, obtained by short suspenders, viz., a rigid connection between cable and floor in the center of the span which prevents oscillations.

It follows, therefore, that a strict adherence to the maximum deflection is imperative. This deflection in the finished bridge will be 124.74 feet below saddle plates or 127.64 feet below the point of intersection of the two tangent lines, drawn common to cable and saddle in river and land span. Let us now examine the influences acting on the cable, which tend to change its elevation. They are of various kinds:

1. The weight of superstructure produces an elongation of the wires and hence a sinking of the curve throughout its length.
2. The diagonal stays, relieve the cable in the quarters next to the towers partially of its load, effecting a rise at these places and a depression of the central portion.

3. The inclination of the resultant of pressure towards the river causes a motion of saddles in the same direction, until equilibrium is established between the horizontal tensions of river and land cables. This will produce a further sinking of the cable in the middle span, and a rise in the side spans.

4. The cables are manufactured in a vertical plane, but afterwards they are drawn in an inclined position which causes the vertex of the center curve to rise again.

5. The changes in temperature cause the cable alternately to rise and fall.

Concerning the first point: *Elongation of Wires.*—Extensive tests have been made in regard to the stretch under a certain load. The modulus of elasticity averaged 29000000 which corresponds to an elongation of $\frac{1}{14500}$ of its length per net ton of strain per square inch of section:

2. *Effect of Stays.*—The usual method of calculating the strength of stays consists in attributing a certain equally distributed load to them, and in computing each single one according to the proportionate part of the load, and the angle which its direction forms with the stay. This of course gives only an approximate assurance that the stays really support all the weight allotted to them, and in consequence of this uncertainty some engineers have condemned altogether their application. They, however, neglect the consideration that the principal object of stays is not their supporting power, but the stiffness which they give to the floor. Except by such means, this same stiffness can only be attained by high and heavy trusses, which, though very effective, are considerably more expensive, besides adding greater weight and cost to the cables. For large bridges, therefore, stays are a matter of economy.

The following investigation will show what effect the stays, if supporting a certain load, have on the shape of the cable. Forcing the cable to take the calculated shape, actually will at the same time force the stays to support the assumed load; or reversedly from the shape of the cable the tension of each stay can be computed. Therefore the first objection to the application of stays becomes futile. But there exists a second and more serious one, namely, that in different temperatures they do not work in unison with the cable. This objection is justified in a great measure, and various devices have been proposed to overcome the difficulty. E. W. Serrell (see Report of Board of Consulting Engineers of New York and Long Island Bridge, Feb., 1877) proposes to suspend a lever from the saddles over the tower, and to attach the stays to a series of pivots placed on the line of this lever, so arranged that its movements compensate for the different contractions and expansions of chain and stays. Charles Bender proposes to make suspender and stay in one piece, passing around a roller under the floorbeam. This arrangement is beyond doubt effective. Perhaps an advantageous modification of it might be, instead of making suspender and stay continuous, to attach them separately to a lever turning on a pivot, the arms of which could be so calculated as to confer on stay and suspender its due proportion of load.

The trusses of the East River Bridge are provided with expansion joints at the end of the stay systems, and the resultant motion of floorbeams compensates for elongations and contractions in stays and cable. This, however, is, in the present case, more a fortunate combination of circumstances than an inherent consequence of the arrangement. In all similar cases, it is therefore advisable to examine the movement of certain points of connection of suspender stay and floorbeam, under different temperatures. The point being common to the three parts, its final position must be the result of the motion of each single part. If not, the former equilibrium of forces is disturbed, and one or the other part is overstrained, or performs no work.

3. *Motion of Saddles.*—This takes place theoretically when the horizontal tension of the cable on one side of tower exceeds that on the other. But the friction of rollers counteracts this motion.

All formulas on rolling friction, as given in books, are based upon the supposition that it stands in direct proportion to radius of rollers and to the pressure. But this does not agree with actual experiments, on which alone a certain reliance can be placed. In general, our knowledge of rolling friction is small, and therefore, calculations based on it are more or less liable to errors.

W. Nordling (Mémoire sur les Piles en Charpente Métallique des grand Viaducs), experimenting with rollers of ten centimeters diameter, found the friction for a pressure of 1,000 kilo. to be 3½ kilo. The result of a few experiments made by the author was 4.6 lbs. for 1,000 lbs. with 2 inch rollers and 5 lbs. with 1 inch rollers. Very thorough and valuable experiments were made by Shaler Smith on the friction of drawbridges, (see transactions of the American Society of Civil Engineers, August, 1874). He experimented with eleven different drawbridges, with each from 25 to 70 times, and found as average from six bridges with rollers of 2 feet 8 inches diameter, the friction to be 6.7 lbs. for 1,000 lbs. pressure, and 7.5 lbs. as average of five bridges with rollers of 1′ 6″ diameter. The rollers under the saddles of the East River Bridge are 3½ inches in diameter; the weight upon each saddle is about 1,250 tons. Comparing this case with Shaler Smith's experiments, and assuming that the friction will increase for smaller rollers in the ratio as he found, the friction will be 9½ to 10 lbs. per 1,000 lbs.

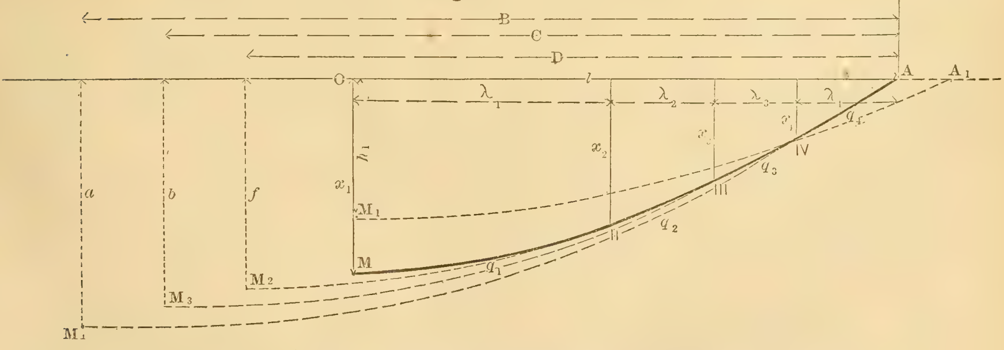
4. *Cradling of Cables.*—Changing the position of cables from a vertical to an inclined plane will produce a negative factor in the causes for depression.

In other words, it will raise the vertex of outer cables 0.455 feet, and that of the inner 0.163 feet. Hence, the cables can be hung this much lower, and the above given deflection of 127.64 can be increased to 128.095 feet.

5. *Changes of Temperature.*—A difference of three degrees Fahrenheit causes the vertex of river cable to rise or to fall one inch. In the following calculation no reference will be taken of changes produced by heat and cold; it is sufficient to carry it out for a certain fixed temperature, and we assume the given deflection to take place at ninety degrees Fahrenheit. Afterwards the guidewire can be corrected according to the temperature of the day in which it is regulated.

The circumstance, that the motion of saddles is not known before hand, or in other words, that the relative span of middle and land cable is not defined, makes a general solution of the problem impossible. But under certain suppositions, the correctness of which afterwards must be proved, we can proceed in two ways. Either we guess the position of guidewire and see whether under all the different influences it will reach a final deflection of 128.095 feet—or else we assume the final position of saddles and work backward, calculating the rise of the cable which would take place if the superstructure were removed. The assumed position of saddles must afterwards be verified, which generally will require two or three trials and repetitions of the calculation. We proceed after the second method and consider therefore the following problem :

Fig. 27



Let AM be half the cable in its final position of the span $2l$ and deflection x_1 loaded symmetrically per lineal foot with q_1, q_2, q_3 and q_4 over the spaces λ_1, λ_2, λ_3 and λ_4. The load q_1 represents the weight of superstructure per lineal foot;

q_2, q_3 and q_4, the same, less supporting power of stays within the spaces $\lambda_2\,\lambda_3\,\lambda_4$. This supporting power is greatest near the tower and diminishes towards the outer stays on account of their greater length and smaller angle. For the sake of simplicity we assume only three variations and consider the load between two points equally distributed. Properly it changes from stay to stay, but the variation is so small that practically it produces no effect and would only make the formulas unnecessarily lengthy and inconvenient.

Under this supposition each piece between points I and II, II and III, etc., is part of a parabola which, if prolonged, will have its vertex in M, M_2, M_3 and M_4, corresponding to the spans l, D, C and B and the deflections x_1, f, b and a. Calling the ordinates of points M, I, II, III and IV in regard to a horizontal line through A: x_1, x_2, x_3, x_4 and the length of curve AM : S, we shall have to determine the following ten unknown parts: S, x_2, x_3, x_4, B, C, D, a, b, f, and need therefore ten equations. The whole system is a suspended polygon in equilibrium, the single parts of it consisting of segments of parabolas. Expressing the length of the whole curve in the adopted symbols will give the first equation. Three more will be obtained in expressing the condition that points II, III and IV are points of certain parabolas. The horizontal force in each suspended polygon is equal throughout; this will give three other equations. Finally we receive three conditions in expressing that the tangents to points II, III and IV are each common to two parabolas. The length of curve AM is given in the following formula:

$$\begin{aligned}
S = {} & B\left\{1+\tfrac{2}{3}\left(\frac{a}{B}\right)^2-\tfrac{2}{5}\left(\frac{a}{B}\right)^4\right\}-(B-\lambda_4) \\
& \left\{1+\tfrac{2}{3}\left(\frac{a-x_4}{B-\lambda_4}\right)^2-\tfrac{2}{5}\left(\frac{a-x_4}{B-\lambda_4}\right)^4\right\} \\
& +(C-\lambda_4)\left\{1+\tfrac{2}{3}\left(\frac{b-x_4}{C-\lambda_4}\right)^2-\tfrac{2}{5}\left(\frac{b-x_4}{C-\lambda_4}\right)^4\right\} \\
& -(C-\lambda_4-\lambda_3)\left\{1+\tfrac{2}{3}\left(\frac{b-x_3}{C-\lambda_4-\lambda_3}\right)^2\right. \\
& \left.\qquad -\tfrac{2}{5}\left(\frac{b-x_3}{C-\lambda_4-\lambda_3}\right)^4\right\} \\
& +(D-\lambda_4-\lambda_3)\left\{1+\tfrac{2}{3}\left(\frac{f-x_3}{D-\lambda_4-\lambda_3}\right)^2\right. \\
& \left.\qquad -\tfrac{2}{5}\left(\frac{f-x_3}{D-\lambda_4-\lambda_3}\right)^4\right\} \\
& -(D-\lambda_4-\lambda_3-\lambda_2)\left\{1+\tfrac{2}{3}\left(\frac{f-x_2}{D-\lambda_4-\lambda_3-\lambda_2}\right)^2\right. \\
& \left.\qquad -\tfrac{2}{5}\left(\frac{f-x_2}{D-\lambda_4-\lambda_3-\lambda_2}\right)^4\right\} \\
& +\lambda_1\left\{1+\tfrac{2}{3}\left(\frac{x_1-x_2}{\lambda_1}\right)^2-\tfrac{2}{5}\left(\frac{x_1-x_2}{\lambda_1}\right)^4\right\} \qquad (1)
\end{aligned}$$

Condition, that points A and IV are points of the same parabola:

$$\frac{B^2}{a}=\frac{(B-\lambda_4)^2}{a-x_4} \quad . \; . \; . \quad (2)$$

The same conditions for points III and II:

$$\frac{(C-\lambda_4)^2}{b-x_4}=\frac{(C-\lambda_4-\lambda_3)^2}{b-x_3} \quad . \quad (3)$$

$$\frac{(D-\lambda_4-\lambda_3)^2}{f-x_3}=\frac{(D-\lambda_4-\lambda_3-\lambda_2)^2}{f-x_2} \quad . \quad (4)$$

Equality of the horizontal force gives the following:

$$\frac{q_4\,B^2}{2a}=\frac{q_3\,(C-\lambda_4)^2}{2(b-x_4)} \quad . \; . \; . \quad (5)$$

$$\frac{q_4\,B^2}{2a}=\frac{q_2\,(D-\lambda_4-\lambda_3)^2}{2\,(f-x_3)} \quad . \quad (6)$$

$$\frac{q_4\,B^2}{2a}=\frac{q_1\,\lambda_1^{\,2}}{2\,(x_1-x_2)} \quad . \; . \; . \quad (7)$$

Tangent in point II is common to parabolas $\overline{M\,II}$ and $\overline{II\;III}$:

$$\frac{\lambda_1}{x_1-x_2}=\frac{D-\lambda_4-\lambda_3-\lambda_2}{f-x_2} \quad . \; . \quad (8)$$

The same condition for points III and IV:

$$\frac{D-\lambda_4-\lambda_3}{f-x_3}=\frac{C-\lambda_4-\lambda_3}{b-x_3} \quad . \; . \quad (9)$$

$$\frac{C-\lambda_4}{b-x_4}=\frac{B-\lambda_4}{a-x_4} \quad . \; . \; . \quad (10)$$

The solution of these ten equations seems at first laborious, but can be simplified much by introducing the following six new equations:

$$\left.\begin{aligned}
&\frac{(B-\lambda_4)^2}{B^2}=n\\
&\frac{q_1}{q_4}\,\frac{\lambda_1{}^2}{B^2}=m\\
&\frac{q_3}{q_4}\,\frac{(C-\lambda_4)^2}{B^2}=p\\
&\frac{q_2}{q_4}\,\frac{(D-\lambda_4-\lambda_3)^2}{B^2}=r\\
&\frac{(D-\lambda_5-\lambda_3-\lambda_2)^2}{(D-\lambda_4-\lambda_3)^2}=s\\
&\frac{(C-\lambda_4-\lambda_3)^2}{(C-\lambda_4)^2}=t
\end{aligned}\right\}\quad(11)$$

From these follows:

$$\left.\begin{aligned}
a-x_4&=na & x_1-x_2&=ma\\
b-x_4&=pa & f-x_3&=ra\\
f-x_2&=rsa & b-x_3&=tpa
\end{aligned}\right\}\quad(12)$$

Substituting equations (12) in (1) the latter obtains the form:

$$\begin{aligned}
&\frac{a^2}{B}-\tfrac{3}{5}\,\frac{a^4}{B^3}-\frac{n^2\,a^2}{B-\lambda_4}+\tfrac{3}{5}\,\frac{n^4\,a^4}{(B-\lambda_4)^3}+\frac{p^2\,a^2}{C-\lambda_4}\\
&-\tfrac{3}{5}\,\frac{p^4\,a^4}{(C-\lambda_4)^3}-\frac{t^2p^2a^2}{(C-\lambda_4-\lambda_3)^3}\\
&\qquad+\tfrac{3}{5}\,\frac{r^2a^2}{(D-\lambda_4-\lambda_3)}-\tfrac{3}{5}\,\frac{r^4a^4}{(D-\lambda_4-\lambda_3)^3}\\
&-\frac{r^2s^2a^2}{(D-\lambda_4-\lambda_3-\lambda_2)}+\tfrac{3}{5}\,\frac{r^4s^4a^4}{(D-\lambda_4-\lambda_3-\lambda_2)^3}\\
&\qquad\qquad+\frac{m^2a^2}{\lambda_1}-\tfrac{3}{5}\,\frac{m^4a^4}{\lambda_1{}^3}\\
&\qquad\qquad=\tfrac{3}{2}\,(S-l)\quad.\quad.\quad.\quad.\quad(1a)
\end{aligned}$$

Calling now:

$$\begin{aligned}
&\tfrac{3}{5}\left\{-\frac{1}{B^3}+\frac{n^4}{(B-\lambda_4)^3}-\frac{p^4}{(C-\lambda_4)^3}\right.\\
&\qquad=\frac{t^4p^4}{(C-\lambda_4-\lambda_3)^3}-\frac{r^4}{(\mathrm{D}-\lambda_4-\lambda_3)^3}\\
&\left.+\frac{r^4s^4}{(D-\lambda_4-\lambda_3-\lambda_2)^3}-\frac{m^4}{\lambda_1{}^3}\right\}=v\quad.\quad(13)
\end{aligned}$$

And:

$$\begin{aligned}
&\left\{\frac{1}{B}-\frac{n^2}{B-\lambda_4}+\frac{p^2}{C-\lambda_4}-\frac{t^2p^2}{(C-\lambda_4-\lambda_3)}\right.\\
&\qquad+\frac{r^2}{(D-\lambda_4-\lambda_3)}-\frac{r^2s^2}{(D-\lambda_4-\lambda_3-\lambda_2)}\\
&\qquad\left.+\frac{m^2}{\lambda_1}\right\}=u\quad.\quad.\quad.\quad.\quad(14)
\end{aligned}$$

And substituting these values in (1*a*) we arrive at:

$$a^4+a^2\,\frac{u}{v}=\frac{3}{2\,v}\,(S-l)\qquad\text{or:}$$

$$a=\sqrt{-\frac{u}{2\,v}\pm\sqrt{\frac{3}{2\,v}\,(S-l)+\left(\frac{u}{2\,v}\right)^2}}\quad(15)$$

A combination of equations (2), (3) to (10) results in:

$$\left.\begin{aligned}
D&=\frac{q_1}{q_2}\lambda_1+\lambda_2+\lambda_3+\lambda_4\\
C&=\frac{q_1}{q_3}\lambda_1+\frac{q_2}{q_3}\lambda_2+\lambda_3+\lambda_4\\
B&=\frac{q_1}{q_4}\lambda_1+\frac{q_2}{q_4}\lambda_2+\frac{q_3}{q_4}\lambda_3+\lambda_4
\end{aligned}\right\}\quad(16)$$

If these values of D, C and B are substituted in equations (11) we find n, m, p, v, s and t, and consequently we have in (12) six simple equations for determining the last six unknown parts. These are: b, f, x_2, x_3, x_4 and a or x_1, according on which preliminary supposition the calculation was based. If originally the deflection of the guide wire was assumed, we know the length S and find (a) from equation (15) then (16) gives the unknown deflection x_1. If, on the other hand, x_1 is known at the beginning, we find (a) from equation (16), and S by substituting its value in (1*a*).

The numerical values in the present case are: $l=799.55'$, $\lambda_1=399.77'$ $\lambda_2=\lambda_3=\lambda_4=133.26'$; $q_1=1212$ lbs. $q_2=1024$ lbs. $q_3=762$ lbs. $q_4=180$ lbs. $x_1=128.095$.

The span $2l$ is equal to the distance between centers of towers plus the distances between these and points of intersection. By the trigonometrical survey the former was found to be 1595.5 feet, and two careful measurements made across the footbridge by two different methods gave for the same distance 1595.3 and 1595.4 feet proving the correctness of each result. In this calculation the figure of 1595.4 was taken as the mean value. The total load to be supported by stays will be 222500 lbs. divided so, that the first set between points A and IV supports 137500 lbs., the second 60,000 and the third 25,000 lbs. These weights deducted from the total weight and reduced to the lineal foot give the above values for q_1 q_2 q_3 q_4.

By substituting these values in equation (1) we find:

$$S=812.467'.$$

Now suppose all weight be removed, then the saddle will move from A to A_1, point M will rise to M_1, and the dotted curve $A_1\ M_1$ (see Fig. 27) will be the position of the guidewire. In order to find its deflection h_1 we must know the length of $A_1\ M_1$. This is equal to AM less the contraction caused by removing from the cable all load, and consequently all tension.

At point A the tension is a maximum, expressed through:

$$q_4\frac{B}{2a}\sqrt{B^2+4a^2}=2892000 \text{ lbs.}$$

and at M a minimum:

$$\frac{q_1\ \lambda_1^{\ 2}}{2(x_1-x_2)}=2794080 \text{ lbs.}$$

The average between these two and the tension in points I, II, and IV, is:

2849451 lbs. = 1424.72 tons.

If q_1 was uniformly distributed over the whole cable, that is, if no stays would help in the support of the load, the deflection x_1 would be 125 feet and the maximum tension at A would be 3237858 lbs.

This compared with the greatest tension above reveals the curious fact, that, while the stays support $\frac{2}{9}$ths, of the total load, the tension in cable is decreased only $\frac{2}{17}$ths.

Calling L the length of cable $A_1\ M_1$, w its area in square inches, T the average tension, in tons, of cable AM, we find L by the formula:

$$L+\frac{L\ T}{14500-w}=812.467$$

$$T=1424.72 \text{ tons } w=133.928\ \square''$$

$$\text{hence } L=811.871$$

If $A_1\ O$, the span of guidewire, be called l_1 we finally find its deflection h_1, through the relation:

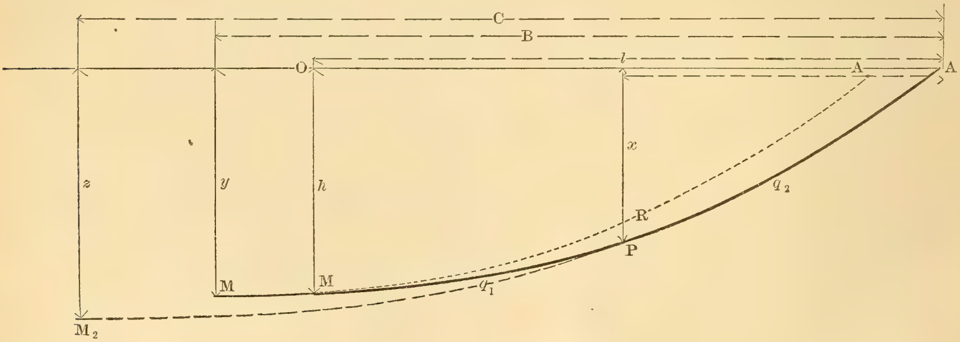
$$l_1\left\{1+\tfrac{2}{3}\left(\frac{l_1}{h_1}\right)^2-\tfrac{2}{5}\left(\frac{l_1}{h_1}\right)^4\right\}=L$$

$$h_1=\sqrt{\tfrac{5}{6}l_1^{\ 2}\pm\sqrt{\tfrac{5}{2}l_1^{\ 4}\left(1-\frac{L}{l_1}\right)+(\tfrac{5}{6}\,l_1^{\ 2})^2}}$$

The distance AA_1 we supposed to be equal to 0.1 foot, hence

$l_1=799.55+0.1=799.65$ and $h_1=121.926$, which determines the deflection of the guidewire in case the assumption of the distance AA_1, will prove correct. This will be the case if with due regard to the friction of rollers, the horizontal tensions of river and land cables are alike. To determine the latter, a calculation similar to the one for middle cable, must be made; but it will be sufficient to consider the load supported by stays as equally distributed. Though this assumption will make a noted difference in the shape of curve, it will change the amount of horizontal tension so little as to be of no practical value, while on the other hand, it will much simplify the formulas. Considering the uncertainty in rolling friction, it is altogether not necessary to be too exact in this calculation.

Fig. 28



In the diagram above, the curve $A\ R\ M$ indicates the land cable, which was adjusted to balance the middle cable before being loaded. If now the weights q_1 and q_2 are suspended from it, A will move to A_1 and the curve will take the position $A_1\ P\ M$, M being a fixed point. Our task is to find a relation between the abscissas and ordinates of the new curve, which will enable the determination of the horizontal tension. The average tension in curve $A_1\ P\ M$ must be equal to the average tension of middle cable, and as we know the length

ARM, we also know the length $A_1 PM$. If M_1 and M_2 are the lowest points of the two parabolas *MP* and PA_1 the above length is also expressed by the equation:

$$S = C\left\{1+\tfrac{2}{3}\left(\frac{z}{C}\right)^2\right\} - (C-\lambda)\left\{1+\tfrac{2}{3}\left(\frac{z-x}{C-\lambda}\right)^2\right\}$$
$$\div (B-\lambda)\left\{1+\tfrac{2}{3}\left(\frac{y-x}{B-\lambda}\right)^2\right\} - (B-l)\left\{1+\tfrac{2}{3}\left(\frac{y-h}{B-l}\right)^2\right\}$$

or:

$$\frac{z^2}{C} - \frac{(z-x)^2}{C-\lambda} + \frac{(y-x)^2}{B-\lambda} - \frac{(y-h)^2}{B-l} = \tfrac{3}{2}(S-l) \qquad (1)$$

In this equation the higher powers of the fractions $\frac{z}{C}$, etc., which are very small, were neglected, as greater accuracy is not necessary. The horizontal forces at A_1 and P must be equal,

$$\frac{q_2 C^2}{z} = q_1 \frac{(B-\lambda)^2}{y-x} \quad . \quad . \quad (2)$$

In point P both parabolas *MP* and PA_1 have a common tangent,

$$\frac{C-\lambda}{z-x} = \frac{B-\lambda}{y-x} \quad . \quad . \quad . \quad (3)$$

To these three equations must be added those of the two parabolas of which the whole curve is composed:

$$\frac{(B-\lambda)^2}{(B-l)^2} = \frac{y-x}{y-h} \quad . \quad . \quad . \quad (4)$$

$$\frac{C^2}{(C-\lambda)^2} = \frac{z}{z-x} \quad . \quad . \quad (5)$$

With these five equations we can determine the five unknown parts B, C, x, y and z.

From (5) and (2) follows:

$$z-x = \frac{z}{C^2}(C-\lambda)^2 \quad . \; . \; . \quad (6)$$

$$y-x = \frac{q_1}{q_2}\frac{z}{C^2}(B-\lambda)^2 \quad . \quad . \quad (7)$$

and from a combination of (7) with (4):

$$y-h = \frac{q_1}{q_2}\frac{z}{C^2} B-l)^2 \quad . \quad . \quad (8)$$

A result of these last equations is:

$$\left.\begin{aligned} B &= \frac{q_2}{q_1}(C-\lambda)+\lambda \\ y &= \frac{z}{C^2}(C-\lambda)(B-C)+z \\ x &= z-\frac{z}{C^2}(C-\lambda)^2 \end{aligned}\right\} \qquad (9)$$

$$z = \frac{C^2 h}{2\,l\,C - 2\,l\,\lambda - \frac{q_1}{q_2}(l-\lambda)^2 + \lambda} \qquad (10)$$

Substituting the values of $z-x$, $y-x$, $y-h$ and B in equation (1) and calling $\frac{3}{2}(s-l) = m$ we arrive after proper reductions, at:

$$z^2\left\{3\,l\,C^2 + C\left[(l-\lambda)^2\left(3-3\frac{q_1}{q_2}\right)-3l^2\right]\right.$$
$$+\lambda^3\left(3\frac{q_1}{q_2}-2\right)+\lambda^2 l\left(3-6\frac{q_1}{q_2}\right)+3\frac{q_1}{q_2}l^2\lambda$$
$$\left.+\left(\frac{q_1}{q_2}\right)^2(l-\lambda)^3\right\} = m\,C^4 \quad . \quad . \quad (11)$$

Several expressions in (11) and (10) are constant, and we set, therefore, as an abbreviation :

$$\left(l-\lambda\right)^2\left(3-3\frac{q_1}{q_2}\right)-3\,l^2 = N$$

$$\lambda^3\left(3\frac{q_1}{q_2}-2\right)+\lambda^2 l\left(3-6\frac{q_1}{q_2}\right)$$
$$+3\frac{q_1}{q_2}l^2\lambda+\left(\frac{q_1}{q_2}\right)^2(l-\lambda)^3 = R$$

$$2l\lambda-\frac{q_1}{q_2}(l-\lambda)^2-\lambda^2 = p.$$

Substituting the value of z in (11), the latter obtains the form:

$$3\,l\,C^2 + CN = \frac{m}{h^2}\left(4l^2C^2-4lCp+p^2\right)-R$$

from which follows:

$$C = -\frac{N+\frac{4\,m}{h^2}lp}{6\,l-\frac{8\,m}{h^2}l^2} \pm \sqrt{\frac{\frac{m\,p^2}{h^2}-R}{3l-\frac{4\,m}{h^2}l^2}+\left\{\frac{N+\frac{4m}{h^2}lp}{6l-\frac{8m}{h^2}l^2}\right\}^2}$$

The value of C once known it is easy by means of equations (9) and (10) to find B, x, y and z.

The numerical values of the constant parts are in our case:

$$l=952.65', \; h=188.3'$$

$$q_1=1212 \text{ lbs. } q_2=748 \text{ lbs. } \lambda=400'$$

which give the following results:

$$C=1282.8 \quad z=222.1,$$

hence horizontal tension:

$$H=q_2\frac{C^2}{2z}=2770592 \text{ lbs.}$$

This is 23488 lbs. less than the one in middle span; but the weight on one saddle is 1250 tons (the total superstructure weighing about 5,000 tons), therefore if equilibrium consists between the two spans, the friction for 1,000 lbs. must be:

$$\frac{23488}{2500}=9.4 \text{ lbs.}$$

This is the same as was found to correspond with the experiments, and, therefore, our supposition, that each saddle moves 0.1′ towards the river, can be considered as correct and also the calculated deflection of guidewire.

In regulating the guidewires for the East River Bridge, it was at first thought sufficient to adjust only the center span, and to rely upon the force of gravity to establish a balancing curve in each land span. But the result showed, that, though the wires were supported on small rollers, the friction was too great to allow a free rendering. A separate adjustment for each span was therefore deemed necessary.

Two guideboards placed on the same level were fastened across the archways in both towers and a level instrument, on a scaffold behind, was so adjusted, that the cross line in the telescope coincided with the upper edges of both boards, while the telescope itself was level. With this arrangement it was possible to sight all four wires from the same position, because a turn of the telescope would not change the height of sight line.

The guideboards were moveable in an iron frame in order to adjust their height according to the temperature. A similar arrangement served for the land spans. A board was fastened to the masonry of each tower forty feet below saddleplate. A tangent line from the upper edge of this board drawn to the land curve, will intersect the face of anchorage at a certain height, which, determined by calculation, will serve to establish a sight line for the regulation.

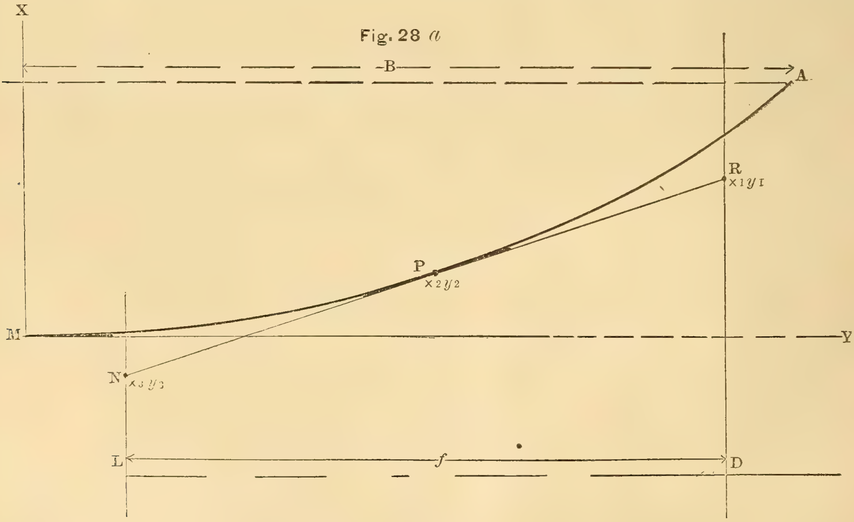


Fig. 28 *a*

APM (see Fig. 28*a*) represents the curve of land cable, *NR* the tangent in point *P*, *NL* the face of anchorage and *RD* that of tower. Taking *M* (the vertex of the curve) as origin of coordinates, *My* as axis of ordinates and *MX* as axis of abscissas and calling the coordinates of *R*, *P* and *N* : x_1y_1, x_2y_2 and x_3y_3 we have first the two linear equations :

$$y_1=ax_1+b$$

$$y_2=ax_2+b$$

from which follows:

$$y_1-y_2=a(x_1-x_2) \qquad (1)$$

x_2y_2 being also a point in the parabola, we have the relation:

$$y_2{}^2=2px_2- \qquad (2)$$

and taking the differential of x_2:

$$\frac{dy_2}{dx_2}=2p$$

$$\frac{dy_2}{dx_2}=\frac{p}{y_2}$$

This is the equation for the tangent to point *P*; but (a) also denotes this tangent in equation (1), hence we have $a=\frac{p}{y_2}$ and (1) obtains the form:

$$y_1-y_2=\frac{p}{y_2}(x_1-x_2)$$

$$y_2=\frac{2px_2}{y_1}+\frac{p}{y_1}(x_1-x_2)$$

The position of point *R* being a fixed one, its coordinates are equal to constants and we can set:

$$y_1=c \quad x_1=d$$

hence

$$y_2=\frac{2px_2}{c}+\frac{p}{c}(d-x_2)$$

$$y_2{}^2=\frac{p^2}{c^2}(d^2+2dx_2+x_2)$$

or

$$2px_2=\frac{p^2}{c^2}d^2+\frac{p^2}{c^2}2dx_2+\frac{p^2}{c^2}x_2{}^2$$

$$x_2=\frac{c^2}{p}-d\pm\sqrt{-d^2+(\frac{c^2}{p}-d)^2}$$

in which $p=\frac{B^2}{2h}$

x_2 being known, the value of y_2 is found through equation (2) and x_3 by the relation: $y_1-y_3=a(x_1-x_3)$ or as $y_1-y_3=f$:

$$x_3=\frac{ad-f}{a}$$

A successful regulation can only be accomplished in perfectly calm weather, and it requires therefore careful watching for sometimes several weeks until a favorable day offers itself. This was the case at the East River Bridge, where for three weeks all attempts of regulating were fruitless. As even a light breeze suffices to displace considerably a wire of such a length, and as the utmost exactness is imperative, the importance of an opportune day is evident. The wires used for guidewires were carefully selected from rings of equal weight per foot. It was necessary to examine about fifty rings in order to find three of equal weight. This shows the importance of the precaution. The regulating commenced on the New York land span and proceeded from there to middle, and, lastly, to the Brooklyn land span. Afterwards the wires were let free and found to be in perfect balance. In other less important cases, where a difference in deflection of a foot or eighteen inches is of no consequence, the calculation may be simplified by neglecting the higher powers of small fractions. Guessing, however, at the effects of stays according to existing bridges may be simple and may apparently also result in a strong and substantial bridge, but, nevertheless, should not be recommended, because it deprives the engineer of the opportunity to calculate exactly the tensions in the different parts of his bridge, the ignorance of which may, if not fatal to the bridge, prove at least a loss of safety or of capital.

VITALITY IN ARCHITECTURE.

From "The Architect."

If the proverb be true—and few will be disposed to deny it—that a live dog is better than a dead lion, it must surely be of the utmost importance in all enterprises of artistic composition, amongst other things, that they be living and not lifeless. In a general way almost everybody who looks at a picture or a statue in the Royal Academy halls is able to recognize vitality; in a building, although not after the same manner, the intelligent observer may none the less clearly perceive the presence of a spirit of design which is quite entitled to be called life, and perhaps in contrast, not far off, another spirit, or want of spirit, which it is no mistake to identify with deadness of soul.

Some authorities affirm that in this singular century of ours architecture is all dead. Chaos, they will go so far as to say, is what prevails, and nothing else. We have no architecture of our own. There is no such thing in being as nineteenth century style. A facade might just as well be on paper as on an edifice; indeed a great deal better if the producer of it on the paper has at his finger tips the vitality of touch which makes a good drawing. To copy is the order of the day. One copies from old Italy, another from old Greece, another from old France or Germany, another perhaps from old England; but everybody copies from somewhere a long way off and something probably long since obsolete. If anyone attempts a device by his own inspiration, he is found to have no inspiration of his own at command, and consequently his efforts end in affectation and delusion, in the creation of all kinds of dead devices and not living. As an especially notable case in point, there is, for example, the Queen Anne mode of architecture now in vogue, a more particularly lifeless affair than almost anything else that has ever been tried except perhaps the style of the Pavilion of King George IV. at Brighton.

But apart from the deadness of our architecture in the mass, it is suggested that our architects in detail are a whole class of very deadly-lively people. Now and then, and here and there, an individual artist makes his appearance with a manifestation in youth of something like vigor; but you have only to wait a little and you find that this is galvanic and not vital. He promises great things. He discountenances his brethren as weak vessels. He arms himself in the newly exhumed harness of some forgotten time or place, and is a nine days' wonder because he is actually a novelty. But when novelty wears off he has after all no more life in him than other people. The entire field of our current architectural art is a valley of dry bones, they say.

All this is obviously very discouraging. The enemies of the architect—and everybody in this world must have his enemies—take up their song against him with infinite gusto when argument of this kind comes on the carpet. His friends are nonplussed. He himself holds his peace for sheer weariness. For indeed the case is so easily made out against him that he knows not what to do to defend himself.

How is it possible, then—and the question has been frequently asked—to breathe into the nostrils of architecture and of architects a little of the breath of life? In other words, in what direction may we look for the chance of being able to commence a new era of design, accepting the principle, as we should certainly have to do, that it must begin at the beginning and probably at a very small beginning? All we want, let it be said, is vitality, but pray let us have that. However rough it may be, however timid it may be, let it be but life and not deadness, and when once started it will go.

Leaving out of account for the moment ecclesiastical architecture, and reserving the question whether it possesses, any more than secular, real vitality rather than galvanic, let us look at the secular alone as that in which the cry for life is most urgent. The prospect is certainly not a cheerful one.

A few years ago the hope of English architecture lay in the endeavor to devise some form or forms of Secular Gothic which should combine the conveniences of modern building with the authentic features and details of mediæval art. On the face of it this was an enterprise which might safely be said not to promise much. The so-called poetic quaintnesses which make up almost the whole character of genuine Gothic, non-ecclesiastical work in France, Germany, or Belgium, must manifestly accord so doubtfully with those prosaic arrangements, as we call them, which constitute the modern essentials, that there would be more than a probability of the prosaic in this case as effectually overwhelming the poetic as in most other cases it does when urged on by essentials to compete with non-essentials. Not only so, but our very modes of construction were quite opposed to the character of mediæval secularity, and, however closely we imitate the masonry, carpentry, ironwork or whatever else of the thirteenth and fourteenth centuries, we soon found that it was only an imitation at the best and not a reality. On the whole, we presume the idea of building Gothic warehouses, banks, and offices, is in London abandoned altogether, and probably in the great provincial cities also, while the town halls alone may for some time longer be made subjects of experiment on the ground that municipality, like ecclesiasticism, has its traditions rooted in the Middle Ages. At any rate it is difficult to point to more than one or two examples of our recent Secular Gothic which are found to command general approbation now; and it is not at all unlikely that even these exceptions, which prove the rule of failure, will in course of a little further time fall into rank with the rest. All this, then, may probably be acknowledged to constitute by no means a successful endeavor to vivify English Architecture. No doubt the Gothic style was both forcible and fashionable, but the force in this development of it was not vital, and the popularity was, as usual, but fleeting.

To go back from the present time just twenty years, some of our readers may remember better than others the very peculiar position in which the genius of architecture was placed by its professors in the great double competition for the Government Offices at Whitehall which was held in 1857. No particular style of design was dictated in that instance, and the architectural world was engaged in the Battle of the Styles in a way which, for such a world, was more earnest than usual. There were to be some twelve or fourteen premiums awarded in the two divisions of the contest, and the judges were what is called a mixed commission, composed of one antiquary, one painter, one engineer, one amateur, and so on, and, as a particular favor, one architect. Half the number of designs in each division, speaking roughly, were Classic, and half Gothic. The course adopted by the mixed commission was eminently characteristic of such a body, and as devoid of vitality as any such thing could be. The best of the designs were simply placed in alternation of style, and the premiums awarded to Classic-Gothic-Classic-Gothic, if we are not mistaken, with the amiable regularity of a see-saw. The absurdity of the idea was apparent enough, but the state of criticism was such that the "impartiality" of the decision was universally applauded, and Lord Stanhope, Mr. Brunel, Mr. David Roberts, Mr. Burn, and their two or three colleagues of the same stamp, were considered indeed to have done a remarkably smart thing. It was perhaps a still more admirable result, as the outcome of the whole transaction, when Sir Gilbert Scott, after many years of controversy, was appointed to be architect of the edifice, on the humorous ground, as stated in Parliament in the interest of Secular Gothic by the ingenious Mr. Beresford Hope, that he had stood second in each division of the competition, and was therefore in a manner ahead of both the men who were first in only one. Lord Palmerston, equally ingenious, remarked that it was the first time he had ever heard the doctrine that the horse which ran always second was entitled to the stakes; but his lordship enjoyed at least this satisfaction in the end—that he was able to compel the architect to abandon his beloved Gothic and build in something like the rival manner.

Secular Gothic, nevertheless, has triumphed since then, and has had a very fair innings. It is now succeeded by the Queen Anne style. Having regard therefore to the demand for vitality, what is this? As we have above hinted, it seem to have come in as dead as a door nail. If we had been writing a few years ago and had said as dead as a Dutch William, the phrase might have passed for a tolerably good architectural joke. The style of the Dutch William and that of Queen Anne are of coure in effect the same thing; the Dutch William, or, red brick house of the age of William the Third, being indeed no more than the advanced development of the mode of his predecessor. Compared with any tolerably well-dressed example of our Secular Gothic, we must really submit that the specimens we see around us of modern Queen Anne are as yet not one whit more spirited, but rather less; and, as every month we wait to see what is to come of the change, the question becomes more important whether any vitality at all is to be manifested in it.

If we are called upon to define this artistic vitality, we shall be quite safe in asserting, in the language of theology, that the quality cannot be defined and needs no defining, just as some of the most physiological of our philosophers are quite unable to define the quality of life in nature at large, and are content to suggest that, as it cannot be allowed to be electricity, it is something for ever occult.

It is not necessarily originality, nor even novelty. Bold eccentricity is an imitation of it, but not the reality. Slap-dash recklessness is as wide of the mark as mere mechanical finish. Even the article of finesse may be but a dead trick; and it need scarcely be said that high education and superabundant knowledge are to all appearance more likely to result in polished inanity than in anything more hopeful. Some authorities consider that at any rate the most likely way to reach it is to cultivate an intelligent and thoughtful *brusquerie*, trusting that such a spirit, being undoubtedly vigorous, may eventually expand, or contract, into vitality. Nor does it seem desirable just now, as the world goes, to discourage this hope. Thought and freedom, with modesty, appear as likely to have good effect as anything could.

No doubt mediæval architecture displays a large amount of unmistakable vitality, and we are willing to maintain that its revival in our own day has been productive of not a little that looks very like genuine life, at least in the best church work. But we fear it cannot be said that in classic architecture even the French, with all their finesse, have reached the same point, while we ourselves are far behind. There are those who hint that it is reserved for the engineer to make the dry bones of classic architecture live, but there is at least no sign for the present how this is to be accomplished.

THE INITIAL STIFFNESS OF THE ST. LOUIS ARCH, ETC.

By HENRY T. EDDY, C. E., Ph. D., University of Cincinnati.

Written for Van Nostrand's Magazine.

In a communication "Concerning the St. Louis Arch," published in the August number of this Magazine, C. Shaler Smith, C.E., has stated that in two particulars my recent discussion of that bridge was erroneous. This distinguished engineer has so complete an acquaintance with the theory and practice of bridge building, that his opinions upon these questions are of great weight; but as I am unable yet to agree with him respecting the points in dispute, I wish in few words to show the substantial truth of the opinions which he claims are erroneous.

In the first place, he asserts that the following statement is "erroneous in the highest degree:" "The St. Louis Arch is wanting in initial stiffness to such an extent, that the weight of a single person is sufficient to cause a considerable tremor over an entire span."

This statement was made to the writer by two credible and competent witnesses who had examined the bridge independently. Now, admitting for the moment that this is an exaggerated statement of the fact, I think we are entitled to assert that the Arch is deficient in initial stiffness, even from Mr. Smith's own testimony; for he states in the same paragraph, that "the maximum effect of all is produced by the passage of a coal wagon loaded to seven tons, jolting over the rough flooring." Now, this is less than two per cent. of the live load which the Arch is designed to sustain, and is a mere nothing compared with the enormous load which the bridge would sustain without injury. It is of no consequence whether the weight be seven tons or less, the fact that any such small weight produces a maximum effect of this kind shows the initial flexibility of the Arch. The cause of this flexibility appears to lie in the fact, that the points of zero bending moment are shifted back and forth for a great distance along the Arch by the jolting of the wagon; and it is not beyond belief that at certain temperatures the same effect may be produced by the weight of a single person jumping up and down at the crown. When, however, a great weight moves upon the bridge, the bending moments immediately become more considerable, and the points of zero bending moment become more sharply defined by the comparatively large moments on either side of it; or, to describe the fact in graphical terms, when the bridge is not loaded, the curve of equilibrium approximates closely to the curve of the arch itself, and a slight additional weight moves their points of intersection long distances; but in case of a large eccentric load the curves diverge widely, and cut each other at a larger angle, while a small additional load changes the configuration very little. This initial flexibility appears to be the necessary condition of any stiff girder subjected to very small bending moments, and the case of the stiffening truss of an ordinary suspension bridge, mentioned by Mr. Smith, is exactly in point. The case of the Suspension Bridge, at Cincinnati, affords a curious confirmation of this theory. The vertical component of any tremor is propogated but a short distance along the bridge, owing to the frictional stiffness of the roadway and truss which quickly destroys waves of this kind, as was explained in my recent discussion of that bridge; but, on the contrary, the horizontal lateral component of the tremor due to jolting of passing loads is noticeable for considerable distances. The flooring and roadway, excepting when a strong wind is blowing against the bridge, are precisely in the condition just spoken of as favorable to the propogation of considerable tremors.

These results show the incorrectness of supposing that "theorizing on this special subject is useless," provided the theory and facts agree. In the second place, exception is taken to the following sentence of my article: "It is the conviction of the writer that the stiff arch rib adopted in the construction of the St. Louis Bridge was a costly mistake, and that, if a metal arch was desirable, a flexible arch rib with stiffening truss was far cheaper and in every way preferable." Now any bridge which costs so much that the income from it does not now, or cannot in the near future, yield a fair return on the capital invested, is "a costly mistake" in proportion to the magnitude of the investment. I am informed that the St. Louis Bridge, costing some twelve or thirteen millions of dollars, was a very bad investment of capital, and I suppose I share the information with a large constituency. I shall be glad to learn that I am mistaken, and that the bridge pays a fair per cent. upon its first cost. Were Chief Engineer Smith now called upon to design the bridge, he would, I have no doubt, recommend a truss instead of an arch, as the feasibility and cheapness of a 520 feet truss is proven by the design adopted for the 520 feet span of the Southern Railway Bridge, at Cincinnati. I had supposed, as above stated, that it was possible to build a flexible arch and straight truss more cheaply than a stiff arch. It seems from the admissions of Mr. Smith, that the cost of manufacturing the parts of the flexible arch is less than that of the braced arch, but that the cost of erecting the former is much the greater of the two, owing to the false works needed in the former case, and not needed in the latter. Now, no one is perhaps more fully informed than Mr. Smith, as to what it is possible to do in the way of erecting bridges without false works, and I should like to ask him if he is willing, in the light of what has now been accomplished in this particular, to adhere to the statement, that "a flexible arch could not" be erected, of the dimensions proposed, without false works. I have not found time to make the necessary computations, but it appears reasonable to suppose that the bracing between the arch and its stiffening truss could, at a very moderate expense, be designed to sustain the stresses it would be subjected to in case the arch were erected without false works.

In conclusion, the St. Louis Bridge is of an unusual type, and its construction was, in some respects, an experiment; it ought, therefore, to receive such discussion and criticism at the hands of those interested in bridges, as to settle as many disputed points as possible. If the bridge is wanting in initial stiffness, the engineering profession ought to be informed of the fact; and, if possible, the real reason of it, according to sound theory, should be also pointed out, in order that the defect may be avoided in future. If the first cost of the bridge was excessive, that too is a mistake which may perhaps not be again committed, especially if it be shown that in spans of this length some other design presents fewer difficulties in an engineering point of view, and is less costly to execute.

THE MANUFACTURE OF GONGS.

By G. W. YAPP.

From "Journal of the Society of Arts."

The effect of hammering renders Chinese gongs extremely brittle, a sharp, sudden blow causing them to fly to pieces like glass. The Chinese, therefore, use them with great care near the outer edge, tapping them gently with a padded stick, and cause the vibrations to increase very gradually until their full sound is evolved. When by accident they are broken, the pieces are collected with great care for the fabrication of new gongs. These pieces are heated to a dull red, and when cooled are easily broken into fragments. The best of these are selected, and, being mixed with the metal scraped off the gongs in the manufacture, are melted in common crucibles like those used in England, each crucible containing 7 lbs. or 8 lbs. of the metal, in a special furnace which holds two crucibles. The fuel used is a kind of short-flamed coal, something like anthracite in appearance. The combustion is conducted with much care, the coal being placed around the crucibles by means of large tongs through a circular aperture in the furnace; and the heat is maintained by means of a simple blower, which consists of a long rectangular box with a wooden piston and clappers.

When the contents of a crucible are melted, the latter is taken from the furnace and weighed, and as soon as it is emptied it is refilled and replaced in the furnace. The temperature is very high, and it is in the upper part of the furnace that the broken metal is heated to redness, as already stated.

When melted, the metal is well stirred in the crucible before being turned out, the oxide and scum being at the same time carefully removed from the surface. It is then poured out into a mould, which consists of a disc of iron lying on a stone slab, and on which is placed a rim of clay to form the edge of the mould. The whole is then greased with oil obtained from oleaginous peas and sprinkled with fine sand; a conical cover of burnt clay is placed on the moist clay ring, and an orifice at the top of the cone is fitted with a funnel, through which the molten metal is poured. The object of all this arrangement is double; it prevents the metal cooling too rapidly in the mould, and it protects the workmen from the spitting of the metal. When the metal is set, but still red, the cover and the clay are removed, and the cake, which is less than half an inch thick, is well scrubbed with a sort of wooden brush on both sides to remove all impurities.

The first hammering is performed while the gong metal is still red, on a piece of cast iron about 6 inches high and 10 inches to twelve inches in diameter, and mounted on a block of wood. The disc is then beaten with a hammer which has a spherical head, and which, having a long and flexible bamboo handle, allows the workman to strike a hard but not dead blow. Two men are employed in this operation, one wielding the hammer and the other turning the metal disc in such a manner that it is gradually rendered concave. When this first hammering is finished, a disc of 14 inches diameter is raised to 2½ inches to 3 inches; the piece is then carried into another shop, where there is a furnace about 4 feet in diameter, heated with charcoal, and furnished, like the former, with a blowing-box. A skilled workman, who is seated by the furnace, removes the disc from the latter at the critical moment to an anvil close at hand, and guides it while it is being hammered. On one side is a cistern of cold water, level with the floor of the shop, and on the other is a simple machine on which the disc is clipped round the edge by a cold chisel. While being heated, the workman turns the partly-formed gong round and round and over and over on the mass of incandescent charcoal, so that the metal may be heated thoroughly throughout; and when on the anvil—which, like the former, is a mass of cast iron—it is directed by the chief workman and beaten by five men with hammers and long handles. A peculiarity in the arrangement is that three of the men hammer continually in regular rotation, while the other two wield large hammers, but beat in unison with the rest. The ability and precision which these men show with their heavy hammers is said to be marvelous. The chief workman stops the operation when the sound of the metal tells him when it is getting cool, places it again in the furnace, and, when sufficiently heated, it is hammered a second time in the same manner. When this is done the gong is nearly of the proper form, and five or six in the same stage of fabrication are then heated together, and afterwards hammered together on the anvil. While this is being performed, all the five strikers aim at the same spot, the firemen turning and directing the work. By this system all the gongs are brought to the same shape and thickness one with the other. But the hammering is continued even after this, the two strikers exchanging their heavy hammers for wooden mallets with flat faces; this hammering is continued for a long time—three-quarters of an hour, says M. Champion, for gongs only 20 inches in diameter. Finally, the gongs are separated, and each one again hammered alone—principally, it would appear, in order to make anv corrections in form—and the edges are carefully pared with a cold chisel. At this stage the gongs are very brittle, clippings being easily broken between the fingers; they are, therefore, heated to dull redness, and plunged for a few seconds in cold water, which is said not to contain any added substances to aid in the tempering.

The gongs are then taken into another shop, where they are scrubbed with a wollen rag and salt water; the water in evaporating leaves a small amount of salt on the metal, and the gong in this condition is again placed in the furnace, turned about in every direction, and again hammered. When the central portion of the gong is finished, the edges alone are heated, in order that any faults may be corrected. During these last operations, in order that the action of the fire may be more regular, and that no heat may be lost unnecessarily, a large sheet-iron cover, suspended to a bamboo handle, is held over the gong while in the fire, and is lifted from time to time to allow the firemen to see and turn the work.

Still the work is not yet completed: the edge of the gong has to be turned up to the proper angle, which operation is described as requiring the greatest ability in the workmen, for a single false blow would cause the metal to crack. The gong is now heated to redness for the last time, and thrown into cold water, where it is left for two or three minutes, when it is taken out and briskly rubbed with a wooden mallet to remove any oxide or foreign matter that may adhere to the surface. The final correction of the edge of the gong is effected by a workman who sits on the ground, and who uses two hammers with short handles, one to strike with and the other as an anvil. When he has completed his work, another man takes the gong, places it on an anvil about 8 inches square in the face, and with a round-faced hammer, weighing about 1 pound, with a short handle, passes over the surface, systematically commencing at the center and proceeding by concentric rings to the outer edge. Sometimes, however, the blows are given in the direction of the radii, but the reason of this is not explained. The blows are vigorous, but the wrist of the workmen must be elastic, as it were, so that the shock shall not last too long; but, with all possible care, the work sometimes fails at this point, and should a crack occur, which the workman knows immediately by the sound, the piece is thrown with the waste metal. The traces of this last series of blows are generally apparent in the finished gongs, although before leaving the factory they are scraped with steel tools, either entirely or partially, the scraping being always effected from the center to the circle indicated.

The composition of these gongs has been found by the analysis of many specimens to be as follows:

Copper	82.00 parts.
Tin	17.00 "
Iron	1.00 "
Nickel	traces. "

The last-named metal can only be discovered by operating upon several grammes of the alloy.

In the manufactory inspected, the men were, on account of the excessive heat, working only during the night. They were paid fixed sums, and were bound to produce a certain number of gongs; the foreman who had the complete direction of the work received one piastre (about 4s. 6d.) per day, and the workman half that sum. The whole of them worked all but naked.

At Pekin and other places in the North of China, gongs may sometimes be seen a yard and even more in diameter; but these are rarely seen in the shops; they are said to be made in Cochin-China. A remarkably fine example was shown in the Japanese section of the Paris Exhibition of 1867; it was suspended, as usual, by means of silk-covered cords, and was struck by means of a piece of wood weighing probably 20 pounds, which was also suspended with one end opposite the center of the gong. The sound of this instrument was superb. The resonance of gongs varies materially, and the Chinese class their tones as male or female; those which have been subjected to the most careful and prolonged hammering produce the male tones.

M. Champion remarks that the Chinese gong-makers have a careless and apathetic air, but the skill, sureness of hand, and vigor which they exhibit in effecting the above long and tedious operations are surprising; their activity and energy is such that it is questionable whether any European workman could conduct such an operation successfully in the same time. The most celebrated place for the production of gongs is Su-tchou, a town remarkable for many manufactures.

The work is not carried on during the hottest months, on account of its laborious nature. The tam-tam is a necessary instrument at all marriages, funerals, public and religious fêtes, in short, in all ceremonies, and even on the occasion of visits of the superior mandarins; the demand for them is consequently enormous, and their production gives employment to a large number of men.

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A DISCOVERY of mineral salt of much interest and importance has, says the London *Times*, recently been made at Aschersleben, in Prussia, in the vicinity of the Hartz Mountains. Within the last twenty years the Governments of Prussia and Anhalt have been deriving large profits from the working of sundry pits or mines productive of potash salts, situated at Stassfurth and Leopoldshall. Hitherto these undertakings have enjoyed a monopoly, but an independent party of explorers, aided by the diamond rock-boring apparatus, have succeeded in reaching the potash deposits at moderate depths not far from Stassfurth. The first boring reached what is called the "kainit" portion of the potash layer, which was proved to have a thickness of fifty English feet. As the Prussian mining law entitles the discoverers to a concession equal to an area of 2,189,000 square meters, it is computed that this discovery includes about 66,000,000 tons of potash salt.

But the explorers, consisting chiefly of English capitalists, have proceeded further, and by means of other borings have obtained the command of an enormous area of these valuable deposits, which are now going to be extensively worked. The discovery is likely to be of great service to chemical industry, by providing an ample supply of one of its staple commodities, the want of which threatened at one time to be rather serious. The extraordinary fertility imparted to the soil by the use of potash manure also renders the discovery a matter of direct interest to the agriculturist. Experience gained in Germany and Holland shows that by the use of the kainit and other forms of potash, land naturally poor can be made to bear extraordinary crops. This system of fertilization has been found peculiarly advantageous in the case of peat lands and moors.

PITCH OF RIVETS AND THICKNESS OF WEB IN RIVETED PLATE GIRDERS.

By THEODORE COOPER, C. E.

Written for Van Nostrand's Magazine.

The calculation of the sectional areas of the flanges of plate girders is readily performed, and generally correct; but when we examine the existing practice as to the pitch of rivets and thickness of webs employed, we find a wide variation. They are too often assumed by guessing or left to the option of the manufacturer. This must be due to a want of comprehension as to the laws governing these points. They are as readily calculated as any other proportions of the girders, governed, of course, as other parts are by our present knowledge of the resistance of the material under the kind of strain acting upon them.

We could prove our formulæ in a general manner, and then descend to particular examples, but we believe most practical men abhor analytical investigations, we will therefore prove our propositions for the commoner practical cases.

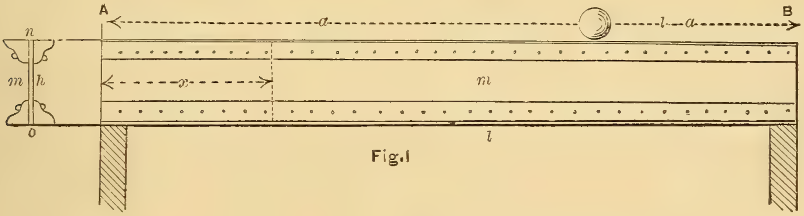

Fig. 1

1st. A simple girder supported at the two ends and carrying a single weight W at any point. The reaction at support A, or proportion of W delivered to that point equals $\frac{W(l-a)}{l}$(1) ; The reaction at B$=\frac{Wa}{l}$(2). The moment at any point x equals $M_x=\frac{W(l-a)x}{l}$(3). The stress produced by this moment upon the flanges at x, is, $F_x=\frac{W(l-a)x}{lh}$(4). That portion of the vertical force W which passes to the support A, or $\frac{W(l-a)}{l}$ is constant for all points between W and A, and is known as the *vertical shearing force* between these points. These are the well known formulæ as usually applied. We will now proceed to develop the stress coming upon the rivets connecting the web with the flanges n and o.

By examination of equation 4 we see that the flange strain varies for each value of x; being nothing at the supports, and increases regularly till it reaches a maximum value at the weight W. This increase in the stress is received from the web, and must be transferred by the rivets connecting the *web* with the *flanges*. The *number of rivets* therefore *between any two flange sections*, must be *sufficient* to *transfer this difference of stress.*

For any point x, $F_x=\frac{W(l-a)x}{lh}=\frac{Vx}{h}$ since V the vertical shearing force at this point, equals $\frac{W(l-a)}{l}$.

For any other point $x-\delta$ $F_{x-\delta}=\frac{V(x-)\delta}{h}$. The difference is the stress upon the rivets in the space δ (which we will call ω) or

$$\omega=\frac{Vx-V(x-\delta)}{h}=\frac{V\delta}{h}(5)$$

Let δ=the distance between two consecutive rivets or the rivet pitch, then the stress upon any rivet equals the $\frac{\text{vertical shear}\times\text{the pitch}}{\text{depth of the girder}}$ or the pitch $\delta=\frac{\omega\times h}{V}$(6), that is, the pitch required is equal to the allowed stress upon one rivet multiplied by the depth of the girder, and divided by the vertical shear at the required point.

For this particular case, the depth and shear being constant, the pitch will be uniform from W to the supports, but different on opposite sides of the weight, unless the weight is at the center of the girder.

2d. A similar girder with a uniform load per foot of *w*.

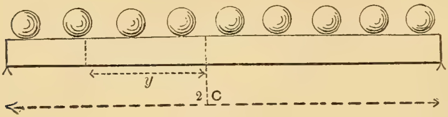

Fig. 2

The vertical shear at any point y, $V_y = wy$.

Flange strain at y, $Fy = \frac{W(c+y)(c-y)}{2h} = \frac{W(c^2-y^2)}{2h}$.

Flange strain at $y+\delta$, $F_y + \delta = \frac{w\,(c^2-(y+\delta)^2)}{2h}$

As before, the difference of these flange strains will be the stress upon the rivets in space δ,

$$\text{or } \omega = \frac{w}{2h}(c^2 - y^2 - c^2 + y^2 + 2y\delta + \delta^2)$$

$$= \frac{w\delta}{h}\left(y + \frac{d}{2}\right) = \frac{V_{y+\frac{\delta}{2}}\,\delta}{h} \qquad (5)$$

which is identically the same as determined in previous case.

3d. A similar girder *fixed at both ends* and carrying a uniform load *w*.

The vertical shear at any point y, $V_y = w_y$

$$\text{Flange strain at } y,\quad F_y = \frac{w(c^2-y^2)}{2} - \frac{12}{32}wc^2$$

Flange strain at $y+\delta$,

$$F_y + \delta = \frac{W(c^2-(y+\delta)^2)}{2} - \frac{12}{32}wc^2$$

$$\text{Hence } \omega = \frac{V_{y+\frac{\delta}{2}}\,\delta}{h} \text{ as before.}$$

We have restricted our investigation to parallel flanges for sake of simplicity, but the same results may be worked out for girders with inclined flanges.

What has been proved for stationary loads will equally apply to all cases of moving loads, the proper value for the vertical shear being determined for each special case.

For moving loads an additional fact must be borne in mind, that as a load passes from one end to the other of a girder the vertical shear is alternately negative and positive for each point, or in other words, the stress upon each rivet under a moving load is first towards one support, and then towards the other. Experiments show that when any piece is subjected alternately to strains in opposite directions, the total effect is more nearly the sum of the two, and for *rivets especially* the *sum* of the *two stresses* should be considered. This can readily be done by taking the sum of the vertical shears, as the total shear at any point. For example, a single concentrated weight W passing from one end to the other of a supported girder. The shear toward the support A is represented graphically by the triangle BA*c*, and towards the support B by the triangle AB*d*. The shear at each end being equal to W, and at the center to $+\frac{W}{2}$ and $-\frac{W}{2}$. By summing the two shears at all points we see it becomes equal to W. All the rivets, therefore, in such a girder should be proportioned for a stress equal to the whole weight.

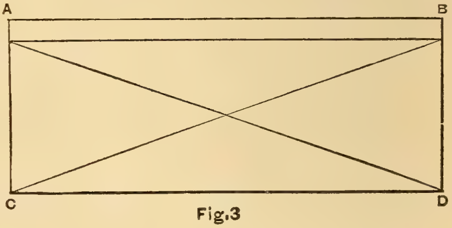

Fig. 3

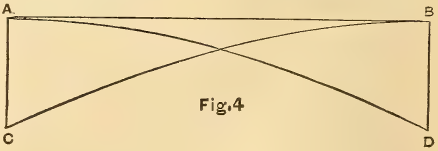

Fig. 4

For a distributed load passing over a girder, the shear is represented by the two parabolas AB*d* and BA*c*. The shear at each end being $\frac{Wl}{2}$ and at the center $\pm\frac{Wl}{8}$.

For such a girder it will be sufficient practically to proportion the rivets at the ends for a shear of $\frac{Wl}{2}$ and at the center for $\frac{Wl}{4}$, or *twice* the *pitch* at the *ends.* The intermediate ones proportionally.

Value of ω. We have represented the stress transferred by each rivet by the symbol ω. This stress tends to shear the rivet, and also to elongate or tear out the rivet holes in the pieces connected by the rivet. (We shall not take into consideration the stress transferred by the friction due to the contraction of the rivet, as it is uncertain in amount and in duration), ω must then, in well proportioned girders, be less than or equal to the safe shearing resistance of the rivet. It must also be equal or less than the safe compressive resistance upon the area of the rivet hole in the thinnest member (web or flange) which is usually the web sheet.

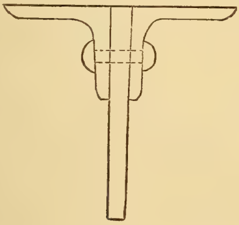

Fig. 5.

We therefore have ω=or<

$$\begin{cases} 6{,}000 \text{ lbs.} \times \text{area of rivet (double or single shear)} & (7) \\ 12{,}000 \text{ lbs.} \times t \times d & \\ \quad t = \text{thickness of web in inches} & \\ \quad d = \text{diameter of rivet in inches} & \end{cases}$$

We have taken the allowed stress for shearing of rivets at 6,000 lbs., this being a fair value for iron which should have 10,000th safe strain under a tensile force, and 12,000 lbs. per □″ as the safe compression upon the rivet hole, this being a proper value judging from the results obtained by the tearing action of pins in pin holes.

Whichever value of ω is the *least* of these two, must be used in each particular case.

		Single Shear.	Double Shear.
ω for Shear of Rivets =	For 1″ Rivets.	4712 lbs.	9424
	$\frac{15}{16}$	4142	8284
	$\frac{7}{8}$	3608	7216
	$\frac{13}{16}$	3111	6222
	$\frac{3}{4}$	2751	5502
	$\frac{11}{16}$	2227	4454
	$\frac{5}{8}$	1841	3682

ω for compression on rivet holes	Thickness of Web.	Diameter of Rivets.						
		$\frac{5}{8}$	$\frac{11}{16}$	$\frac{3}{4}$	$\frac{13}{16}$	$\frac{7}{8}$	$\frac{15}{16}$	1″
	$\frac{1}{4}$	1875	2063	2250	2434	2625	2812	3000
	$\frac{5}{16}$	2344	2578	2812	3045	3281	3515	3750
	$\frac{3}{8}$	2812	3093	3375	3656	3937	4218	4500
	$\frac{7}{16}$	3281	3609	3937	4265	4593	4921	5250
	$\frac{1}{2}$	3750	4126	4500	4868	5250	5624	6000
	$\frac{9}{16}$	4218	4640	5062	5482	5906	6327	6750
	$\frac{5}{8}$	4687	5156	5625	6094	6562	7030	7500
	$\frac{11}{16}$	5156	5672	6188	6703	7219	7734	8250
	$\frac{3}{4}$	5625	6188	6750	7312	7875	8437	9000

By substitution of the least value of ω for the particular thickness of web and diameter of rivet to be employed, in the formula (6).

$$\delta=\frac{\omega\times h}{\mathrm{V}} \qquad (6)$$

taking the pitch and depth in the same unit of measure (inches preferably) we obtain the desired pitch.

THICKNESS OF WEB.

In formula (7) we see the influence of the thickness of the web, and from it can readily determine the *least* value allowed for such thickness. From (7) and (6) we get

$$t=\frac{\omega}{12000\,d}=\frac{\mathrm{V}\delta}{12000\,dh} \qquad (8)$$

all dimensions in inches.

Practically the pitch of rivets (for a single row) should not be less than $4d$. Hence for the case of a *single row of rivets* $\delta=4d$.

$$t=\frac{4\mathrm{V}d}{12000\,dh}=\frac{\mathrm{V}}{3000\,h} \qquad (9)$$

(t and h in inches)

Formula 8 is *general* and *applicable* to all cases. Formula 9 will apply to the more *common cases* where the parts only allow of *one row of rivets.* These formulae give the varying thickness allowed in webs at different points of the girder by substituting the value of V, the vertical shear, at the required points.

Examples.—1st. A load W (stationary) =24000 lbs. at center of a girder with a single row of rivets and pitch$=4d$, using (9) we get

Fig.6

$$\text{Thickness in inches}=\frac{4}{\text{Depth in inches.}}$$

2d. For the same load in motion

$$\text{Thickness in inches}=\frac{8}{\text{Depth in inches.}}$$

3d. For a uniform load Wl,

$\mathrm{V}=\frac{\mathrm{W}l}{2}$ at the end of the girder and (9) becomes

$$t=\frac{\mathrm{W}l\text{ in feet}}{6000\,h\text{ in ins.}}=\frac{\mathrm{W}}{72000}\frac{l}{d} \qquad (10)$$

(l and d being reduced to same unit)

By which we see that for the same unit of load, the thickness of the web depends upon the ratio $\frac{l}{d}$; or for the same ratio of $\frac{l}{d}$ the thickness depends upon the unit of load. We can therefore tabulate these results:

	W=1500	2000	2500	3000	3500	4000
$\frac{l}{d}=8$..........	$t=\frac{1}{6}''$	$\frac{2}{9}$	$\frac{5}{18}$	$\frac{1}{3}$	$\frac{7}{18}$	$\frac{4}{9}$
=10..........	$\frac{5}{24}''$	$\frac{5}{18}$	$\frac{1}{3}$	$\frac{5}{12}$	$\frac{1}{2}$	$\frac{5}{9}$
=12....... ..	$\frac{1}{4}$	$\frac{1}{3}$	$\frac{5}{12}$	$\frac{1}{2}$	$\frac{7}{12}$	$\frac{2}{3}$
=14..........	$\frac{7}{24}$	$\frac{7}{18}$	$\frac{1}{2}$	$\frac{7}{12}$	$\frac{7}{10}$	$\frac{7}{9}$

For the lighter weights, it will be more economical to make the web of the same thickness throughout the girder, restricting the size to $\frac{1}{4}''$ as the thinest web to be used in any case.

For the heavier loads it will become a question of circumstances whether to make the web of one thickness for its whole length, or whether to reduce it towards the center and maintain the angles parallel by means of filling plates —or using wider angles, and increasing the frequency of the rivets by having more than one row of rivets.

The preceding investigation determines only the *minimum* value for the thickness of the web, necessary to resist the compressive strain brought upon it at the rivet holes, by the horizontal stress ω transferred to the flanges. In addition, the web must be able to resist the buckling tendency due to the vertical shear V at any point. We must therefore consider

THE WEB AS A COLUMN.

Let ABCD be a section of the web, AC being a section at any point and V the vertical shear at that point, ω is the horizontal stress for a length δ on the line AB, $\frac{\omega}{\delta}$ the same for a unit of length

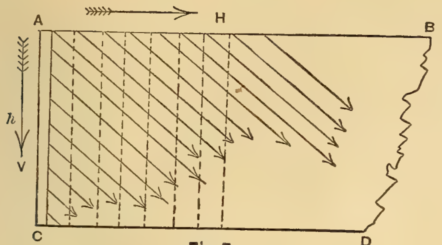

Fig.7

and $H=\frac{h\omega}{\delta}$, will be the amount of this stress for a distance h equal to the depth of the girder. But by equation (6) $\frac{h\omega}{\delta}=V$, whence we see that the horizontal and vertical shears are of equal intensity. We may therefore consider the vertical force acting upon any horizontal unit of the web to be the same as the portion of V acting upon a unit of the surface AC or $\frac{V}{th}$. From equation (9) $\frac{V}{th}=3000$ lbs. Therefore if our web has been determined by formula (9), we must find what depth of girder will safely resist as a column a strain of 3000 lbs. per □″. Using Gordon's formula with $\frac{1}{4}$ of ultimate as a safe strain, we have $3000\text{ lbs.}=\frac{9000}{1+\frac{h^2}{3000t^2}}$, h being depth of girder and t thickness of web. Solving this we obtain $\frac{h}{t}=77$.

We may safely assume then that the web as determined by the formula (9) is able to resist buckling if the depth of the web does not exceed 80 times the thickness.

VERTICAL STIFFNESS.

When the depth of the web exceeds the above limit, it must either be made thicker, or else vertical stiffness must be added to resist the tendency to buckling.

It will also in all cases, be necessary to introduce vertical stiffness at the points of support, and beneath each concentrated load. They must be proportioned as columns and *must contain rivets enough* to transfer or receive the whole shearing force coming upon them, to or from the *web.*

A close fitting against the flanges alone will not be sufficient. This is self evident by an inspection of the forces shown in the section of the web in preceding paragraph.

WEB SPLICES.

When the web is formed of several pieces, these pieces must be spliced strongly enough to transfer all strains coming upon the web; which is V both in the vertical and horizontal directions, for a length of web equal to the depth of the girder. Therefore, the number of rivets on each side of a web, splice must be determined by the formula.

Number of rivets $=\frac{V}{\omega}$ taking the *least* value of ω from the preceding tables.

THE SEWAGE QUESTION.

By C. NORMAN BAZALGETTE, Barrister-at-Law.

From Proceedings of the Institution of Civil Engineers.

II.

A DIFFERENT tone distinguished the statements of the Commissioners in their second Report on the A B C process, dated the 4th of July, 1870, in which (p. 19) they say, "It is no doubt very desirable, in the interest of those towns where sewage cannot be dealt with by irrigation, that an experiment in intermittent downward filtration should be conducted on what may be considered a working scale, when all those difficulties would arise which do not show themselves in a laboratory experiment, and when it would be proved whether the process can be conducted on the drainage water of, say, 20,000 people, with the efficiency to which our laboratory experiments pointed, and without creating a nuisance."

The contrast between this hesitation of 1870 and the confidence of 1871 is sufficiently striking. In 1870 there are difficulties to be considered which do not show themselves in a laboratory; but in 1871 "the laboratory experiments may be considered conclusive." In 1870 some doubt is expressed as to the possible efficiency of the process; but in 1871 it would require only 3 acres to permanently purify the sewage of a population of 10,000 inhabitants. An experiment upon a working scale was obtained at last at Merthyr Tydvil, under the auspices of Mr. Bailey Denton, M. Inst. C.E., and the Commissioners, in their fourth Report, 1872, were able to point to this practical proof of the accuracy of their theoretical conclusions. They say (p. 55) referring to the works at Merthyr, "they were designed by Mr. Denton expressly for the purpose of realising on a large scale the results of that process of intermittent filtration which had been devised and investigated in the laboratory of this Commission." And again (p. 56), referring to the dilution of the effluent by subsoil water, "Nevertheless, and even assuming a very high degree of purity for the subsoil water, the net result of the action of the soil of the intermittent filters upon the sewage was highly satisfactory, especially when it is considered that the sewage of a town of 50,000 inhabitants was thus purified on about 20 acres of land." And again (p. 102), "That intermittent filtration, conducted either on the extensive or intensive scale, is perfectly trustworthy for the abatement of the nuisance thus created, there is now ample testimony to prove." It thus stands upon record that at Merthyr the sewage of a population of 50,000 has been satisfactorily purified upon 20 acres of land, or in the proportion of 2,500 persons to an acre, and that the apprehension formerly expressed as to possible nuisance has been proved to be without foundation.

Never was there a more complete delusion. Never were exaggerated conclusions, based upon laboratory experiments, more completely confuted than by that very experience at Merthyr, which was quoted and relied upon as confirming their truth. Before proceeding to prove the facts incident to the first practical exposition of Downward Filtration at Merthyr, it is curious to observe how deeply the misapprehension concerning them had sunk into the minds of the Commissioners. In the Session of 1872 Dr. Franklin gave evidence before the House of Commons in support of the Birminingham Sewage Bill, and in speaking of Downward Filtration, had his attention called (*Q.* 3769) to the passage already referred to, in which the Commissioners declared their confidence in its permanent efficacy, but at the same time their apprehension that the apparatus on a large scale might give rise to a nuisance, and he then replied, "Yes, at the time that Report was written we had not seen the Merthyr works. They had only been established a few months." So also Mr. W. Hope, V.C., in answer to the late Mr. Horace Lloyd, in the course of his evidence upon the same Bill, gave this explanation of his not having visited the Merthyr works: "*Q.* 2101. I asked if there were anything to be learned there, you would go there?—*A.* If I thought I could learn anything more than I am taught by Dr. Frankland's experiments, I should go there." And again, "*Q.* 2103. But you have never gone to see it?—*A.* Because it tallies exactly with Dr. Frankland's experiments. If the result had not been as good as Dr. Frankland predicted, I should have gone." And the same confidence in the Merthyr fallacy, which distinguished the evidence of Dr. Frankland and Mr. Hope, was equally characteristic of that which was given by the majority of the many eminent experts who supported that Bill, so that when the Bill left Committee the fact stood, founded upon a concurrence of the highest testimony in the kingdom, that the experience of the Merthyr filter beds was identical with the conclusions of the Rivers Pollution Commissioners, and that, the reliability of such experiments having been established, they might safely be accepted as a model for future practice. But the most remarkable incident in the whole history of the Merthyr filter beds is, that Mr. Bailey Denton, at whose instance they were laid down, appears to have participated to some extent in the general misapprehension, and by his public utterances to have assisted in its dissemination, although it must be at once admitted that they exhibit a large amount of contradictory statement. For example, in a Paper by him in the Journal of the Society of Arts, December 8th, 1871, it is stated (p. 65), "Short as the interval has been since intermittent downward filtration was first suggested by the Rivers Pollution Commissioners, that process has, like irrigation, undergone a change..... The change to which I have referred has arisen on the proof which I have had the satisfaction myself of affording, that vegetation may be grown upon the surface of filtering areas, even when receiving sewage equal to the discharged refuse of 3,000 persons to each acre, thus adding, in the most apposite manner, to the cleansing powers of the soil the skavangering properties of vegetation." And again (p. 66), "Having dwelt upon the practice of irrigation, I ought now to explain the process of intermittent filtration as it may be carried into practice, but as I shall presently deal with it when considering 'Land as a purifier of sewage,' I will only state that by adopting the process as technically described, the liquid refuse of from 1,000 to 3,000 persons—and probably more—may be cleansed by the soil of a single acre of land." On what authority does Mr. Denton state that a proportion of 3,000 persons to an acre may be adopted? Is it on the faith of the Rivers Pollution laboratory experiments, or of the experience of Merthyr? Surely not the latter, for (p. 68) he puts the population contributing sewage to the filter beds at Merthyr at 30,000, which, it will be presently shown, is an excessive estimate, and the acreage at 20 acres, which would only justify a proportion of 1,500 to an acre, or less by one-half than the proportion previously suggested.

The only way in which this apparent inconsistency can be explained, is by supposing that when speaking of the proportion of 3,000 persons to an acre, Mr. Denton alludes merely to the temporary effect of such a proportion; but that when he has in view the permanent purification of the sewage, the proportion must be reduced to 1,000 persons to an acre. In a Report made by him in 1873, suggesting Intermittent Downward Filtration as the best means of disposing of the sewage of Kew, Mortlake, and Barnes he says (p. 54), "The quantity of land, actually required for the purification of sewage of average foulness by intermittent filtration, may be calculated at 1 acre for every 1,000 persons if it be drained 2 yards deep, so as to secure 2 cubic yards of filtering material for every square yard of surface. The Rivers Pollution Commissioners deduced from their laboratory experiments that 1 acre of suitable land drained 2 yards deep might suffice for 3,300 people, and in increasing the quantity of land to 1 acre for 1,000 persons, as I propose to do, the object is to avoid any doubt as to permanency of effect."

When Mr. Bailey Denton gave evidence in support of this scheme before Major Tulloch, R.E., Assoc. Inst. C.E., the Local Government Inspector, who held the usual district inquiry preliminary to the grant of a provisional order, he at once admitted that his supervision of the Merthyr works had been limited to five or six occasional visits, and expressed himself unable to speak definitely as to the extent to which the practice of Downward Filtration had been carried there. Under these circumstances, and for the purpose of arriving at an accurate knowledge of the Merthyr facts, it was considered advisable to procure the attendance of Mr. Harpur, Assoc. Inst, C.E., the Resident Surveyor, under whose immediate superintendence Downward Filtration had been adopted, and had continued in operation from the time of its introduction in January 1871 until the time of the inquiry in April 1874. His evidence is not a little remarkable, considering that it was actually given as an advocate of Intermittent Downward Filtration, objecting to a proposal which would have this effect, to use his own words, "that the valuable process of Downward Intermittent Filtration would be put off very seriously throughout the country."

The following evidence extracted from the short-hand notes of the proceedings, shows the actual facts as to Merthyr, and the true conclusions to be drawn from them, as defining the capacity of Downward Filtration to deal with sewage:—

On p. 57 of the Notes Mr. Harpur is asked: "*Q*. Then we may take it that it was only for five months that the sewage of 25,000 people was put upon 20 acres at Merthyr Tydvil?—*A*. That is the fact; the whole of the sewage. *Q*. And that is the whole of your experience in reference to the power of 20 acres to receive and deal with satisfactorily the whole of the sewage of the 25,000 people?—*A*. Just so. *Q*. Then in taking 50,000 the Rivers Pollution Commission was in error?—*A*. There was not the sewage of 50,000; nothing like it. *Q*. Then the deductions drawn by the Commissioners in their Report, based upon the sewage of 50,000, must therefore be erroneous?—*A*. Yes; they must be erroneous."

Again, p. 69: "*Q*. What do you consider is the population the sewage from which might be safely drained or turned upon the 20 acres at Merthyr Tydvil?—*A*. I have given that a great deal of consideration, and my opinion is that those 20 acres would take the sewage and the sewage of 12,000 in perpetuity. I do not think they would cleanse the sewage of more than 12,000. *Q*. That is the result of two years' experience and careful watching?—*A*. Yes, it is." So that so far these significant facts are distinctly proved by the Resident Surveyor at Merthyr: (1) That for a short period of five months only the filter beds were exposed to their greatest strain, sewage being applied in the proportion of 1,250 persons to an acre. (2) That the opinion based upon two years' experience is, that if permanent purification is sought, the proportion must not exceed 600 persons to the acre. Before directing attention to the marked contrast between this evidence and the statements of the Commissioners, it will be well to establish its conclusiveness by carrying it a stage further. After stating that he would only recommend Downward Filtration where sufficient land cannot be obtained for irrigation, Mr. Harpur is asked (p. 60): "*Q*. Under such circumstances as those at Merthyr Tydvil, where the land is exceptionally favourable, you would not put more than the 12,000 to the 20 acres?—*A*. No; I do not think it would be safe generally to place more sewage than that of 500 on an acre. *Q*. Not more than 500 persons to an acre is the result of your experience?—*A*. Yes; it is so."

How far, then, are the experiments of the Rivers Pollution Commissioners substantiated, or their statements justified, by the experience of Merthyr? If Mr. Harpur is accurate in his facts and opinions, their statements are erroneous and their experiments fallacious. Instead of the capacity of the land to receive sewage being continuously tested by the application of the sewage of 2,500 to the acre, the greatest temporary strain to which it was ever exposed was in the proportion of 1,250 persons to the acre. Instead of their opinions that the sewage of from 2,000 to 3,300 persons might be cleansed on a single acre of land, Mr. Harpur states that 600 persons to the acre, in his opinion, represents the capacity of the filters. The issue between these opinions is important, and inasmuch as Downward Filtration is by way of marking a new era in the much-vexed question of the disposal of sewage upon land, it is essential that its capacity should be accurately defined by the experience obtained from its practical operation. The truth can only be ascertained and error refuted by a survey of the evidence which has been supplied by such experience. In order to place the Merthyr facts beyond the range of discussion, it is necessary to examine the evidence relating to them in somewhat tedious detail. Mr. Arnold Taylor, one of the Inspectors of the Local Government Board, having visited the works at Merthyr, presented a Report, dated November 8th, 1872, which may be taken as conclusive up to the time when that Report was made. The following are the most important extracts: "The population of the district draining into the river Taff is estimated at 40,000, of whom about 20,000 are living in houses directly connected with the existing sewers; the other 20,000, having cesspool or surface drainage only, or perhaps no drainage at all, contribute but little to the existing volume of sewage. This should be borne in mind, because I see it constantly stated in the newspapers and elsewhere, that the sewage of the population of Merthyr—*i.e.* 40,000 people—is being successfully purified upon 20 acres of land at Troed-y-rhiew. The fact is, that for some four or five months at most, viz. from January to May 1871, the sewage of 20,000 was run on to the 20 acres of filtration area. But after that time, and to the present moment, one-half only of this sewage of 20,000 people has gone to the 20 acres, the other half having, since June 1871, been continuously applied to the 54 acres of adjoining land under ordinary irrigation.

"I do not wish to be misunderstood. The filtration process at Merthyr must be regarded by every one who visits the place as a very great success, and Mr. Denton is justly entitled to the credit of having first applied on a large scale a system which Dr. Frankland had previously worked out in a series of careful laboratory experiments.

"But just because it is so successful, does it seem to me of importance that the exact truth should be stated with respect to earth filtration as a means of sewage purification.

"The exact history of the Merthyr case will be thus: That for some five months, say from January to June 1871, 20 acres of land, most exceptionally situated and very carefully prepared, were found sufficient to purify a volume of sewage from 20,000 persons, equal to a daily discharge of say 900,000 gallons, or at the rate of 45,000 gallons per acre per day. That for the next eighteen months, the same 20 acres are found capable of purifying half this quantity, the rest being distributed and purified over the 54 acres of irrigated area.

"When I saw the land at Troed-y-rhiew last August my impression was this: That the 20 acres of filtration area did show signs here and there of saturation, and that though the appearance of the surface and of the crops grown upon it was wonderful, regard being had to the quantity of sewage which had been poured upon so small an area, yet that the appearance of the crops and the surface of the 54 acres of the adjoining irrigated farm was in all respects the better of the two.

"It is stated by Mr. Harpur that he has dug trial holes at various parts of the filtration area, and that in none of them has he found any sign of earth saturation or of offensiveness. Hence it may be argued that by a continuance of the present intermittent system of filtration, one 5-acre area being in use at a time, whilst the other three are at rest, the land is capable of being used for an indefinite period of time.

"This may be so, but what I urge is, that the 20 acres of earth filters at Merthyr purified the sewage of 20,000 people for at most five or six months; that since then they have purified the sewage of 10,000 people, and that by the end of this year or the early part of next they may have ceased altogether to act as continuous earth filters.

"The Merthyr system may therefore be certainly quoted as an example of successful sewage purification, but the following exceptional favorable conditions and circumstances ought at the same time to be taken into account:

"First, then, at Troed-y-rhiew, where the filtration areas are situated, there is no population which is likely to be at all sensitive on the subject of nuisance from any offensive smells or emanations.

"Second. The surface soil is light, and of a kind the best suited for active filtration.

"Third. The subsoil, a deep bed of open, porous, water-charged gravel, not only acts most favorably as an open filter, but also gives to the sewage liquid, after it has passed through 6 feet of earth, some three or four times its own bulk of pure water.

"These are favoring circumstances which cannot easily be found in combination. They are found at Troed-y-rhiew, and hence, in my opinion, the main cause of the success of the system of intermittent earth filtration as applied to the purification of the Merthyr sewage. But I believe that this success will be even more marked when the larger area for irrigation purposes is completed, for then, if earth filtration be practiced at all, it will only be resorted to on an emergency or in wet or winter weather."

How do the statements of the Rivers Pollution Commissioners stand verified by this evidence? What becomes of the theoretical proportion of 3,300 persons to an acre? or of the illustration afforded by the experience of Merthyr, where, to quote their words, "the sewage of a town of 50,000 inhabitants was then pumped on about 20 acres of land," or in the proportion of 2,500 persons to an acre? Compare this statement with the facts, that, under circumstances so favorable as to be rarely expected in combination, a proportion of 1,000 persons to an acre, according to Mr. Arnold Taylor, was maintained for about five months, but was subsequently reduced to 500 persons to an acre, and so the filters were subjected to a strain somewhat less severe than the ordinary irrigation farm at Bury, where the permanent proportion observed is 577 persons to an acre, and a far less strain than is imposed upon the Eton sewage farm. This proves that irrigation can, upon the principle contended for by the Rivers Pollution Commissioners, namely, cubical capacity of aerated soil, or soil cleared of subsoil water, outdo Intermittent Downward Filtration in its purifying power; in other words, irrigation is, in some cases, naturally more intensive in its operation than a system of Downward Filtration, for the simple reason that, in some irrigation farms, the subsoil water lies naturally at a lower depth than in a filter bed, where it has been artificially lowered to a depth of 6 feet. With a view of ascertaining the degree of purity of the effluent at Merthyr, various chemical tests were applied. If it be true that deterioration was actually taking place when the proportion of only 500 or 600 persons to the acre was observed, it would seem to be somewhat doubtful whether 1 acre of filter bed drained 6 feet deep could effectually purify even that limited population with permanency of effect; or, to put the case somewhat differently, it would be questionable whether a 6-feet depth of drainage gives a sufficient cubical capacity of oxygen-charged soil to effect permanent purification in the proportion of 500 persons to an acre. This would seem to be the fact, for the results of the experiments at Merthyr in 1871 and 1872, by the Rivers Pollution Commission and the Committee of the British Association, on the utilization of sewage, clearly indicate that such a gradual deterioration was taking place in the quality of the effluent. The following table, based on the experiments of the Rivers Pollution Commission, exhibits the decrease in the percentage of impurity removed from the sewage by the action of the filters in the interval between the months of June and October 1871 :

	In June, 1871.			In October, 1871.		
	Quantity contained in Sewage.	Quantity contained in Effluent.	Percentage removed.	Quantity contained in Sewage.	Quantity contained in Effluent.	Percentage removed.
Total solid mattor.........	54.000	34.600	35.97	49.200	33.480	31.95
Organic carbon............	2.788	0.249	91.07	1.282	0.323	74.81
Organic nitrogen..........	0.783	0.056	92.85	0.952	0.107	88.76
Ammonia....	4.854	0.075	98.46	1.280	0.058	95.47
Total combined nitrogen...	4.780	0.349	92.70	2.058	0.455	77.89

A comparison of the two series of experiments made by the Committee of the British Association gives a similar result :

In January, 1872, the quantity of organic nitrogen removed was 96.43 per cent.
In July, 1872, the quantity of organic nitrogen removed was 86.55 per cent.

In the above comparisons the effect of the subsoil water in diluting the sewage is not taken into account.

The Committee of the British Association, in their Report of 1872, p. 151, thus summarise the results of their experiments : "The effluent water this summer was not quite so pure as last winter, but still four-fifths of its nitrogen was in the form of nitrates and nitrites."

It will have been observed that the most recent evidencc referred to, as to the operation of the Merthyr filters, has been that of Mr. Harpur in the spring of 1874. There had then been three years' experience of their action, which might have been considered sufficient to form a judgment as to their effect. And yet, in a Paper, submitted to the Conference at the Society of Arts in 1876, Mr. Denton gives color and credence to what might have been justly looked upon as the somewhat faded pretensions of Downward Filtration. First of all, he makes the following statement (p. 23) :—"We have now had sufficient trial of actual works to assure us, first, that the suggestion which Dr. Frankland based upon his laboratory experiments, to the effect that an acre of suitable soil drained 6 feet deep, and used intermittently, would cleanse the sewage of 3,300 people, can be realised." What is the "trial of actual works" alluded to as proving the practicability of cleansing sewage with such a proportion of population to soil? The specific experience is referred to in the following statement, but it must be observed that the difference between temporary and permanent effect is not always clearly expressed. It is this (p. 24): "Although experience at Merthyr, Kendal, Walton Convalescent Hospital, and other places, has most conclusively shown that 1 acre of suitable land will efficiently and for a constancy cleanse the liquid refuse of 1,000 people, a fact, the importance of which cannot be over-estimated in those cases where land is very difficult to get and very expensive when obtained, I may be pardoned for stating that I do not wish to be considered the advocate of dealing with the sewage in such a concentrated form under all circumstances." To these allegations the Author makes the following distinct and unqualified reply:

1. That experience at Merthyr has never proved that the sewage of 3,300 persons could even be cleansed temporarily upon an acre of land, the greatest proportion which ever obtained there being from 1,000 to 1,250 persons to an acre (*vide* the evidence of Mr. Harpur and Mr. Arnold Taylor).

2. That experience at Merthyr has never conclusively shown that 1 acre of suitable land will efficiently and for a constancy cleanse the liquid refuse of 1,000 people; but it has conclusively shown that if permanency of effect be required, the proportion, where the filter beds have a 6-feet depth of drainage, must not exceed 500 persons to the acre.

3. That the case of Walton Convalescent Hospital has never conclusively shown that 1 acre of suitable land will efficiently and for a constancy cleanse the liquid refuse of 1,000 people, for the simple reason that the proportion there has never exceeded 400 persons to an acre, as is proved by the following extract from the short-hand notes of the inquiry already referred to, when, in answer to questions, Mr. Denton stated as follows:—"*Q.* Have you ever carried the laboratory experiments of Dr. Frankland into anything like practical effect? —*A.* The Merthyr Tydvil works are nearly three years old, and the works at the Convalescent Hospital at Walton have been in operation four years; once in twenty-four hours the sewage is intermittently applied there. *Q.* What is the population per acre there?—*A.* It varies from 100 to 400. I am not instancing this as a proof that Dr. Frankland's statement is correct." The only further comment to be made upon this evidence of 1874 is this, if the Walton Convalescent Hospital was not cited as substantiating Dr. Frankland's statements, why is it quoted in support of them in the Paper of 1876?

4. That Downward Filtration as practised at Kendal has never conclusively shown that 1 acre of suitable land will efficiently and for a constancy cleanse the liquid refuse of 1,000 people. The facts are given by Mr. Denton, in the Journal of the Society of Arts already referred to, thus (p. 25):—"At Kendal, where there is a population of 13,500, the intermittent downward filtration work has hitherto been confined to about 5 acres of land, contrary to the advice which I gave the Corporation when first consulted, to the effect that 15 acres should be prepared. The experience gained on these 5 acres surpasses all expectation. It confirms, nevertheless, the opinion already stated, that the proper proportion of sewage to land should be that of 1,000 persons to an acre. In this view it appears that the Corporation now concur, for the sewage is not always confined to the 5 acres, but is occasionally applied also to the remaining 10 acres which I had proposed should be prepared for filtration."

Does this mean that the sewage of 13,500 persons has been applied to 5 acres of filter beds, or at the rate of 2,700 persons to an acre? or has no distinction been drawn between population and population directly contributing sewage? To this vague generalization of statement the origin of the Merthyr misapprehension may be traced. Referring to the return furnished from Kendal to the Society of Arts, it appears that, for this population of 13,500 persons, there are only 500 closets in use, so that it is erroneous to speak of the sewage of a population of 13,500 persons draining upon the 5 or 5½ acres of filtration area. What the sewage-contributing population proper is, it is difficult to estimate, but taking the above limited number of closets, coupled with the fact that middens are principally in use, the sewage probably is not derived from more than one-third of the population. Adopting this view, the sewage of a population of 4,500 would have been dealt with upon a surface area of filter bed of 5½ acres (for 5½ acres, and not 5 acres, as appears from a letter from the Borough Surveyor of Kendal to the Author, dated November 13th, 1876, are the accurate dimensions), which yields a proportion of rather more than 800 persons to the acre. This is, of course, to some extent speculative, in the absence of positive information. But before proceeding to conclusions there are several important elements for consideration. First of all, the sewage is rendered extremely dilute by the enormous infiltration of subsoil water into the sewers; secondly, the depth at which the subsoil water in the filter beds stands below the surface-level is not stated, so that it is impossible to argue upon the capacity of Downward Filtration as practiced at Kendal, inasmuch as the whole principle is based upon the cubical capacity which a 2-yards depth of drainage would afford; thirdly, according to Mr. Denton, and also from information supplied by the Borough Surveyor, the whole 15 acres will soon be required for the purposes of filtration, the present limited surface showing a tendency to become clogged. Then, if the Author is right in the assumption that one-third of the sewage proper passes to the filters there, the history of the operation of Downward Filtration at Kendal is this: That for a time the dilute sewage of the town has been applied to the filter beds in the proportion of about 800 persons to an acre; that such filters have shown symptoms of clogging; and it is, therefore, intended to reduce the proportion from 800 persons to an acre to 300 persons to an acre. If this statement of the Kendal experience be an accurate one (and the Author need hardly say that, as he proceeds upon assumption in this case, and is not dealing with facts proven, he is open to correction), there is nothing in any of the cases cited to establish either the soundness of Dr. Frankland's conclusions or the accuracy of Mr. Denton's statements. Merthyr clearly fails at the touchstone of inquiry, Walton fails on Mr. Denton's own evidence, Kendal it is submitted fails on account of the facts stated, and by obvious parity of reasoning from the circumstances at Merthyr.

Surely it is time that the Merthyr fallacy was stamped out, and yet the advocates of Downward Filtration seem unwilling that it should die. The following extracts from a recent correspondence will, however, it is hoped, supply it with a suitable apotheosis. Mr. Denton commences his Paper by introducing (p. 25) a letter from himself, dated May 11th, 1875, addressed to Mr. Jones, the Chairman of the Merthyr Local Board, which, will "satisfy the most sceptical that the value of intermittent downward filtration is as great in combination with surface irrigation on a wide area as when adopted by itself on a small one." In this letter, after expressing apprehension lest the extension of the irrigation area should be interpreted as meaning the abandonment of Downward Filtration, he thus concludes: "Will you kindly say if your Board has changed its mind as to the retention of the filtration areas as part of its permanent works, and if so, what has led to such change?" And again, "I am glad to find that Mr. Dyke (the Medical Officer of the district) has the same confidence in the permanency of their action as I have. In his letter to me of the 6th of May last, he says: "There is still no evidence of any clogging or over-saturation of the beds." In his reply (also set out in the same Paper) Mr. Jones states that the Board contemplates no abandonment of the filtration areas, but looks upon them as a "safety valve." Then the Paper concludes, so far as Merthyr is concerned, with the following observations (p. 25): "If anything more than this is needed to prove the thorough success of the intermittent downward filtration work at Merthyr, I cannot do better than point to the following passages of a letter from Mr. Dyke, the medical officer of the district, who wrote me as lately as July last in the following terms." The gist of Mr. Dyke's letter was to the effect that filtration was perfect, the effluent pure, and the vegetables grown on the filters had been eaten without producing diarrhœa; and then the important statement follows "that there has never been any sign of clogging or over-saturation." Can anything more be needed to prove the thorough success of the Intermittent Downward Filtration work at Merthyr? Only this letter from Mr. Harpur, which states how this success has been achieved. It is addressed to the Author, is dated 8th November, 1876, and runs thus:

"The total area of land acquired by the Board for sewage purposes, including roads, rivers, unsuitable and at present unformed land, is 334 acres.

"The sewage is now being applied to 210 acres, including the 20 acres of filtration areas. Of the land acquired about 90 acres have yet to be formed for the reception of sewage.

"For the last four years no more sewage has been applied to each acre of the filtration areas than to an acre of the irrigated ground.

"The population draining to the sewage farms is 48,000. Each acre of both the irrigation land and the filtration areas receives the sewage of 229 persons.

"The filtration areas do not show any sign of deterioration.

"It is intended to use them permanently for irrigation."

Can anything more be needed "to prove the thorough success of the Intermittent Downward Filtration work at Merthyr" than the facts:

(1) That for the last four years the filter beds have never received the sewage of more than 229 persons to the acre, and yet up to the present time "there is no evidence of any clogging or over-saturation of the beds."

(2) That the filter beds are to be preserved—for what? to perform *en permanence* the functions of ordinary irrigation.

Conclusions.—1. Intermittent Downward Filtration is merely an artificial production of intensified irrigation, and is a useful process to adopt, where land for irrigation cannot reasonably be acquired, or where the necessary depth of soil free from subsoil water cannot be obtained without recourse to artificial drainage.

2. That there is no evidence to show that a larger proportion than the sewage of 500 persons can be permanently purified upon an acre drained 6 feet deep, though there is positive evidence to show that it must not be exceeded, and that this proportion therefore represents the capacity of downward filtration over and through such area and depth of soil.

3. That Dr. Frankland's experiments are strained in the conclusions drawn from them by the Rivers Pollution Commissioners, and that such conclusions, instead of having been confirmed, have been refuted by subsequent practice.

4. That the statements with regard to Merthyr are without foundation or justification; the true facts being that for five months the sewage was applied in the proportion of 1,000, or at most 1,250, persons to an acre, then of 500 persons to an acre, and afterwards permanently of 229 persons to an acre. That though the formation necessary for downward filtration has been preserved, to all intents and purposes the filters are performing the permanent functions of an ordinary sewage irrigation farm.

III. THE DRY EARTH SYSTEM.

The first and most prominent objection to the introduction of this system into any town is, that instead of being substitutive for a water-carriage system, it is merely supplementary to it; for there must still be a water-carriage system in every town where the dry earth system is adopted, independent of it, which in its capacity shall be equivalent to such a water-carriage system as would be laid down even if the dry earth system was not practised. Therefore the cost of the dry earth system must be superadded to the cost of the water-carriage system, and thus the expense to be incurred by the town in respect of its drainage must be increased without any additional benefit being conferred, but, on the contrary, enormous additional expenditure, disadvantage, and difficulty.

To make good these propositions, assume that it is proposed to introduce the dry earth system into the metropolis, which may be taken to contain a population of 4,200,000 souls, and a superficial area of 150 square miles, composed to a large extent of roof and paved surface. The rain falling upon this hard surface cannot be absorbed, and must flow somewhere. Can the dry earth system take it? No, for it only proposes to intercept the solid refuse of the town; therefore a system of sewers at once becomes a necessity. Then, again, what is to become of the dry-weather flow of sewage or the sewage proper, the dimensions of which are measured by the water supply of a town? The water supply of London is 30 gallons a head per day, which gives a daily volume of sewage to be disposed of amounting to 126,000,000 gallons in twenty-four hours. Can the dry earth system deal with this great bulk of liquid? It does not profess to. Therefore there must be a system of sewers to receive it, and a system, moreover, of equivalent magnitude to that which would be adopted if the dry earth system were not in the town; for the dry earth system does not reduce to any appreciable extent the volume of sewage produced, inasmuch as its proper province is to intercept the solid matters, with a small portion of the liquid, which would otherwise be carried away in the water supply. Nor does the dry earth system diminish to any great extent the foulness of the sewage, inasmuch as, according to the Rivers Pollution Commissioners, the foulness of the solid refuse is only one-seventh of the foulness of the liquid, and the extraction therefore of so subordinate a polluting element from the sewage could not much affect the degree of its general pollution. Of course some portion of the liquid refuse is disposed of by the dry earth system, and in the sense, therefore, that the volume of foul liquid in the sewers is diminished in quantity and more diluted in quality it may be said that its foulness is reduced.

Again, if the dry earth system is to be a substitutive system, supplanting all other systems, what is to become of the garbage and washings of the streets, manure consequent on traffic, liquid refuse from breweries, slaughter-houses, cattle markets, urinals, factories where offensive trades are carried on, and to resolve the dry weather flow into some of its constituent parts, the household slops from kitchens and bedchambers? Either streets must fail to accumulate refuse, such trades and institutions cease to exist, slops and ablutions be banished, or some provision must be made for them. This provision can only be by means of a water-carriage system of sewers. Hence the first proposition becomes manifest, that given the dry earth system, the water-carriage system is still a necessity. If this be so, it follows as a natural consequence that the cost of the dry earth system must be superadded to the cost of the water-carriage system, and that as the dry earth system disposes of nothing which is not disposed of by the water-carriage system, it imposes, therefore, an additional expense without conferring any corresponding advantage.

To illustrate the statement that its operation would be attended with great cost and enormous difficulty the following figures, which proceed upon the assumption of its introduction into the metropolis, may prove instructive.

Moule's Earth Closet System as applied to London.—4½ lbs. of earth per head are equal to 14.66 cwt. per annum, or (on a population of 4,200,000) 3,078,600 tons, cubic yards, or cart loads; or 10,000 cart loads per day for six days in the week. The same quantity would have to be carried out, with the addition of 470 loads of fæcal matter. There are 450,000 houses in London; therefore each cart would deliver earth and remove soil from 45 houses per day. Suppose two pails to each house equal to 900,000 pails, then the figures will stand thus:

(1) *Prime cost of introduction of system.*

	£
450,000 houses in London; water-closets removed, earth closets substituted, and metallic wagons provided, say, 700,000 closets, at £10 each....................	7,000,000
10,000 carts and horses. { horses, £40 carts, £20 £60×10,000	600,000
3,078,600 cubic yards of earth taken off land ½ yard deep=1,272 acres, say two years' supply=2,544 acres at £300..................	763,200
Drying sheds (say 600 acres) at £1.200	720,000
	9,083,200
10 per cent. for London depots, offices, &c......................	908,320
	9,991,520

(2) *Annual expenditure.*

	£
3,078,600 cubic yards carted in 10,000 carts per annum at 12*s.*=£6,000 (1 man 4*s.*, 1 boy 2*s.*, 1 horse 4*s.*, wear and tear of cart 2*s.*)............	1,878,000
Labor in digging, loading and unloading carts, railway to London and back at 2*s.* per yard.........	307,860
	2,185,860

	£
Representing at 4 %..	43,717,200
	9,991,520
	53,708,720

These figures have not been strained; on the contrary, each item has been stated upon the most moderate estimate, and in some cases evidently at too low a figure. So that, after having constructed and paid for a water-carriage system, an additional preliminary expenditure of £9,991,520 would be needed to introduce the dry earth system, and an annual expenditure of £2,185,860 to maintain it, which, capitalized at four per cent. and added to the original outlay, represents the enormous sum of £53,708,720, and all this without effecting anything which the water-carriage system is not competent to perform. These figures also illustrate the cumbrous machinery which would be required to give practical existence to the system on a large scale. Where are thousands of acres to be obtained near London for the supply of mould? How is the constant manipulation of sewage refuse to be tolerated in an English town? And, lastly, how are the already overcrowded streets to accomodate the multitude of carts which would be let loose upon them, when the number of such carts would actually exceed the existing number of hansoms and four-wheeled cabs, omnibuses, and tram-cars?

Enough has been said to demonstrate the impracticability of introducing the dry earth system into a populous town. That it has a province cannot be denied, but it is essentially a limited one. It is well adapted for the wants of small rural villages like those of Halton and Aston Clinton in Buckinghamshire, with a joint total of 55 cottages, where it has been adopted, and of detached houses so situated that an abundance of suitable earth can be obtained. So also it may be appropriately applied in the case of detached public buildings under some general control, such as hospitals, workhouses, almshouses, and asylums, always premising, as a condition precedent to its adoption, that the soil in the neighborhood should be of a porous character, so as to be capable of absorbing the liquid house refuse undisposed of by the dry earth system, which is of so decomposing and putrescible a character that if allowed to stagnate in trenches or open ditches it would soon poison the atmosphere and generate malaria. But when it is proposed to introduce it into towns, the conditions are, as has been shown, entirely changed, and the sphere of its usefnlnes sbecomes narrowed in proportion as population increases. In fact it may be laid down as a universal proposition, that the applicability of the dry earth system becomes diminished in the inverse ratio to the increase of population to which it is proposed to apply it. The general conclusions, therefore, which must be laid down with regard to this system are:

1. That its adoption in the case of large towns would be costly, cumbrous, useless, and virtually impracticable.
2. That its use must be limited to rural places and detached buildings where a water-carriage system cannot easily be constructed.

IV. THE LIERNUR OR PNEUMATIC SYSTEM.

This system is one of those which still persists in asserting a pertinacious and pernicious vitality in spite of the experience which ought long ago to have extinguished it, and would have done so, had it not been for the energy of those interested in its survivorship. There appear no less than seven notices of it in the Society of Arts Journal of May 1876, one by Captain Liernur, one by T. A. Van der Kloes, Director of Public Works, Dordrecht, and the other five by Mr. Adam Scott, Captain Liernur's agent in this country. But as the term "Liernur System" may not suggest to the minds of all persons the principle upon which it is based and the machinery by which it is applied, the following description extracted from a pamphlet by Mr. Adam Scott, and verified by the inventor's specification, may assist a proper comprehension of its defects.

"In a building, in any convenient part of the town, is placed a steam engine, which drives an air pump, so as to maintain about three-fourths vacuum in certain cast-iron hermetically-closed reservoirs sunk below the floor.

"From these reservoirs central pipes radiate in all directions, following the main streets. On these central pipes are laid, from distance to distance, street reservoirs sunk below the pavement.

"From the street reservoirs, up and down the street, are main pipes, communicating by short branch pipes with the closets of each house.

"All the junctions of pipes with reservoirs are furnished with cocks so that they can be shut off or turned on at pleasure, like water mains, and are got at by cock boxes, and turned by keys in the ordinary way.

"The vacuum created in the central building reservoirs can thus be communicated to any given street reservoir, so as to furnish the motive power by which, when the connections with the houses are opened, all the closets are simultaneously emptied.

"When their contents reach the central reservoir, they are in like manner forced through the central tubes to the reservoirs under the central building, and thence transfered by means of vacuum power to the hermetically-closed tanks above the floor of the building. From these retorts the matter is decanted in a fluid form in barrels, for immediate transport to the country, by means of hermetically-closed apparatus."

As regards the inception of this system, it is said that Captain Liernur got his idea from the rude pneumatic system in use at Milan, where a vacuum is obtained by first filling the tanks with water and then exhausting them. The water is carried about in ox carts. The system was first introduced into Prague in 1863 for some military barracks ; later into an insane asylum at Hanau. In answer to the statement of the Berlin Commission, that in these places it had proved offensive, and so unsatisfactory as not to be extended, Captain Liernur stated that the first attempts had been improved upon, and that in Holland he had overcome the difficulties which had proved so troublesome at Prague and Hanau. A Commission at the Hague recommended Liernur's system for that city in 1867, but it has never been introduced there. It has had a similar fate lately in Rotterdam. In St. Petersburg, at the beginning of last year, Captain Liernur was making an experimental introduction of his method, with the understanding that the city was not to bear any of the expense, aud that no extension was to be made unless the system proved satisfactory. About the same time he was also preparing plans, which he hoped might be accepted, for Gaeta and Naples. His system has been partially introduced at Leyden, Amsterdam, and Dordrecht in Holland, but none of the cities or towns in England have been persuaded to follow the example of their Dutch neighbors.

The objections to the system, which render its adoption impracticable, may be shortly stated. The most important is that which has already been mentioned with regard to the dry earth system, and which apart from all other considerations, is absolutely fatal, namely, (1) that the Liernur system only deals with the solid refuse and a limited quantity of liquid, and is therefore merely supplementary to a water-carriage system, thus entailing additional cost and conferring no corresponding advantage. (2) The machinery by which the pneumatic system is applied, consisting principally of wrought-iron pipes and chambers, is enormously expensive, and of limited capacity as compared with brickwork sewers. (3) That it is complicated and liable to get out of order. (4) That the closet arrangements for the houses, as well as the operation of decanting sewers, are offensive, and such as could not be tolerated in an English town.

From the evidence afforded by the experience of the system in Holland, it may be premised, in the first place, that in the towns where the system has been introduced in that country, there was absolutely no previous sanitary provision, the whole sewage being either thrown by hand, or allowed to pass away into the tide-locked canals characteristic of the Netherlands towns, so that the introduction of any system was of necessity an improvement on the former unsatisfactory condition of things.

1. *Leyden.*—Plans were accepted for Leyden in 1870. The population is 40,000, and out of this population 1,200 people in the poorest quarter have had the Liernur system applied to their houses. The evidence as to the measure of success with which its operation has been attended has been a matter of animated discussion. In a letter, addressed on April 1st, 1873, by the Master of Public Works, J. W. Schaap, to the Magistrate

President of the Commission of Manufacturing Industry, he speaks in the following favourable terms of the system (Seventh Annual Report of the State Board of Health of Massachusetts, p. 319): "While it cannot be denied that the expenses of introducing the system and the necessary experiments were greatly in excess of what was anticipated, that the amount received for the fæcal material was less than was anticipated, and that the interest on the money and the instalment paid must be taken into account, yet a favourable judgment, as to the result, must remain." And again, "Therefore the great question concerning this matter is to be looked upon as entirely settled, and the system deserves recommendation to extension everywhere as well as in this city." So far the opinion was satisfactory, but it was soon to undergo a change. In one of his Papers to the Society of Arts, Mr. Scott states, that it was reported that a Government Commission from this country had visited Holland to inquire into the Liernur system. After expressing doubts as to the existence of such a Commission, he refers to their reported conclusions (p. 60): "as these have thus, in an irregular way, become public, it is right I should, to prevent persons being misled, record the fact that most of the so-called conclusions are misleading, and some of them, in dealing with matters of fact, are simply untrue, as I can show by official letters in my possession. For instance, one of the statements made is that the system is not to be further extended at Amsterdam and Leyden. I have spoken above as to Leyden." On referring back Mr. Scott appears to have said: "I may add to this that the Commission for Public Works have reported in favor of extending the system to the whole town of 40,000 people. In that report they designate it as 'the system of the future for all large towns.' The Financial Commission have reported in similar terms, as have also the Mayor and Aldermen." Surely there is a strange discrepancy of statement somewhere. In his Paper to the Conference at the Society of Arts on the 9th and 10th of May, 1876, Mr. Scott asserts that the Government Commission, if it existed, has been guilty of untruth, and that the Mayor of Leyden had actually reported in favor of the extension of the Liernur system to the whole city. But how can this statement be reconciled with the following opinion expressed by the Mayor on the 21st of January preceding? Replying to the United States Consul at Rotterdam, he says (his letter being published in the Seventh Annual Report of the State Board of Health of Massachusetts, p. 318), "Meanwhile there exists for the present no intention to extend the system in other parts of the city, because of the considerable expense involved in so doing." And again, in conclusion, "Although the experiment with the Liernur system, taken in this city from a technical point of view, may be said to have entirely succeeded, the financial results, however, must become considerably more favorable before an extension of the system will be resolved upon." If, then, the Mayor, to whom Mr. Scott refers, be taken as the best interpreter of his own sentiments, and of the intentions of his fellow-townsmen, the Government Commission, if it existed, was right after all, and did not state what was "simply untrue"; but Mr. Scott must himself be in error with regard to the "matters of fact." The system, then, is not to be extended at Leyden, on account of its cost.

But perhaps the most suggestive and pertinent evidence which has been contributed, with reference to the operation of the system at Leyden, is that which is contained in a Report by a Committee of the Town Council of Southport, which was appointed, at the time when its adoption for Southport was under consideration, to visit Leyden and report upon it. The Report is dated December 15th, 1874. As the statements and opinion expressed apply with equal force to its operation in other localities, and go far to prove the objections which have been raised against its use, it will be expedient to refer to them. The following are the more important passages:—"It should be stated that no slop, or waste water, passes into the Liernur closets. These liquids are disposed of in the quarter sewered on the Liernur system, in the same way as in other parts of the city. Neither does kitchen waste go into the closets, so that the system, as developed at Leyden, is one of collection of human excreta only, and no other duty is subserved by it." "In order to complete the statement of what we observed at Leyden, it is necessary to say that 'sewers,' as we understand the term, do not exist there. The canals which intersect the city in every direction are the sewers, and house drains enter the nearest canal at the most available point, taking apparently the most direct route without the slightest care or regard as to whether they go under houses or not. This primitive method appears to have prevailed from time immemorial." "A pneumatic system of collecting human excreta would, therefore, relieve the sewers of no appreciable amount of their contents under the present, or even a universal watercloset system. The daily collection at Leyden from the Liernur closet is only at the rate of about a quarter of a gallon per head, so that even if this were much more augmented than would be compatible with any economical manufacture of 'poudrette' the relief afforded to the sewers, however valuable as regards the quality of their contents, could, as regards quantity passed in, be altogether insignificant. It is clear, then, that the pneumatic system will not, if adopted, relieve the borough of the cost of constructing new sewers, which must be of sufficient capacity to carry away, not only the daily flow of between 30 and 40 gallons per head of a rapidly increasing population, but the storm water, which constantly increases in volume as a larger area of the borough is built over and paved, and less of the rainfall sinks into the soil itself." "Captain Liernur himself put down £4 per head of population as the probable cost of applying the system here."

"The impression of your deputation is:

"1. That the system of sewerage already adopted by the Council would be absolutely necessary, even if the pneumatic system were in actual operation in the borough.

"2. That the pneumatic system is highly ingenious and efficient, and probably highly meritorious in a sanitary sense also, though no statistics appear to have been collected which would give us a measure of its excellence.

"3. That in a town of so large an area as Southport, relatively to population, the pneumatic sewerage works would be enormously costly to put down and maintain.

"4. That the amount of revenue derivable is very difficult to estimate.

"5. That, from a financial point of view, it would, therefore, be a great experiment; and,

"6. That if experience in England should at any future time demonstrate in a fuller and more complete manner than has been done at Leyden, that the pneumatic system is, as has been claimed for it, 'an engineering, a sanitary, and a financial success,' it will be as open to Southport as to any other place to lay it down and profit by the practical experience of places having larger means to undertake costly experiments."

The above Report proves that the Liernur system is merely supplementary, both in its operation and its cost; that it disposes of nothing that the water-carriage system cannot dispose of without its assistance; and that there is no sanitary advantage to be gained by its adoption. On the question of cost, it is to be observed that Captain Liernur put the cost of introducing the system at £4 per head of population at Southport, as against £2 5*s.* at Leyden. Making allowance for the difference in the price of labor between the two countries, it is probable that £4 per head in England is considerably below the mark; but adopting it for the purpose of argument, and taking the population of London at 4,200,000, the cost of the introduction of this system into the metropolis would reach the enormous total of £16,800,000; and to this expenditure the cost of the main-drainage system, which would be in no sense superseded by the new system, would have to be superadded. It is difficult to estimate what the annual expenditure for the maintenance and working of the system would be; though one thing is sufficiently clear, that no profitable return from the manufacture of 'poudrette' could be looked for in England.

Assuming the cost of maintenance and working at 2*s.* a head, this would represent for London an annual outlay of £420,000, which, capitalised at £4 per cent. would require £10,500,000 to be raised; and adding this sum to the prime cost, it would appear that the introduction of the system into the metropolis would involve an expenditure of £27,300-000. It is worthy of remark that it is customary in Continental towns to remove the solid refuse by manual labor, at a cost largely in excess of anything in operation in England, and the annual cost, therefore, of the pneumatic system would not commend itself so strongly as an objection abroad as it would in this country.

2. *Amsterdam.*—Mr. Scott, in his Paper of May 1876, on the operation of the system at Amsterdam, quotes passages from a Decree of the Common Council, issued on the 10th of April, 1873—an account of the system by Dr. Egeling, of the Hague, which appeared in "Public Health," November 2nd, 1874—and a letter from Mr. Bergsma, of Amsterdam, dated August, 1874, in proof of the success with which its adoption has been attended there. Without going so far back in point of time, the following extracts from the Seventh Annual Report of the State Board of Health of Massachusetts, United States, published in January 1876, which treats of this system from personal observation, sufficiently indicate the measure of success with which its practical operation has been attended. The writer says (p. 312). "In Amsterdam, the odors from the canals have been for years extremely offensive. They are all open sewers, with no current, and foul gases are constantly bubbling up during the summer. During winter the offence is less, and arises chiefly from the distribution of so much filth upon the ice. Naturally, the authorities were willing to try any system which promised any solution of their difficulty. They adopted the Liernur system in one of the poorest quarters of the city in 1870. On the 10th of April, 1872 Mayor den Tex and the aldermen decided upon its compulsory adoption in seven other small districts. At the present time, it has been introduced for a population of 6,000, or one-fiftieth of the whole city. Mayor den Tex, and the present Master of Public Works, state that it has given entire satisfaction in the poorer parts of the city, where there was absolutely no accomodation before, and where the closets connected with it are out of doors. They state, also, that its first cost renders it doubtful whether it will be extended even there; and that among the better classes the system is considered inferior to waterclosets and cesspools." This extract contrasts strangely with Mr. Scott's Report, as to the extension of the system at Amsterdam. Again: "Captain Liernur's calculations of cost, although very high, provide for the removal of excrement alone. With his system, there must be also sewers for rain-water, street drainage, slops, &c. He would have separate pipes, too, for the drainage of the soil." Again: "By the politeness of Mr. Bergsma, Secretary of the Board of Works of Amsterdam, a friend of the system, I was able to see it in actual operation.

"The emptying of the tanks was complete, rapid, and, as far at I could see, successful. There was no trace of odor. In the central building, where the matter was transferred to barrels, and in its immediate vicinity, the stench was very great. In the houses of the poorest class, where the house is used by several families, they become soiled, and in some cases filled to overflowing, before any one takes care of them. They become clogged occasionally by coffee-grounds, ashes, &c., which will find their way into them. They are not as offensive as the midden, or privy, still to be found in many large cities. They are in all cases out of doors, and the people who use them prefer them to the arrangements which existed before their introduction.

"In a few houses of the middle class, where they are in the yards, a few rods from the houses, they were scrupulously clean, received frequent washing, and were not offensive. In a primary school-house, where the closets were separated by only a narrow passage-way from the class-rooms, they were very frequently washed, and although there was a slight disagreeable odor, there was nothing really offensive at the time of my visit. It is undoubtedly true, as the teacher said, that the closets are much more satisfactory than anything which they had ever had before.

"In the houses of the better class, there is so much complaint of the bad odor from the closets, that they get rid of it as best they can by flushing, with a sudden dash of a large quantity of water, after each use of the seat. Occasionally they become entirely clogged, when the odors are simply insupportable. Mr. Dyck, a resident of Amsterdam, who has also a farm in the country, states that twice in one year this intolerable stench drove him and his family entirely out of his house, until the obstructions could be removed.

"Captain Liernur proposes to meet this difficulty by automatic waterclosets, using only one litre of water at each time of use; but they have not been put to the test of practice, and so small a quantity can be of no more service there than in our ordinary hopper closets.

"A part of the original plan was to sell the excrement at a remunerative price; but people of the neater class cannot be prevented from flushing their closets liberally, and the contents of the reservoirs contain so large a proportion of water as to have the general appearance of ordinary sewage, except in color. Dr. Amersfordt made a contract with the city to take the whole of it, to be delivered on his farm at 11½ cents (U. S. currency) for each 100 litres. The contract expired January 1, 1875, and has not been renewed, for two reasons. Dr. Amersfordt states that the distribution of largely-diluted manure from barrels is costly and difficult; also that, when delivered to him in the winter, it is often frozen in the barrels. It is now sold for 16¾ cents (U. S. currency), delivered by boats on the farms, during the summer only. In winter it is carted down below the city and thrown into the harbor, as it cannot be sold."

Thus the example of Amsterdam proves the substantial character of some of the objections which have been urged against the system. For instance, it is abundantly clear that the central building, where the process of decanting the liquid into barrels goes on, is an institution which could never be tolerated in an English town; that the closets are of such a character that, though they might be appreciated by the poorer classes in the open air, would be insufferable as applied to the wants of a high-class population; that the manufacture of manure can never be attended with profit, first, because the closets can only be preserved in a sanitary state by a copious use of water, which water is fatal to the efficient manufacture of "poudrette," and, secondly, because it cannot command a sale.

3. *Dordrecht.*—At Dordrecht, the Liernur system was applied in 1875, upon an area of 17,300 metres, containing a population of 800 inhabitants, distributed over 128 houses fitted with 117 privies. The great feature of its operation there appears to be the manufacture of poudrette on a large scale, and a provisional project for the extension of the system to other parts of the city is contingent upon the success of this manufacture. In his account of the system at Dordrecht, published on the 7th of October, 1875, which appears in the Journal of the Society of Arts of the following May, Mr. T. A. Van der Kloes, Director of Public Works there, states that it would be several months before the "poudrette scheme" could be in regular operation, and he is not therefore in a position to supply statistics with regard to its manufacture. In a letter addressed, on the 20th of January, 1876, by D. W. Hoop, the Mayor of Dordrecht, to the Honorable Frederick Schütz, United States Consul at Rotterdam, the Mayor states that "the extension of the above-mentioned provisional project is delayed, awaiting the results of the poudrette preparations, with regard to which it is thought that sufficient information will be obtained within a few months;" and again, in conclusion, "with regard to the profit on the capital invested in the system, nothing can be stated as yet with certainty, as this community finds itself still too undecided to do so definitely. While the experiment is being carried on, we await more definite regulations from the person who has the contract for cleansing the city."

So that so far poudrette has not proved a financial success at Dordrecht. At Leyden, the Mayor's letter proves that it has been a financial failure, and the same result has followed the manufacture of manure by Dr. Amersfordt at Amsterdem. In addition to this practical experience, the Report of the State Board of Health for Massachusetts (p 317) concludes by saying, "It should be remembered that a similar experiment failed in Amsterdam twenty years ago, that the Paris poudrette sold for one twentieth of the value placed upon it by the chemists; and that an English company has become bankrupt in attempting to pay two francs a cubic mètre for the contents of the cesspools in Paris, leaving several hundred thousand cubic mètres at Bondy, of which the authorities would be glad to rid themselves."

Surely nothing more is wanted in the way of practical proof to point the irresistable conclusion: that though the Liernur system may be used with partial and costly advantage in the tide-locked cities of the Hague, where a system of sewerage is said to be impracticable, the population is not fastidious in its notions, and anything in the nature of a system must be an improvement on the previous unsanitary condition, it can never for one moment have a place in the operations of the English engineer.

V. SEABOARD AND TIDAL OUTFALLS.

Having regard to the unsuitability and costliness of chemical processes, and of the dry earth and Liernur systems, and the many difficulties, financial and otherwise, which attach to the disposition of sewage upon land, there can be but little doubt that, given a seaboard town, the proper destination of the sewage is the sea. By some, who insist strongly on the value of sewage, it has been objected that such means of disposal are, in fact, an absolute waste of most precious material—material which, if properly handled, must constitute in itself an almost inexhaustible mine of wealth. But day by day the belief that sewage possesses any recuperative value is becoming rudely dispelled, and the doctrine is gaining ground that, instead of keeping sewage at the door as though it were a friend, the right view is to treat it as an enemy, to get rid of which by any means or at any cost should be the primary consideration.

But though such an outfall certainly suggests,in the case of a seaboard town, a natural, economical, and feasible method of dealing with its refuse, there are a few points affecting its construction and position, such as the propriety of the tide-locked outfalls, the course of currents, silting up, which are more or less matters of controversy. Into the first of these points, a tide-locked out-fall, it is not proposed to enter in this Paper, as it comes more properly within the province of a discussion upon the principles of sewer construction than the disposition of sewage at the out-fall. As to the second point, namely, course of currents, it may be laid down as a general rule that, assuming any doubt to exist with regard to the force or set of the currents to which the sewage is to be committed, careful float observations should be taken with a view to preclude the possibility of its being returned upon the beach. If such precautions are taken, there ought to be no special difficulty in guarding against the occurrence of such a return. The last point, or the question of silting-up at the outfall, is one which affects tidal rivers rather than marine outfalls. But if the site for such outfall be judiciously selected, and proper means be taken, in the town which drains to the outfall, for the exclusion of silt and heavy matters by catchpits and an efficient system of scavenging, there can be little reasonable apprehensions of silting-up taking place. At the metropolitan outfalls, no less a quantity than 120,000,000 gallons of sewage, taking the dry-weather flow alone, is passed daily into the river—a volume of sewage equivalent to a fair sized stream—and yet no silting-up has occurred. In fact, it would seem to be a logical inference, in the absence of disturbing causes, that where the velocity of the current into which the sewage is discharged is greater than the velocity of the current which carries it down through the sewers to the outfall, there cannot be much danger of silting-up or interfering with the navigable channel. Of course it is difficult to frame a rule to meet all cases. Much must depend upon the means taken to exclude heavy matters from the sewers, and much upon the character of the locality sewered, some localities producing far more detritus than others. Then, again, the amount of sewage discharged, the sectional area of the river, the volume of water in its channel, and the velocity of the current, are all items in the consideration of the propriety of the outfall, so far as silting-up is concerned. But still it may be accepted as a generally sound proposition, that where efficient means are adopted for the exclusion of heavy matters, such as road detritus, from the sewers, sewage may be discharged into tidal rivers without any material amount of silting-up taking place at the outfall.

This completes the classification which the Author adopted for the purpose of reviewing the various processes or systems in practical use for the disposal or utilization of town sewage. He now ventures, referring once more to such classification, to submit the following definite conclusions for the consideration of The Institution of Civil Engineers, in the hope that they will induce such a discussion as may tend to promote a more unanimous opinion, than at present appears to exist, upon the various questions which attach to their consideration:

1. TREATMENT WITH CHEMICALS.

Conclusions—No chemical process can efficiently deal single-handed with sewage, but must be assisted by subsequent natural or artificial filtration of the treated sewage, and therefore no chemical process *per se* should be adopted for the purification of town sewage.

2. APPLICATION OF SEWAGE TO LAND.

(*a*) *Irrigation.*

Conclusions—That where land can be reasonably acquired, irrigation is the best and most satisfactory known system for the disposal of sewage. That no profit must be expected from the cultivation of crops by the sanitary authority, and only a moderate one by the farmer. That no definite standard can be laid down as to the proportion population should bear to acreage.

(*b*) *Intermittent Downward Filtration.*

Conclusions—That Intermittent Downward Filtration may be practiced, where irrigation cannot be reasonably adopted. That experience shows that the permanent proportion of population to acreage, where land is drained six feet deep, should in no case exceed 500 or 600 persons to an acre. That the term Intermittent Downward Filtration means no more than the production by deep drainage of a state of things frequently found in irrigation, and that irrigation can in some cases accommodate a larger proportion of population to acreage than Intermittent Downward Filtration. That Intermittent Downward Filtration, as expounded and explained by the Rivers Pollution Commissioners, has never had and never can have any practical existence.

3. THE DRY EARTH SYSTEM.

Conclusion—That the dry earth system can never be adopted for the purpose of dealing with town sewage, but may be occasionally used with advantage in small hamlets or detached buildings or institutions.

4. THE LIERNUR OR PNEUMATIC SYSTEM.

Conclusion—That the Liernur system is of such a character that it should never be imported into an English town.

5. SEABOARD OR TIDAL OUTFALLS.

Conclusion—That towns situated upon the sea-coast, or within the tidal range of rivers, should avail themselves of the means of outfall thus presented, as affording the most economical and efficient means of dealing with their sewage, careful regard being always had to the position of the outfall. That proper precautions being observed for the purpose of excluding silt from the sewers, and care being taken in the selection of outfalls upon the banks of tidal rivers, there is no reasonable danger of the silting-up of the navigable channel.

TORPEDO PRACTICE.

From "Iron."

The actual use of torpedoes as engines of war, though theoretically most valuable both for attack and defence, has drawbacks which are worth considering. We may refer, by way of evidence, to Captain Scott's remark on Mr. Donaldson's paper in regard to the Whitehead torpedoes—that every ship of war carrying a supply of these missiles, carries a terrible and novel source of danger to herself. Again, in the present war, it is reported that, with the aid of skilled divers, torpedoes can be successfully disarmed. It is not difficult, therefore, to understand that the manufacture of torpedoes is one thing, but that their successful use is totally different. For coast protection there are two classes of torpedoes, the mechanical and the electric. At the present time, except in peculiar circumstances, the mechanical torpedo presents so many disadvantages —not the least of which is that its success depends a great deal on accident, and is, consequently, very problematical —that it may almost be excluded from consideration. Yet, when mention is made of the use of torpedoes during the present war on the Danube or elsewhere, it would not do to assume that the mechanical torpedo is discarded. This weapon is useful enough if it is properly planted, if it is properly made, if a great many other considerations have been carefully noted, and if, finally, luck is on its side. But, once laid down, it cannot be tested, nor can it be manœuvred in any way. It is as likely to injure friend as foe, and is not unlikely to be found perfectly harmless to both. In fact, it is a two-edged weapon, which is as dangerous as it is useful, and it proved its terrible unsteadfastness in a variety of ways during the American War. Now, when it is reported that the Turks are prepared to render Russian torpedoes inoperative, reference is evidently made to this class of weapon, which can easily be ruined without risk of serious danger. The electric torpedo is, however, a more formidable weapon, and can hardly be destroyed or ruined, except by accident, or by taking advantage of extraordinary carelessness on the part of those who are interested in its safety. The deadly nature of this weapon may be realized by remembering that the operator is on shore, undisturbed, with plenty of time for quiet and accurate calculation, and dependent only on nerve for the success of his torpedo. But, and this "but" is very large, the use of electricity, especially in a case like this, where the results are so serious, requires a large exercise of dexterity and delicacy. The smallest deviation from the prescribed course may lead to an awful disaster, and a slight want of nerve may send a friend instead of an enemy to the bottom. In fact, the position of the operator is not unlike that of a pointsman or signalman on a railway. Captain Fisher places this in a strong but intelligible light in his remark that "when that most trying time comes in which the utter destruction of an enemy's ship may rest solely on the operator, he will be able to act instantaneously, without hesitation. There is no time to think then. The enemy, if he is wise, will be forcing his passage at his utmost speed—one instant he is in danger from the torpedo, the next he may be safe—with the exception perhaps of a good shaking." His subsequent remark is worthy of being inscribed in letters of gold in every battery and every man-of-war:—"The great secret of the successful management of torpedoes will be found to consist in paying the utmost attention to apparently trifling precautions, and in trusting no one."

In the use of torpedoes, or in their actual application in the time of war, one of the most important lessons to be learnt is their planting. Great judgment and care are essential here, and it forms naturally an important feature in all courses of torpedo instruction. In referring to the difficulties of this operation, Captain Fisher has pointed out that it is of the utmost importance in laying down a torpedo to insure the preservation of its exact position; to avoid any chance of a fracture of the insulated wire through twisting; to calculate accurately the exact height and force of the tide, so as not to expose the torpedo at low water, nor make it inoperative at high; and to consider carefully currents, winds, the action of spring tides, and any other exceptional points in the channel under protection. Position also requires great consideration and experience; for it is quite possible to lay down scores of torpedoes which would be perfectly useless. For instance, it is essential to plant them at such a distance from the line of coast under protection as to prevent ships from coming sufficiently near to use their guns effectively. It is also important to plant them, like lettuces or cabbages, sufficiently far apart from one another not to interfere with independent action. Reference has been made recently to the valuable aid to be derived from the *camera obscura;* but this instrument, useful as it is, has been rather overrated, for its accuracy in daytime is difficult to insure except in peculiar circumstances, and at night it would naturally be useless.

But while the difficulty in using the electrical torpedo is chiefly technical or mechanical, and can be overcome by practice, the treatment of torpedoes to be used at sea, whether in the "spur" or "Whitehead" form, calls not only for technical skill and dexterity, but for personal qualities of the highest order. The "spur" torpedo is intended to be used in the new Thorneycroft boats, such as the Lightning. The object of these boats is, by the attainment of a speed of twenty or twenty-five miles an hour, to attack any man-of-war by coming up with it, launching against it a torpedo, and then running away again. Simple as this operation may seem, it is attended with such manifest danger as to prove that special qualities of nerve and readiness are requisite. To use a torpedo in this way efficiently, it is important to be very careful both as to the place and mode of using it. As failure will, almost inevitably, prove fatal, constant practice is necessary to attain the necessary proficiency. In fact, to be successful demands on the part of the officer chosen for this work an amount of nerve and of delicate judgment not exceeded in any other warlike operation. The great recommendation about this mode of using torpedoes is that, if successful, its success is undoubted. The case of the Housatonic, mentioned by Mr. Donaldson at the United Service Institution, is a case in point. But precision in attack is absolutely essential. It is quite possible, as has been proved by experiment, to explode a torpedo harmlessly, although in actual contact with a ship's side. In one case the charge was submerged only six feet, and although the charge consisted of fifty pounds of powder, no vital injury was done. To quote Captain Fisher on this point—"The vertical effect of a submarine charge, no matter at what depth, is much greater than that in any other direction; this is but natural. The explosion is dammed up laterally, stopped by the bottom, reacted upon laterally, reacted upon by the bottom; so it is evident the greatest effect must take place in a vertical direction. The principle should always be borne in mind when endeavoring to attack an enemy's vessel with outrigger torpedo boats." He quotes an instance, the case of the Albemarle, where "the torpedo was submerged ten feet, and exploded under an overhanging portion of her stern, which, though several feet from, yet was virtually over, the torpedo."

Although this subject admits of much elaboration, and a great deal remains to be said about the special danger of the Whitehead torpedo, not only in its actual use, but in the mere fact of carrying such a destructive weapon in a man-of-war, we have said enough to show that there are two sides to the torpedo question. The torpedo is a very double-edged tool, and, valuable as its powers are for attack, its value depends mainly on the exercise of exceptional and high qualities.—*The Times.*

Captain Morgan Singer, of the Vesuvius, torpedo ship, has been carrying out a series of experiments in the capacious repairing basin at Portsmouth, with the object of discovering the best means of defending ships against the attack of the Whitehead torpedo. Up to the present time the ingenuity of the naval architect has been mainly exercised to protect ships against the entrance of shot and shell; and while for this purpose he has gone on increasing the thickness of the side armor, the bottom of the ship, against which the attack of the torpedo is directed, remains in almost its original weakness. How to protect the hull against both the gun and the torpedo and at the same time preserve the seaworthiness of the ship is one of the many problems in naval construction which have yet to be solved. In the meantime two methods of protection against torpedoes are proposed—viz., the employment of fast "satellites" to ward off and attack the torpedo boats before they can get within range, and the erection of crinoline frames around the ships themselves, by means of which the rush of the projectile would be arrested before striking the hull. Both devices have their disadvantages. The general use of patrolling craft would immensely complicate the exigencies of an engagement by provoking a number of subsidiary battles around the main action; besides, the champions of these small craft seem to forget the all-important fact that in the next naval engagement the principal antagonists would themselves use the torpedo. Hence, even with the adoption of an auxiliary fleet, it is essential that our ships of war should be fitted with some defence against this terrible submarine engine. The torpedo net will necessarily impede the way and obstruct the manœuvring of an ironclad, and, such being the case, the object of Captain Singer has been to ascertain the form of crinoline which would oppose the least resistance to the water and the most resistance to the torpedo. Various forms of nets, composed of alternate lines of bars and chains, have been tried, but the size and power of the missile have been so increased of late that it has been found that the torpedo nets of the Thunderer afford little or no protection. Last week a chain net, formed of chains 5-16ths of an inch thick, was easily perforated by the Whitehead. The great fault of the nets is their extreme rigidity, which opposes a solid wall to the impact of the torpedo, and the bars are consequently snapped. After many trials, however, very successful results have been obtained from a wire grummet, matting, composed of wire strands about half an inch in thickness rove into open meshes. It possesses considerable flexibility, and, as it yields when struck, the force of the torpedo is not suddenly, but gradually, arrested, and as it loses its momentum it is thrown back by the recoil of the mat. During the experiments a torpedo thus forced back twirled its tail off without inflicting any injury to the matting. This is the nearest approach to perfection which has yet been attained.

MOMENTUM AND VIS VIVA.

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II.

III. IMPACT OF ELASTIC BODIES.

In the formula deduced in the last section for the common velocity of two bodies after impact, one of them was supposed to be at rest before impact. In order to make the formula more general let us suppose both the bodies to be in motion, in the same direction, their centers of gravity being in the same line of motion, and suppose their velocities to be such that impact shall occur. Let M and M' be the masses of the bodies, and U and U' their respective velocities. Let M' be in advance of M; then in order that impact may occur, U must be greater than U'. In that case the mass M will overtake the mass M', and have its velocity reduced by the impact, while that of M' will be increased, until they acquire a common velocity. Let X represent that common velocity, or the velocity of the two bodies at the instant their velocities have become the same. Then, by a course of reasoning similar to that employed before, we shall have the proportion,

$$M : M' :: (X-U') : (U-X);$$

from which we get $X=\dfrac{MU+M'U'.}{M+M'}$

The common velocity is deduced without reference to the nature of the bodies, and is therefore the same for elastic as for inelastic bodies. The bodies would be compressed by the impact, and if perfectly inelastic would remain compressed, with no tendency to recover their form, and would move on with the common velocity. If they were elastic they would immediately tend to recover their form, and in so doing would re-act upon each other in such a way as to produce a further change of velocity in each; and they would separate, and neither of them retain the velocity X. In the impact of elastic bodies we therefore distinguish two periods of the action. In the first period the bodies are compressed, and acquire a common velocity, and in the second period they to a greater or less extent recover their form, and their velocities again become different.

A perfectly elastic body would be one which after compression should completely recover its form, and which should exert in the recovery of its form a force exactly equal in intensity, at corresponding points of the action, in the inverse order, to the force by which it was compressed. There are no known solids which are perfectly elastic. Physicists usually call the force with which bodies are compressed the *force of compression*, and the force with which they recover their form the *force of restitution;* and the ratio of these two forces, for the various common solids, has been determined by experiment. This ratio is called the *modulus of elasticity.*

If impact were produced between two perfectly elastic bodies a certain velocity would be destroyed or generated in each respectively during the first period of the impact, while the bodies were undergoing compression and acquiring a common velocity; and since they would be urged apart in the recovery of their form by a force correspondingly equal in intensity and duration to that exerted in the compression, each would suffer a further change of velocity during the second period of the impact, equal to the change produced during the period of compression. If the bodies were perfectly inelastic there would be no second period to the impact and no change of velocity in either from the action of a force of restitution. And it is obvious that with bodies partially elastic the ratio of the change of velocity effected in each body during the first period of the impact to that during the second period will depend on the degree of elasticity of the substances. This ratio is taken by physicists as equal to the modulus of elasticity. Whether that modulus is so determined that this is precisely correct in all cases, may perhaps admit of question; but for all the purposes of the present discussion we may consider it sufficiently accurate. This ratio or modulus is denoted by E. For perfect elasticity E would be equal to 1; for perfect inelasticity it would be equal to 0; and for all intermediate degrees of elasticity its value is between those limits.

Recurring to the case of the mass M' moving with the velocity U', followed in the same line by the mass M with the velocity U, greater than U', an expression has been found for their common velocity at the instant of greatest compression, or the close of the first period of the impact; and that common velocity has been denoted by X. The velocity which the mass M will have lost during this first period will then be represented by $(U-X)$; and the velocity which will have been imparted to M' in the same period will be represented by $(X-U')$. In the second period of the impact the force of restitution, from the elasticity of the bodies, will still further diminish the velocity of M and increase that of M'. The ratio of the velocities destroyed or produced in each body during the two periods being E, the subtraction from the velocity of M in the second period will be $E(U-X)$; and the additional velocity imparted to M' in the same period will be $E(X-U')$. Thus the total subtraction from the velocity of M, during both periods of the impact, will be

$$(U-X)+E(U-X);$$

and the total addition to the velocity of M' during the whole time of impact will in like manner be

$$(X-U')+E(X-U').$$

If we now denote the velocity of M after the bodies separate by V, and that of M' by V', we shall therefore have

$$V=U-(U-X)-E(U-X)$$
$$=X-E(U-X).$$
$$V'=U'+(X-U')+E(X-U')$$
$$=X+E(X-U').$$

If in these formulas we substitute the value of X previously obtained, we find after reduction,

$$V=\frac{MU+M'U'}{M+M'}-\frac{M'E(U-U')}{M+M'}.$$
$$V'=\frac{MU+M'U'}{M+M'}+\frac{ME(U-U')}{M+M'}.$$

If the mass M' were moving before impact in the direction towards M, instead of in the same direction, we should find the same formulas with the exception that the algebraic sign of U' would be changed throughout. If the mass M' were at rest before impact, U' would be equal to 0. The latter part of this demonstration mainly corresponds to that given in Silliman's Physics.

From the above formulas, if we know the masses of the two bodies, their velocities before impact, and the value of E, we can easily determine the respective velocities of the bodies after impact; and knowing their velocities we can then calculate the momentum and vis viva of each. Let us illustrate the use of the formulas, and the proper interpretation of the results, by the solution of a special example.

Suppose there be two cast-iron balls weighing respectively 32 1-6 pounds and 160 5-6 pounds. Their masses will be $M=1$, and $M'=5$. For simplicity suppose M' to be at rest, but free to move, and suppose M to strike it in the line of their centers with a velocity of 1,000 feet per second. We shall have $U=1,000$, and $U'=0$. Take the value of E for cast-iron at 0.73, as given in Silliman's Physics, and suppose the balls capable of enduring the shock without breaking. Then by substituting these values of M, M', U, U', and E, in the formulas for the value of V and V', we obtain after reduction

$$V=-441\tfrac{2}{3}. \qquad V'=288\tfrac{1}{3}.$$

The value of V being negative we infer correctly that the ball previously in motion with the velocity of 1,000 feet per second will recoil from the shock with the velocity of $441\frac{2}{3}$ feet per second in the direction opposite to that of its former motion. But its momentum, or the intensity of the constant pressure required to bring it to rest in one second will nevertheless be $441\frac{2}{3}\times 1 = 441\frac{2}{3}$ pounds. The value of V' being positive the ball previously at rest will move with the velocity of $288\frac{1}{3}$ feet per second in the direction of the motion of the first ball before impact. Its momentum will be $288\frac{1}{3}\times 5=1,441\frac{2}{3}$ pounds. The arithmetical sum of the momenta of the two balls after impact is therefore

$$441\tfrac{2}{3}+1,441\tfrac{2}{3}=1,883\tfrac{1}{3} \text{ pounds.}$$

But the momentum of the first ball before impact was only $MU=1,000$ pounds. If then momentum represents a quantity of motion, and if motion cannot be generated by the impact, how shall we account for this great increase of momentum? We should have to assert that since the mass M after impact moves in the direction opposite to its former motion, its quantity of motion is less than no motion at all, or is equivalent to the destruction of $441\frac{2}{3}$ units of motion, and must therefore be subtracted from the quantity of motion of the mass M'; thus leaving a quantity of motion in the two balls equal to 1,000. But although it may be true that two equal and opposite pressures on a body will destroy each other, so that the body shall remain at rest, it is not true that the motion of two equal bodies with equal velocities in opposite directions is equivalent to the rest of either or both the bodies, or that their motion may be destroyed by acting on each other. Motion cannot be destroyed by the destruction of other motion. Whenever the motion of one body is destroyed, motion of one kind or another is *produced* in other bodies.

Take, for instance, two equal inelastic bodies moving towards each other with equal velocities. When they strike, they may apparently destroy each other's motion, but in fact the motion of neither is destroyed except by conversion into motion of another kind, viz: heat. And if all the work which could be performed by this heat could be applied to the double mass in one direction, it would give it a velocity just equal to that of either body before impact. Besides, there are many ways in which the direction of motion may be changed without material loss of velocity. Suppose the mass M in the above example after the recoil to strike perpendicularly a fixed elastic spring. It will be thrown back again in the direction of its first motion with a little loss of velocity. And then even the algebraic momentum of the two balls would have been increased by more than half the momentum of M before impact. And yet the velocity and momentum of both the balls in the same direction would be wholly due to the primitive velocity of the first ball. Or the direction of the motion of M may be reversed by allowing the ball to describe a semi-circular arc in a rigid groove, or to ascend an inclined plane till the force of gravity causes it to return. Or, if we suppose the bodies to be moving horizontally, their motion might be applied, by means of pulleys or otherwise, so as to raise vertically just as great a weight and to just as great a height as though they were moving in the same direction. Has the mass M then less than no motion at all?

It would seem impossible to get clear of confusion in this subject, without discarding entirely the expression *quantity of motion*, and confining the term *momentum* to its exact meaning. The definition of momentum as a *quantity of motion* can only lead to absurdity, as we have already seen. But that definition is so widely accepted, and supported by such high authority that it may be worth while to sift it a little further.

Take the case considered above, the mass $M=1$, its velocity $=1{,}000$, and suppose that its quantity of motion is then equal to 1,000 of something, (who can tell what?) It should seem evident that a body cannot impart to another body more of a quantity of motion than it possesses itself. But yet, if the quantity of motion of M be equal to 1,000, we must allow that it imparts to M' a quantity of motion equal to $1{,}441\frac{2}{3}$; and this is absurd.

Again, if two bodies have no motion they must be at rest. But suppose we have two equal bodies M and M, moving in opposite directions with equal velocities V and V. Then if MV equals the actual quantity of motion of each, and if a quantity of motion in one direction destroys an equal quantity in the other direction, the total quantity of motion in the two bodies is $MV-MV=0$. Therefore the bodies have no motion, and hence must be at rest. But this is false, since they were supposed to be in motion.

This argument may be absurd, but we are inevitably led into all sorts of such absurdities if we attempt to consider momentum as a quantity of motion, or anything but what it really is. But with the correct understanding everything is perfectly clear and simple.

In the example we have now considered, one of the bodies was supposed to be at rest; but aside from that there was nothing peculiar about the conditions. And we found that after impact the arithmetical difference between the momenta, or, if you please (since the balls would be moving in opposite directions), the algebraic sum of the momenta of the two balls, was just equal to the momentum of the mass M before impact. Hence we might infer in general, that whatever the elasticity or masses or velocities of the bodies, the algebraic sum of the momenta after impact would be equal to that before impact; or that if the two bodies were moving before impact in the same direction, and after impact in opposite directions, the arithmetical difference of their momenta after impact would be equal to the sum of their momenta before impact. But we may give a general demonstration of this, which will not only prove it true, but will also show us why it should be true.

The masses of the two bodies being M and M', their respective velocities before impact being U and U', and after impact V and V', the change of velocity which the mass M experiences by the impact is $(U-V)$; and the change produced in the velocity of M' is $(V'-U')$. Now whatever the relative masses or velocities, or the degree of elasticity of the bodies, or whatever heat or molecular motion may be developed in the impact, the total resultant pressure on M is at every instant through the whole time of the impact precisely equal to the opposite resultant pressure which is exerted on M'. The changes of velocity of the two bodies will therefore be to each other inversely as the masses. We thus get the proportion

$$M : M' :: (V'-U') : (U-V);$$

whence

$$M'(V'-U')=M(U-V);$$

from which we have

$$MV+M'V'=MU+M'U';$$

in which the two members represent the algebraic sums of the momenta respectively after and before impact.

This last equation is derived in some works on Physics, by transforming and combining the general values for V and V' after impact. And upon the evidence of this equation the assertion is founded, that *by the impact of bodies, whether elastic or otherwise, no motion is lost.* But the equation does not prove that by any means. It simply proves that the algebraic sums of the momenta before and after impact are equal. And even that must be taken with a clear view of the circumstances of the case. For instance, in the example already solved, we found that the mass M would have a velocity after impact of $441\frac{2}{3}$ feet per second in the direction opposite to its previous motion. You may say, if you please, that it has a negative momentum equal to 441⅔ pounds, but you must not understand that this actually destroys an equal part of the momentum of M'; for the bodies are supposed to have no connection with each other after impact. All we can say is that if the momentum of M *could* be applied so as to destroy an equal part of the momentum of M', the remaining momentum of M' would be equal to the momentum of M before impact. But this does not prove that there is no motion lost. For in our example the bodies are partially inelastic, and in the impact there must have been more or less change from ordinary motion to heat; and we shall find further on, that the heat developed by this case of impact, if equally distributed through the two balls, whose combined weight is 193 pounds, would raise the temperature of the whole mass 11.4784 degrees Fahrenheit. Has there then been no destruction of motion? True enough, there may not have been, if you consider this heat as motion. But then, the momentum after impact entirely neglects this motion, and yet comes out algebraically equal to the momentum before impact, and arithmetically much greater still. Can it then in any way represent motion?

If, however, we make momentum what it is, there is no difficulty in seeing why it should in every case be algebraically equal before and after impact, and why in some cases it may be arithmetically much greater after than before. For it follows from the continual equality and opposition of the resultant pressures on the two bodies for the same length of time, that the changes of velocity are inversely as the masses, and thus that the algebraic addition to the momentum of the one is equal to the algebraic subtraction from the momentum of the other, and hence the algebraic sum of the momenta cannot be altered by the impact, whether the bodies are elastic or not. But if, as in the above example, the degree of elasticity and the relative velocities and masses of the bodies are such that in the second period of impact the force of restitution entirely destroys the velocity of M in its former direction and gives it a new velocity in the opposite direction, then the algebraic sum of the momenta after impact means the arithmetical difference of those momenta; and of course the actual or arithmetical sum of the momenta after impact must therefore in such case be greater than that before impact, by just twice the actual momentum of M in its new direction.

We need not be afraid of the fact that it is possible to have an actual increase of momentum by impact. For we found that with perfectly inelastic substances, whatever change there might be from ordinary motion to heat, the momentum would be the same after impact as before. Now if a very small mass with a high velocity impinge on a very large mass at rest, when their velocity becomes common it will be very small; and then if the bodies are elastic and the force of restitution operates to separate the bodies and change molecular motion back into ordinary motion, at the instant the velocity of the small mass becomes zero the momentum of the large mass will be equal to that of the small mass before impact; then if the force of restitution has not finished its work a new velocity will be given to the small mass in the opposite direction, and the momentum of the large mass will be still further increased by exactly the momentum given to the small mass in the new direction.

It may also help us to see how actual momentum could be increased by impact, to consider the following proposition, viz: *Of two unequal masses moving with such unequal velocities that the vis viva of the one is equal to the vis viva of the other, the momentum of the one which has the less velocity is the greater.* This may easily be demonstrated generally, but it will be sufficient to consider a single example. Let the mass M have a velocity of 1,000 feet per second, and let M'' be another larger mass such that when moving with a velocity of 500 feet per second its vis viva, or the work which it can perform, shall be equal to the vis viva of M. The momentum of each mass is the constant pressure which can bring it to rest in one second. But if the mass M be brought to rest by a constant pressure in one second, we know from the laws of uniformly retarded motion that it will move through 500 feet in coming to rest. And the mass M'' in coming to rest in the same way would move through 250 feet. Therefore if the mass M'' performs the same amount of work in passing through 250 feet, as the mass M performs in moving 500 feet, the intensity of the pressure exerted by M'', or its momentum, must be twice that of the mass M. The same may be shown by comparing the expressions for the momenta of the two bodies; for under the supposition M'' would be four times as great as M, with half its velocity.

If then, all the power of a moving body could be applied to give a larger mass a less velocity, with no loss of vis viva from the development of molecular motion, the momentum of the larger mass would very properly be greater than that of the mass from which it was derived. But from the nature of the case, in the impact of two bodies, whether elastic or not, whatever molecular motion is once developed cannot be re-converted into ordinary motion without the bodies can re-act on each other so as to be driven apart and move relatively in opposite directions. Thus the algebraic momentum cannot be increased by the impact, although in many cases, as in our example, a body at rest may acquire from a moving body a greater momentum than that of the moving body itself, and thus the actual arithmetical momentum of the two may be very much larger after than before impact.

Let us now resume the case of the two cast-iron balls, and calculate the vis viva of each after impact. The vis viva of M will be

$$\tfrac{1}{2}\times(-441\tfrac{2}{3})^2=97{,}534.722 \text{ foot-pounds.}$$

The vis viva of M' will be

$$\tfrac{1}{2}\times5\times(288\tfrac{1}{3})^2=207{,}840.278 \text{ foot-pounds.}$$

Now we could with just as good reason say that the actual total of the vis viva of the two balls after impact is equal to the arithmetical difference between these values, as that the actual total of the momentum is equal to the arithmetical difference of the momenta; for the work represented by the vis viva will be performed in the same direction in which the momentum would be exerted. But it so happens, by squaring the velocity of M, that whether we consider that velocity positive or negative, the vis viva of M is algebraically positive. In treating of momentum and vis viva, then, in connection with impact, a negative sign of the velocity in the result should be regarded simply as showing the direction of motion, and should not be blindly taken as indicating destruction of motion, or anythind of the kind.

Remembering, then, that after impact, the two balls move in opposite directions, we may still say that the total vis viva, or work which they can perform, is equal to the arithmetical sum of the two parts, or 305,375 foot-pounds. Now the vis viva of the mass M before impact was

$$\tfrac{1}{2}MU^2=\tfrac{1}{2}(1{,}000)^2=500{,}000 \text{ foot-pounds.}$$

Hence the loss of vis viva by the impact, or the work employed in the development of heat is 194,625 foot-pounds, or considerably more than one-third of the whole vis viva of M before impact.

To determine the increase of temperature of the two balls, supposing the heat developed were uniformly distributed through their mass, we have the weight of the two masses given $=193$ pounds, the mechanical work consumed $=194{,}625$ foot-pounds, and the specific heat of iron $=0.1138$. Now the expenditure of 772 foot-pounds of work in developing heat is capable of raising the temperature of one pound of water one degree Fahrenheit; hence the same work will raise the temperature of one pound of iron $\frac{1}{0.1138}$ degrees, or of 193 pounds of iron $\frac{1}{193\times0.1138}$ degrees. Therefore the rise of temperature of the two balls, from the consumption of 194,625 foot-pounds of work would be

$$\frac{194{,}625}{772\times193\times0.1138}=11.4784 \text{ degrees.}$$

To recapitulate, then, the accumulated energy of a mass of iron weighing 32 1-6 pounds, and containing therefore 1 unit of mass, moving with the velocity of 1,000 feet per second, and having accordingly a momentum equal to 1,000 pounds, would be capable of imparting to a mass weighing 160 5-6 pounds such a velocity that its momentum should be $1{,}441\tfrac{2}{3}$ pounds, and of giving to the first mass a velocity in the opposite direction such that its momentum should be equal to 441$\frac{2}{3}$ pounds, besides developing heat enough to raise the temperature of 193 pounds of iron through about 11$\frac{1}{2}$ degrees Fahrenheit.

One or two general conclusions, commonly unnoticed in the books, still remain to be considered. We found in the special example above that after impact the total *vis viva*, both arithmetical and algebraic, was less than before. And this would be found true in every case of impact unless the bodies were perfectly elastic. But if we had supposed the balls perfectly elastic, making $E=1$, we should have found after impact, $V=-666\frac{2}{3}$. $V'=333\frac{1}{3}$, and $\frac{1}{2}MV^2+\frac{1}{2}M'V'^2=500{,}000$, which is equal to the vis viva before impact. And if, in the general equations for the values of V and V' after impact, we make $E=1$, square each of the equations, and multiply both members of the first resulting equation by $\frac{1}{2}M$, and both members of the second by $\frac{1}{2}M'$, then add these equations member to member, and reduce, we shall find this equation,

$$\tfrac{1}{2}MV^2+\tfrac{1}{2}M'V'^2=\tfrac{1}{2}MU^2+\tfrac{1}{2}M'U'^2;$$

which proves that *in every supposable case of direct impact of two perfectly elastic bodies the total vis viva after impact would be equal to the total vis viva before impact.* There would then be no heat remaining as the result of impact.

In the second section it was found that when one body in motion strikes another body at rest there is more "work" performed in reducing the velocity of the former than in imparting velocity to the latter; otherwise there would be no loss of vis viva by impact. This loss of vis viva was accounted for by the equivalent of heat developed. But the question then arises: If the total resultant pressure which reduces the velocity of one body is continually equal to the total resultant pressure which increases the velocity of the other body, the two bodies being in contact, how can there be more work performed in the one case than in the other? The explanation, however, is not difficult. For we have seen that in all cases of impact the bodies are compressed; and this compression will bring the centers of gravity of the two bodies closer together at the moment of greatest compression than at the first instant of contact. If then a body in motion strikes a body at rest, or moving more slowly in the same direction, during the period from the instant of contact to that of greatest compression, the foremost as a mass, or its center of gravity, must move through a less distance than the mass, or center of gravity, of the other. Therefore, since the resultant pressures which change the velocities of the masses are constantly equal, but exerted through different distances, it is a necessary result that the quantities of work performed are different. If now the bodies are elastic, and the ratio of their masses within certain limits, during the second period of impact the case will be reversed and more work will be performed in imparting velocity to the foremost body than in retarding the other, since by the separation of their centers of gravity the mass of the former moves through a greater distance than that of the latter.

A consideration of the relative distances passed through by the centers of gravity of the masses leads to a rather interesting result, as follows:

Assume the conditions and notation adopted at the beginning of the third section, and we have the proportion

$$M : M' :: (X-U') : (U-X),$$

whence,

$$M(U-X)=M'(X-U).$$

Multiplying both members by

$$\tfrac{1}{2}(U+X)(X+U')$$

we obtain

$$\tfrac{1}{2}M(U^2-X^2)(X+U') \\ =\tfrac{1}{2}M'(X^2-U'^2)(U+X),$$

whence the proportion

$$\tfrac{1}{2}M(U^2-X^2) : \tfrac{1}{2}M'(X^2-U'^2) \\ :: (U+X) : X+U').$$

The first term of this proportion represents the number of units of work required to reduce the velocity of M from U to X, and the second term the work required to increase the velocity of M' from U' to X. Now if we let D represent the distance passed through by the center of gravity of the mass M while suffering the reduction of velocity from U to X, and D' the distance passed through by the center of gravity of M' while having its velocity increased from U' to X, then, since the work employed in producing these changes of velocity is performed by resultant pressures constantly equal, the quantities of work will be to each other as the distances; thus giving the proportion

$$\tfrac{1}{2}M(U^2-X^2) : \tfrac{1}{2}M'(X^2-U'^2) :: D : D'.$$

Combining this proportion with the last one obtained above, we have,

$$D' : D :: (U+X) : (X+U');$$

or, if T represents the time from the instant of contact to that of greatest compression,

$$D : D' :: \tfrac{1}{2}T(U+X) : \tfrac{1}{2}T(X+U').$$

Now $\frac{1}{2}T(U+X)$ is the distance through which the center of gravity of the mass M would pass in the time T, if its velocity were reduced with a perfectly uniform retardation, from U to X; and $\frac{1}{2}T(X+U')$ is the distance through which the center of gravity of M' would move in the time T, if its acceleration from U' to X were also perfectly uniform. The actual distances D and D' passed through by the centers of gravity of the two bodies in the time T are therefore to each other in the same ratio as if the center of gravity of the mass M were uniformly retarded and that of M' uniformly accelerated during the time T.

Before concluding this discussion perhaps a word or two further should be said as to the product $\frac{1}{2}MV^2$, to which the name *vis viva* has been given. Some authors apply the expression *vis viva* to the product MV^2, or to *twice* the quantity of work which a moving body can perform. Now we might of course, if we chose, by common consent, give the name *vis viva* to the product MV^2; but if we consider the meaning of the name, (*living force*), there would seem to be much better reason for applying it to $\frac{1}{2}MV^2$, which is the *actual* quantity of work the body can perform by parting with all its velocity. This application of the name does not alter the fact that the vis viva *varies* as the mass into the square of the velocity. Besides, we seem to have the best authority of all, that of the originator of the name, for adopting this use of it. Professor Cooke, (Chem. Phys., p. 53), states that the product "$\frac{1}{2}MV^2$ was named by Leibnitz *vis viva*, or *living force*;" and the fitness of this designation has been already sufficiently shown.

We may now take a brief review of the claims of momentum to be considered the *quantity of motion* of a moving body, and also of the true claims of *vis viva.* We have seen, first, that the unit of momentum is simply a pound pressure, a unit which does not include the idea of motion; whereas the unit of vis viva is a pound pressure exerted through a distance of one foot, a unit which does include the idea of motion. Secondly, we have seen that momentum is not an effect that a moving body *necessarily* produces, but is only a special result under arbitrary conditions, generally impossible to fulfill; whereas the vis viva represents a definite stored up energy which *must* be put forth under whatever circumstances the motion of a body is destroyed. Also, if we would make a proper distinction between the *living force* of a moving body, or the work it can perform, and the *striking force,* understanding by this the mean intensity of pressure the body would exert when brought to rest in any distance, the momentum would be merely one case of the striking force, found by dividing the vis viva, or $\frac{1}{2}MV^2$, by $\frac{1}{2}V$, or the distance the body would move if brought to rest by a constant force in one second. Thirdly, we have seen in case of impact, that however great the change may be from ordinary motion to molecular motion, momentum is never diminished, and in many cases may be actually increased; and these conditions could not obtain if momentum in any way represented ordinary motion; whereas if the bodies are in the least degree inelastic, so that there is any change from ordinary motion to molecular motion, vis viva is correspondingly diminished, indicating its close connection with ordinary motion; and vis viva can only remain undiminished by impact when there is no permanent change from ordinary to molecular motion. Finally, we have seen that the attempt to consider momentum as a quantity of motion is unphilosophical, unnecessary and pernicious, and leads to manifold absurdities; whereas, *vis viva*, or $\frac{1}{2}MV^2$, truly represents a moving body's accumulated or potential energy for moving against resistance, or producing motion in other bodies by parting with velocity of its own.

ON THE PRIMING OF STEAM BOILERS.*

By Mr. WILLIAM MAJOR.

From "The Engineer."

It is certainly humiliating for the profession to be obliged to admit that one hundred years after the introduction of the steam engine they are still unable to subdue an evil so generally experienced as the priming of steam boilers; yet such is the case, as proved by the Serapis. The first question to be solved is: What is the cause of priming in a steam boiler? and until this question is satisfactorily answered engineers will still go on devising mechanical contrivances for obviating it, and their efforts will end in as many signal failures as hitherto. If we ask a practical engineer what the cause of priming is, we generally have dirty water assigned as a reason, and a specific is given for its prevention; or, what is more usual since the more general introduction of high pressure steam boilers, some remarks not very complimentary to the constructor of the boilers of which he may have charge and the priming of which he is unable to master. If we ask the same question of a scientific man—a professor of chemistry for example—he will answer that priming is ebullition caused by the action of heat on the body of water contained in the boiler, but he is not quite certain as to whether this so-called ebullition is a chemical or a mechanical action. With this meagre information the profession has had to content itself, enduring at the same time all the evils attendant thereon.

In the case of the Serapis we have a ship engined with a power sufficient to propel her through the water with a speed of 13 knots an hour, but that engine power was so paralysed through the priming of the boilers that her speed was reduced to 9 knots. The case of the Serapis is by no means exceptional, the same evil obtains in a greater or less degree on board every steamship afloat, and the introduction of high pressure steam together with the surface condenser has very considerably increased the evil. Indeed, break-downs from large bodies of water passing with the steam from the boilers to the cylinders of the engine are daily occurrences, and more valuable steam engines are destroyed from this cause than from all other causes put together. The author need not enlarge further on the subject of priming and its consequences, but will observe that he feels assured of having discovered a better theory for the cause of priming in steam boilers than is yet generally known. He has also found a practical means of preventing the evil, and that too without putting shipowners to any expense, but on the contrary, since to stop priming is to save fuel. The author does not propose any costly or complicated machinery for the purpose of preventing priming, not even a wooden deck placed in the boiler, as it was stated was adopted in the Serapis' boilers. As before stated, the cause of priming is said to be ebullition in the body of water contained in the boiler caused by the heat acting on the water. The author is bold enough to alter the word "ebullition" into the more intelligible word "friction," and he ventures to assert that priming in steam boilers is—as nature's simple law ought long ago to have taught us—caused by friction on the outer surfaces of the steam globules as they pass up through the overlaying body of water into the steam chest, the amount of such friction being always in due ratio to the velocity at which they pass through the body of water, and also depending upon the state of the water as regards its purity. In other words, the friction is least when the water is pure—distilled—while it is the greatest when the water is charged with all the organic and inorganic matter it can hold in solution. The author is only now, after four years practical experience on a large scale, publicly making the above assertion, and with the full conviction of its correctness.

Having satisfied himself as to the feasibility of the friction theory, it occurred to him that we had mistaken the real effects or action of the old-fashioned

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* A Paper read before the Society of Engineers.

system of injecting melted tallow into boilers for stopping their priming. The reason generally given why tallow or other fatty matter stops priming is because it spreads itself over the surface of the water and quiets it. The author, however, does not believe this is the case, since whenever there is a pressure of steam in a boiler, any fatty matter injected into it immediately diffuses itself throughout the whole contents of the boiler, both water and steam. That this is the case can be proved by simply drawing a glass of water from any part or height of a boiler charged with tallow or by condensing a portion of the steam. Both the taste and smell of tallow will be found in both cases. The author believes that fatty matter stops priming by lubricating the globules of steam as they pass through the overlaying body of water, reducing the friction on their surfaces, and, as a natural consequence, causing them to carry less water with them into the steam chest. So long as we continue to supply a boiler with a sufficient quantity of fatty matter, priming will not take place. But it so happens that we cannot use the quantity of tallow, nor indeed of any other ordinary fatty matter necessary for the purpose, without producing other evils which it is difficult, or even impossible, to contend with, at the same time the expense of their use would be greater than most shipowners would like to bear. Tallow contains so much insoluble matter, that if used in the required quantity it would soon choke the boiler up, while both animal and vegetable oils contain so much gummy or resinous matter they would soon cause evil effects on the interior of boilers if large quantities were used. Of all the patent compositions the author has seen or heard of, he has found none that will answer the purpose —at any rate neither for marine nor for locomotive purposes, simply because the chief cause of priming—impurities in the water—is never absent. Therefore what is required is a constant supply of the proper lubricating matter to prevent the baneful action of these impurities on the globules of steam as they are generated. For marine boilers in particular, too great a stress cannot possibly be laid on the simple fact that the cause of priming is never absent so long as the boiler is in use, or supplying the engine with steam, because the water pumped into the boilers always contains either organic or inorganic matter. And this is the case whether the engines are or are not fitted with surface condensers. If the engine is fitted with surface condensers, priming will in most cases be more severe than with the injection condenser, because the action of heat on the metal plates of the boiler is more intense where there is little or no lime on the plates, as with a boiler fed from a surface condenser, than from an injection condenser, where the water is constantly being changed, leaving all its organic and inorganic contents, with the exception of common salt, in the boiler. Consequently a constant supply of fatty matter is absolutely necessary to counteract the friction caused by these impurities.

The author has tried all the oils in common use, together with most of the patent compositions, for the purpose of preventing priming. He has always succeeded in the object sought, but has found them all impracticable when used in large quantities, or constantly supplied to a boiler. The only exception to this rule has been the application of petroleum. The purest rectified petroleum has not only prevented all priming, but has prevented corrosion and removed incrustations arising both from organic and inorganic impurities, and from oxidation of metals as regards the interior of the boilers, as well as of the engines themselves. It would, indeed, appear that by using sufficient petroleum to lubricate the steam globules as they are generated, the preservation of the steam boiler and engine is insured so far as their interiors are concerned. The author has not found that petroleum, pumped into the boilers with the feed-water, has had any injurious effect on india-rubber valves in or about the air pumps, though he has used it in large quantities for several months in succession. In the feed pumps, however, where petroleum, nearly in its pure state, comes in direct contact with the valves, india-rubber cannot be used for them. The advantages attained by the use of sufficient petroleum to prevent the priming of steam boilers have been found by the author to be:

(1) The prevention of priming, or, in

other words, preventing large bodies of water from passing over with the volume of steam from the boiler to the engine.

(2) The prevention of the injurious effects produced by water passing with the steam into the cylinders of the engine.

(3) A great reduction in the quantity of feed-water necessary to produce the required volume of steam, or, in other words, an augmented boiler steaming power in proportion to the smaller quantity of water required to produce the steam required, or a saving of fuel proportionate to the smaller quantity of water necessary for producing a given volume of steam, together with the reduced power required for lifting water for condensing dry steam as against that necessary for the condensation of saturated steam. To these must be added the saving of the power absorbed in overcoming the resistance of water in the ends of the cylinders when working with saturated steam.

(4) Augmented boiler steaming power when working with injection condenser, by the total absence of incrustation or corrosion.

(5) Prevention of rust, corrosion on the interior of the engine, together with the advantage of keeping the interiors of surface condensers perfectly clean without any other help.

(6) Its instantaneous action on any disturbance in the water in the boilers, which is one of its greatest advantages.

(7) The increased durability of both boiler and engines.

The disadvantage of using petroleum in an engine-room—for there is one disadvantage—is that petroleum is a combustible article which requires care in use. It ignites at a lower temperature than common oils, but with ordinary care, and keeping it out of contact with cotton or woolen stuffs the danger of its use in engine rooms is reduced to the same extent as that of its use in dwellings. It is, indeed, reduced to a far greater extent when it is stored in a fixed iron tank with but one opening on top for filling and connecting a suction pipe to the feed-pump of the engine. The danger is then reduced to the bringing of it to the tank. Petroleum will not ignite or burn in a closed vessel; a bar of red hot iron inserted into a closed cistern full of petroleum will not ignite it, because there is not sufficient atmospheric air for combustion. The author knows of no other danger or disadvantage in its use, and when this one disadvantage is put against the many advantages, and when it is considered that its use prevents marine engines from becoming paralysed, as but too often is the case at most critical moments, no practical engineer will, it is thought, differ from the author in the opinion that it will be wise to promote its general use.

In applying petroleum to boilers the author has constructed a combined valve and cock. This contrivance is screwed into the valve box of the feed pump between the suction and delivery valves, the safety cistern containing the petroleum, being placed in a convenient part of the engine-room. As the connection pipe is inserted through the top of the cistern, while no other cocks or connections on cistern are allowed, no danger can ever arise from inadvertence on the part of the men in the engine-room. This valve and cock is all that is required in addition to what is generally found in well-constructed steam engines—namely, a cock on the suction pipe, or a screw on the top of the suction valve of the feed pump, one of these being required for regulating the flow of water to the pump necessary for generating the volume of steam required, and the prevention of waste of petroleum, since any overplus of water the pump takes, and which does not pass into the boiler, is returned through the safety valve on the pump. The author has in practice regulated the water flow to the pump, so as to approach as nearly as possible the consumption in the boilers, leaving the feed cocks full open. This has given two advantages: First, no petroleum was wasted; and, secondly, it has directly shown when the steam was taking an undue quantity of water with it. That it is in the boiler itself that priming must be prevented appears evident, and it is equally evident to the author that all the mechanical devices for the purpose of preventing it have, for the want of the proper knowledge of the cause of priming, signally failed. Some of these devices—the superheater, for example, which is still in general use—are very complicated, and consequently very expensive.

The valve and cock, applied to insure instantaneous action, must have the necessary dimensions in proportion to heating surface in boilers it is intended to supply with petroleum. On starting the engines, or when forcing the fires, priming is generally most violent, and at such times the engineer's immediate attention is necessary, and he will act wisely in giving his boilers a little petroleum previous to forcing the fires. On a voyage from Copenhagen to St. Petersburg and back, with his Danish Majesty's steam yacht Slesvig, last summer, nearly 144 hours' steaming, the author consumed nearly forty gallons of petroleum, and this gave a gain of twenty-five tons of coal, as compared with what the same boilers and engines had previously consumed on the same journey in about an equal number of hours, namely 150 instead of 175 tons of coal. The ship, however, was drawing 2 inches more water last year than formerly, which increased her immersed midship section from 219 to 224.7 square feet, or $4\frac{3}{4}$ square feet nearly. There was, besides, the disadvantage of being in company with a screw frigate, whose sailing powers far exceed in speed the Slesvig's steaming powers, so that the author was constantly either forcing or easing the engines to keep in position with the frigate, which caused a considerable waste of fuel. The Slesvig has a pair of Robert Napier's old-fashioned side-lever engines with open condensers, and of 240 nominal horse-power, but which the author is now enabled to work up to 620 indicated horse-power. With the same draught of water as now, about 9 feet 4 inches, her speed formerly never exceeded ten and a-quarter knots, and this speed was with difficulty maintained for any length of time, for the want of sufficient steam. Since the application of petroleum to the boilers, however, the speed has been increased to eleven knots, and this without any increase in the consumption of coal. The steaming power of the boilers has been increased so much that there is now more steam than the engines consume, while the engines themselves are working with less noise than formerly.

The author succeeded last summer in removing all corrosion and incrustation from the interior of the Slesvig's boilers, and they are now as clean as when they left the boiler maker's hands ten years ago. Petroleum dissolves both the lime incrustations and prevents rust corrosions when under steam pressure, while all the dissolved muddy matter can be blown out from the bottom of the boiler with the ordinary blow-off cock; the boiler requiring no other cleaning out, and consequently there are no stoppages for this purpose. It may be interesting if, before concluding his paper, the author places on record a circumstance which came under his observation last year respecting marine boilers in connection with surface condensers and the use of mineral oils.

When the surface condenser was introduced in connection with marine engines one of the advantages expected to be derived from it was the freeing boilers from incrustation, and thus increasing the evaporating powers of the boilers; the danger of collapse arising from overheated fire-box plates was to be removed. The surface condenser was intended to return to the boiler the water taken therefrom in the form of steam, and as this water could contain no organic or inorganic or earthy particles, it was very natural to suppose that no incrustation could be formed in boilers supplied with them. But one of the first discoveries made after the introduction of the surface condenser was that the boilers supplied from it corroded them to an alarming extent, so that in many instances such boilers were entirely destroyed after only two years' service. This circumstance prevented their coming into general use until about twelve or fourteen years ago, when it was discovered that the corrosive action was caused by metallic particles taken from the interior of the engine by the steam on its passage through it, and carried into the boiler, where they produced a galvanic action which oxidized the plates of the boiler. About the same time it was also discovered that sea-water contained the best non-conducting agent for preventing a galvanic action in the interior of the boilers, namely, its organic and inorganic contents. In fact, the same matters which had hitherto caused incrustation were now found necessary, in smaller quantities, to protect the boilers against oxidation. This protection is afforded by changing the water in the boiler occasionally with water direct from the sea, which leaves a coating of its organic and inorganic contents on the interior surfaces of the boiler, and thereby insures safety from galvanic action, and permits the general use of the surface condenser with marine engines. We have, however, for several years past heard of the collapsing of fire-box plates where they have been in connection with surface condensers, and the opinion invariably given as to the cause of these accidents has been that there was either a want of strength in the construction to assist the steam pressure, or that there had been carelessness on the part of the attendants in not supplying the boiler with sufficient water. This, however, is a great mistake, as there are other causes equally as active in causing fire-box plates to collapse as those just named. One special cause came under the author's observation. About midsummer last year the screw steamship Conatio, of Flensborg, on her way from Cronstadt to an English port, was towed into Copenhagen harbor in a disabled state. The four fire-boxes of the boiler had collapsed. They appeared to have all given way at about the same time. The fireman, on opening the fire-doors, saw the crowns of the fire-boxes sinking, and the engineer immediately opened the smoke-box doors, and took other precautions for reducing the steam pressure, and thereby in all probability saved his own life and the lives of the rest of the engine-room crew. Upon reaching the port the boilers were examined. There was plenty of water in them, and everything else appeared to be in perfect order; yet, on emptying the boiler, the injured crown plates of all four fire-boxes afforded clear evidence of overheating—in fact, they had been red-hot, and this notwithstanding that there had been plenty of water in the boiler, and that there was but a trifling deposit. The only reasonable conjecture as to the cause of the accident was that on the collapsed plates was found a thin layer of ordinary boiler incrustation, not more than a twelfth of an inch thick, mixed with a dark substance, which proved on examination to be the insoluble residue of a mineral oil, which had been used for the purpose of lubricating the interior of the engines. The lubricator used was fixed on the side jacket of the high-pressure cylinder, and from thence the oil passed through the surface condenser to the boiler. This insoluble matter then mixed with the slight incrustation, and which, thin as it was, had been sufficient to make a perfectly impervious coating, which had prevented the contact of the water with the iron plates. Hence the overheating and collapsing of the crowns of the fire-boxes.

In the month of September last the author was requested by the Danish Board of Trade to survey and report upon the condition of a boiler on board a merchant steamship, the Bergen Huns of Copenhagen. This vessel came into port at Copenhagen with a boiler which had been disabled on the voyage from Norway to Stettin. Here was a similar case to that of the Conatio, only perhaps a little more decisive as regards the cause of the accident. The Bergen Huns is fitted with compound engines supplied with steam from a cylindrical boiler having two cylindrical fire-boxes in it, one of which collapsed. The fire-box collapsed eight inches in towards its center over a length of four feet six inches from the back end, not, as might be supposed, along a line drawn perpendicularly through its center, but at an angle of forty-five deg. with it towards the side of the ship. In this case the engineer, seeing there was no want of water in the boiler, ordered the fire to be drawn out of the collapsed fire-box and proceeded with the other fire on his voyage, and came in to Copenhagen some twenty hours afterwards. Such a piece of dangerous stupidity succeeded in his case, but has probably failed in too many others under such circumstances.

On examining the interior of this boiler the author found that half of it—namely, that side on which the feed-cock was fixed—was coated with a substance resembling black varnish or coal tar, having a thickness of about three-sixteenths of an inch and being perfectly impervious to water. This deposit had the appearance of having been laid on with a brush, so equally was it spread, particularly over the fire-box. It was under this coating of impervious matter the fire-box plate had collapsed, while the other fire-box remained uninjured, notwithstanding that it had a coat of ordinary boiler incrustation $\frac{1}{4}$ inch thick. The incrustation on this side retained its ordinary grey color. On inquiring of the engineer from whence this black substance came, he stated that it was produced from the oil used to lubricate the slide valves with. He observed that he was obliged to use a great deal of the oil to keep the valves from making a noise and cutting their faces. The oil used for this purpose was a half crude mineral oil, which still retained so much of its original earthy bituminous constituents, that the latter, combining together with the organic and inorganic matter contained in the boiler, made a perfectly impervious cement which had prevented the contact of the water and iron, and being a bad conductor of heat, caused the iron to be over-heated, and thereby weakened, the steam pressure causing the collapse. The author laid a piece of this compound on a piece of iron heated to a little over red heat, which had, however, produced no further effect than the burning of a little of the bituminous matter out of it. The collapsed plate bore evidence of having been overheated, and it was only owing to the superior quality of the iron, of which the fire-boxes here as well as in the case of the Conatio were constructed, that a most serious loss of life had not taken place.

The reason why only one half of the boiler was coated with the bituminous matter, the author attributes to the circumstance that the injured side of the boiler had on it the feed-cock, and that as the oil, passing through the latter, came first in contact with the incrustation on that half, it was absorbed before it could reach the other side of the boiler. The uninjured fire-box, however, was coated with an incrustation $\frac{1}{4}$ inch thick over the whole of it. Now this thickness of incrustation ought never to be found in a boiler which is in connection with a surface condenser, and the fact of its being there clearly afforded evidence of the engineer's ignorance both of the use of the surface condenser and of the meaning of a trifling incrustation to preserve his boiler from galvanic action and oxidation. But, as seen in the case of the Conatio, the danger is not dependent alone upon the thickness of the coating of incrustation, but chiefly on its porosity. Let the incrustation be never so thin, a bituminous substance is added to it, which binds it together so that it becomes elastic and impervious to water; the iron plate will overheat and collapse under pressure. Hence, wherever oils containing bituminous matter are allowed to pass into a boiler, it is only a question of time as to when the incrustation becomes impervious, and the fire-box under it collapses.

The author was much surprised that the uninjured fire-box in the Bergen Huns boiler had not collapsed also, more particularly as the incrustation on it was spread very evenly and closely. In boilers working in connection with open injector condensers, incrustation attains a considerablé thickness in places where it cannot easily fall off. But on a cylindrical fire-box it would undoubtedly never have laid so compactly as was the case here; it would have been broken and in uneven patches. It appeared evident that some of the oil had also reached the uninjured side of the boiler, and had acted on the incrustation there. The author, therefore, would recommend precautions being taken so as not to allow these oils to enter steam boilers. There is no necessity for their use at all; a little pure rectified petroleum pumped with the feed-water into the boiler will prevent such accidents as the collapsing of fire-boxes. It will be sufficient to lubricate the internal parts of the engines, and, at the same time, will keep the surface condensers perfectly clean.

Looking at the foregoing facts, the author thinks it will not be difficult to account for the disappearance of many missing steamships. At any rate, a careful investigation into the cause of this class of accidents is a matter of the greatest importance, and the author hopes the remarks he has made may have the effect of drawing attention to the subject.

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Californian Ironstone.—An effort is now being made by a number of enterprising American gentlemen to open up vast deposits of ironstone, which are said to exist a few miles above Auburn, on the line of the Central Pacific Railroad.

MODERN EDUCATION.

By ALFRED P. BOLLER, C. E.

Commencement Address to Graduating Class of '77 at Rensselaer Polytechnic Institute.

Once more, in the sequence of events, the season of the year has arrived when the higher educational institutions throughout the land are all aglow with the excitement of "commencement day," while fathers and mothers await the hour, with proud satisfaction, when the coveted parchment, certifying to all manner of learning, will have been formally bestowed upon their representatives in the rising generation. Thenceforth, the diploma becomes a family heirloom, to be carefully stowed away in its japanned tin case among the family archives, or perchance hung upon the wall of the family sitting-room in a handsome frame, a daily reminder of what "our boy" has accomplished. Amid all the changes that have taken place in our educational systems, the practice of a public introduction to the world, of the completed product of such schools as confer degrees, is one that even the most radical iconoclast has not attacked. The commencement season is no meaningless formality, but an important epoch in the life of each generation as it passes along. It is at once the beginning and ending of a never-to-be-forgotten period of one's life. From the controlling influence of home and the instructor, the young man steps at this season into the arena of worldly strife, and thenceforth must bear the responsibilities of his own actions. Further than this, every graduating class adds just so many units to the world's educated intellectual forces, out of the clashing of which are evolved those ideas upon which the progress of humanity depends. Commencement day is therefore not alone a time of public congratulation for the honorable completion of a task, but a solemn occasion to signal the entering upon a new life of a class of young men, to whom is given a very important part to play in their country's development. As such it will always have an interest for society, the members of which will gather on all commencement occasions, year after year, as they have always done, not only paying a cheerful tribute to intellectual success, but also warmed up by that great under current of human sympathy, that makes life worth the living. Like the wedding day, commencement day will never lose its interest for society, but will stand as long as the school and college form the basis of our social well being. The laudable ambition to excel in educational advantages, has in recent years taken a firm hold of the American people, and countless schools and colleges have been established all over the Union. While some are the outgrowth of a pardonable vanity on the part of a wealthy donor, ambitious to perpetuate his name in a community out of which he sprung, the greater portion have been created in response to a demand for knowledge among the masses, impelled as it were by the exactions of increasing culture. Taking them altogether, they are evidence of a nation's effort toward intellectual advancement, and are a standing rebuke to those pessimists who love to dwell mournfully on the "degeneracy of the times." It is a matter for regret that so many of these newer colleges are weaklings, anticipating by years the capacity of the community in which they are situated to properly support. While giving every credit to the motives which founded them, they are costly evidences of the unwisdom, with which men of wealth, ambitious to serve the cause of education, often discharge their self-imposed stewardships. Had the same amount of wealth that has been scattered throughout the country in numberless higher class schools and colleges, that for years must have a very struggle for existence, been concentrated in well established educational centers, the benefit to the cause of sound learning, would necessarily have been very great, and it is to be hoped that future donors will add their benefactions to existing schools, rather than increase their number. The educational period through which we are passing, is in marked contrast to that which gave character to the generation now fading out of sight. This contrast is sharply drawn, and it will be in harmony with this evening's celebration, to ask your attention to some of the leading features of the "new education"—what it has done for society, and what course its future development will probably take. In the first place it is to be remarked, that educational systems are not determined by purely human inspiration, but rather the result of certain social conditions, forming the soil, as it were, out of which grows that system which it is best capable of nourishing. The "*Novum Organum*" could no more have been a product of a barbarous society, than the rose a product of the desert. In all social movements it is impossible to determine the exact line of demarcation between two radically different systems, so imperceptibly does one merge into the other. We know when a change is complete, and we are struck by the contrast with that which it has displaced, but the ending of one, and the beginning of the other, cannot be defined. As in all things the new never displaces the old without a struggle, so the ideas of the new education have not won their well nigh universal acceptance without vigorous opposition from the old regime of school men. It is the story over again in a different shape of the "Revival of the Arts and Sciences" in the fifteenth century, when the supporters of the scholastic wisdom of the middle ages had to yield to the progressive culture of a newer school. That contention history has handed down to us as the struggle between the obscurantists and the humanists, apt terms, tersely embodying the distinctive qualities of each. The obscurantists of our day, what few of them are left, are represented by those who would base educational systems upon transmitted opinions, on metaphysics and the literature of bygone ages. With such nature is held up to the test of authority, and in case of disagreement, so much the worse for nature. On the other hand our modern humanists rupudiate authority as such, regard society and civilization as a development, and test all ideas and asserted truths, in the crucible of natural law and order. While the germs of the new system can be traced back through centuries, in a fitful sort of a way, it was not until the close of the sixteenth century, when Francis Bacon, whom Pope pronounces the "wisest" and "best" of mankind, despite his moral infirmities, gave to the world a system of logic, at once so profound, complete and penetrating that it marks an intellectual epoch, and formed the solid foundation of modern processes of reasoning. He expressed his disgust at the school methods then in vogue in these words: "They learn nothing at the universities but to believe. They are like a becalmed ship, they never move but by the wind of other men's breath, and have no oars of their own to steer withal." The whole key of Bacon's teachings embodied in the idea that all learning, all knowledge should have but one object—"the good of humanity." He held that study instead of employing itself on wearisome and sterile speculations, should be engaged in mastering the secrets of nature and life, and applying them to human use. Instead of hypotheses, he called for facts, and he showed that the only road to truth was by proceeding from effect to cause, thus utterly reversing the customary methods of mental training and culture. It so happened that the intellectual soil succeeding Bacon's time was ripe for just such seed, which has grown and expanded into a tree bearing fruit of abundant promise. The growth was slow at first, but the dropping of its blossoms from time to time rapidly reinvigorated the soil in which it had taken root, and to-day we see its overshadowing branches extending throughout all civilized nations. The tangible fruit of this "tree of knowledge" is *science*, the true meaning of which term it is important for us to bear in mind. As formerly used, it was applied to those branches of knowledge termed physical, and there are those even now who have no higher conception of its meaning. In the present and developing order of things, it has a much broader signification, and refers more to a certain method of investigation, than to specific subjects. Its scope embraces all fields of human research that are capable of being brought under general laws, based upon observed facts. Science aims to bring "thought in harmony with things," and whatever subject is traceable from effect to cause, by inductive processes, is a legitimate field for scientific investigation, be it religious, social, political, or physical. The growing demand, on the part of the intelligent masses, for popularized science is due to the recent appreciation of this very broadened view of the meaning of science. So long as science was confined to the investigations of certain branches of purely physical interest, and taught as was done at our universities a quarter of a century ago, popular interest in it was rather of a sentimental kind. Science, however, when applied to higher problems, such as life, politics or religion, comes home to the thinking individual with transcendent power, and creates in him a very craving to know to what hitherto hidden Arcana, he is being conducted. As applied to education, the scientific method of investigation, as a system, is comparatively modern, and was the natural consequence of the discovery of the successful application of steam to practical uses, the opening era of the most stupendous advancement in things material, intellectual and moral, that the world has ever seen. None other could exist in a soil so prepared, and, as a consequence, we have seen the system based upon old university methods of scholasticism and tradition gradually fade away, until the bare bones are left, and even they are crumbling into the dust of antiquity out of which they sprung. The scientific system of education, the "new education," as it is sometimes called, is unassailable, in that it is a natural one, utterly untrammeled by reverence for the past, or devotion to a "school." It seeks nature before the study, and inculcates the acceptance of all truth based upon the facts of nature, as the only sure foundation of a progressive culture. It wars not with ideas, but with error, and is only intolerant of a refusal to accept truth regardless of consequences. It is impatient of shams of all kinds, and quickly pierces the shield of the charlatan. Jealousy, or a desire to restrict knowledge to a few, it is incapable of fostering, but on the contrary is aggressively active in disseminating information among the masses, which in turn produces a reflex action in the elevation of character and morals. The new system is but the legitimate evolution arising from social betterment, and none other than a "material age" could have supported its development. I am aware that this term "material," is often offensive to many well meaning people, but it is nevertheless true that the whole history of society, shows that all intellectual advancement is based upon certain material conditions. Food for the stomach, clothes for the back, and a cover for the head, are the individual's first needs. Due attainment of these in a greater or less degree is necessary before the receptivity of the mind is such as to appreciate the idea that man is two sided, intellectual as well as animal. The intellect, like the body, grows by what it feeds on, and if this food of which it partakes, consists of an active participation in the affairs of commerce, of mines, or manufacture, it is not to be wondered that its legitimate craving becomes a search after the facts and phenomena of nature, the proper appreciation of which is so essential to still higher social advancement. As civilization progresses, the luxuries of one age, imperceptibly become the necessities of the next, and any given generation would deem it a hardship to be compelled to return to the practices of their forefathers. The railway, steamship and telegraph have made the whole world kin, and year by year draw the nations of the earth together, through a community of interest and an enlargement of sympathy.

Before the age of steam, the experiences and observations of men, were limited to the narrow surroundings of their localities. Now the experiences of the world are brought to their feet, and they realize that so far from being independent factors in the guiding of events, they are so many units in the ocean of humanity, with a definite part to play in the scheme of development, and of an importance just in proportion to their power and wealth, both intellectual and material. Glorify the present as we will, we must not despise the past. There are names in antiquity associated with such commanding genius, and almost divine prescience, that they will live so long as literature is studied or science cherished. Emerson says, the "world has always been equal to itself," and take it at any part of its unquestioned history, it only has produced that which its intellectual soil and material condition was capable of nourishing. In the highest sense, the greatest triumph of science has been the reflex action upon culture and morals, as evinced in the emancipation of the minds of men from baneful superstitions, witchcraft, terrors of the untaught imagination, and a harmful reverence for tradition. Omens and auguries, long potent in influencing the actions of men, no longer hold sway, except among the ignorant and unlettered. Such mental fetters could not last under a system that teaches men that truth alone is worthy of study, to observe nature and follow her teachings. It is this contact with pure truth that elevates mankind, clears the head and purifies the heart. That broadens the sympathies until they take shape in efforts for the general amelioration of mankind, and inculcates the idea that the welfare of society is that of the individual. This spirit of science, which is truth, through self interest and sympathy, finds scope for expression in the building of hospitals, in the organizing of charities, in the improvement of laws, in the extension of the benefits of life insurance; in the elevation of the laborer, and in efforts towards adjusting his relations to his employer. The spirit of science is a great leveller of caste, teaches the equality of men before the law, and shows nations the conditions under which they can govern themselves without the intervention of kings. It has extended to woman control over her own property, and abolished slavery. The scientific spirit has taught people that disease is not a Providence, but neglect of the laws of health, only to be contended against by due observance thereof; that epidemics are preventible, and that rain will not fall without the necessary atmospheric conditions for its precipitation. Take all these reflex results of the new system of education, couple them with the direct physical pursuits of science, the improvement in the modes of living, of water supply, of drainage, the railway, the steamship and telegraph; and compare the result in their effect upon the one problem of life, "Human Happiness," with the best that can be said of the old regime of scholastic education. It is the comparison between the electric light and a candle.

What future developments science has in store for us, it would be rash to forecast with any attempt at details. We know that a vast amount remains to be done, so long as an ideal condition of society beckons us on. There is still an incalculable amount of want, and misery, and suffering, in the world; whole communities in ignorance, and many unadjusted questions between labor and capital. What is known as social science is just beginning to take form, and a host of problems growing out of it are to be worked out. The great work of the new educational system in the future will be the training of men to grapple scientifically with these social problems in all their complex relations, political and physical, and to sow broadcast among the people the idea of *causation.* That things proceed not by chance, but by law, that out of nothing, nothing comes, that there can be no effect without a cause, and that the operations of nature are conducted according to a system instituted when matter was formed and force originated. In physical matters, it is hardly probable that the world will ever see again such startling discoveries as those which have fallen to the lot of this century. The spirit of the "new education" will extend its benefits, and in the end carry them to people yet to be civilized. It will send out more workers in physical fields than ever before, but their work will consist in the development of details, and in the careful scrutiny of the by-paths that the past revolutionary discoveries, so to speak, have opened up. Of such work there will be an endless amount, indications of which are seen in the number of investigators in special lines of research, which from past experience we may expect will be sub-divided from time to time into still other special fields of study, as material accumulates. Civilized nations have insensibly adopted a system of divided labor, as a matter of economy partly, but principally because it has been forced upon them by the limitation of human powers. The system is not without its disadvantages, however, in that the specialist, devoted to one class of ideas, is apt to lose sight of the relativity of all knowledge, and to elevate into a fictitious importance the study he may have in hand. Like the aged German professor who had but one regret upon his death bed, and that was that he had not been spared a few years longer to complete his investigations of the Greek particle a, to the study of which he had devoted his life. The most far-reaching consequence of the general acceptance of the scientific method of investigation, and the latter day broadening of its scope, is its effect upon such speculative and practical questions regarding life, as have profoundly interested men from earliest times. It is pretty plain to most thinking men, that the idea of intellectual freedom is spreading among cultured nations, and with it a broad humanitarian view of men's relations to each other. Under the light of science, old landmarks are being swept away with a remorseless hand, and doctrines and ideas that once seemed as unchangeable as the everlasting hills are being questioned with a penetrating earnestness. What were supposed to be historical facts are either discovered to be no facts at all, or must be so modified in their interpretation, as to have an entirely new significance. Opinions are formed more slowly now than of old, just in proportion as the amount of evidence to be weighed is so much greater now than then. History must be re-reviewed in the light of modern discoveries, which have followed each other during the last quarter of a century, so thick and fast, as to task the intellectual strength of a generation to arrange and classify in their scientific bearings. There are more gifted men than ever before, more specialists in every realm of human thought, and more searchers after truth, who, in all parts of the world, are accumulating facts and data, on which the generalizations of future philosophers are to be based. So far as new discoveries and methods bear upon material matters, our only interest is one of present use. We take a new idea to-day, only to throw it off to-morrow, for one better adapted to our needs, and so advance from day to day to greater prosperity and comfort. But when we come to estimate the effect of new ideas and discoveries upon speculative matters, and apply the modern scientific method of analysis by induction, we shrink from the iconoclasm thereby involved, and often deliberately shut our eyes with stubborn persistence, rather than contemplate for an instant the possibility of error in the cherished teachings of our youth, or in the convictions of mature age. This is perfectly human, is therefore natural, and should not involve the calling of hard names. Being natural, this tenacity in matters of opinion, or of convictions having all the force of truth, play an important part in the scheme of intellectual development, must be weighed as a factor thereof, and not treated simply as an obstinate superstitious phase of human nature to be banished by cynical sneers. Whatever may be the outcome of what may be termed an age of intellectual unrest, through which we are travelling, one thing we can take calm contentment in, and that is in the final exaltation of *truth*, which is the highest aim of science. That end may be a long way off, but so surely as all nature is subject to the law of development and change in some form so surely will some future generation attain the beatitude of perfect intellectual rest. If indeed we ourselves do not find it beyond the experiences of this life—the mysteries of which neither the chemist's crucible nor the biologist's microscope can solve.

Gentlemen of the graduating class, as a practising member of your chosen profession, I may be permitted to extend to you the welcome of fellowship. You have entered the profession of civil engineer at a time of peculiar depression in all matters pertaining to public works, railways and manufactures—and have added to the members of a profession full almost to overflowing. It must be admitted that purely professional practice has not a very encouraging outlook to those whose ,affiliations are not such as to have positions provided for them. In times of great public disaster, such as have followed the commercial world for the last four years, the engineer is the one to first feel the blow, and the last to recover in returning prosperity. His office is one of disbursement, which appears so directly on the expense side of the ledger, that it usually overshadows the indirect benefits with which it should be credited on the other side of the account. He would be a rash man to prophesy a new era of such prosperity as we thought we had previous to '73. How far this prosperity will prove to have been real, it is difficult to say, until all the loose ends are picked up, and the balance sheet finally struck. Just at present, the civilized nations of the world are in possession of enormous productive capacities in all departments of manufactures, apparently far in excess of their respective markets. Transportation facilities seem to have fully kept pace with the manufacturing developments, and their extension at this time does not hold out a very enticing prospect to capital—at least in undertakings of any magnitude. It is hopeless to expect a speedy rectification of the lost equilibrium between supply and demand, or that it will be restored without further financial suffering. New markets must be sought in undeveloped countries, and population must increase to a greater or less extent to utilize what the United States, England, France and Germany, are now capable of producing. Until the balance is restored, prices will rule low, competition will be fierce, bankruptcies will be frequent, and capital will accumulate at financial centers in safe depositories, at low rates of interest. I must confess that the picture presented is not an encouraging one for the rising generation, but an early acceptance of the situation is certainly philosophical, if not agreeable. My object in alluding to such matters on this occasion, is to disabuse your minds of any ideas you may have formed of an early resumption of constructive activity in new works, which would give ample scope to your professional ambition; and further, to indicate the direction that I conceive to be the one where such an educational training as is given by the Rensselaer Institute will prove of value. I allude to the region of *economics*, the obtaining the most out of the least. In times of high prices, when prices are abnormally large, the idea of profit by saving is apt to be overlooked. On the other hand, when prices are low, the question of any profit at all, is a matter almost entirely of saving. The manufacturer, the merchant and transporter, are all asking themselves, *not*, how large a price they can charge for the commodities in which they deal, or for services rendered, but what is the lowest possible price for which such commodities or services can be afforded. Such questions involve a multitude of details, and it is just here where scientifically trained men have a large field before them. The economical organization of labor, the perfection of machines, the prevention or utilization of former waste, the rectification of past constructive blunders—are all questions of legitimate scientific study, which invested interests are rapidly recognizing. The economics of transportation, the management of railways and canals, are subjects that will repay the profoundest study, and in these directions the field for a brilliant professional reputation is most enticing. There are numberless vexed questions and unsolved problems in connection with transportation matters, that only scientific methods of thought, coupled with practical experience and observation, can grapple with. Ex-President Grant, is credited with the remark, that certain public men had the misfortune to begin their career as Major-Generals. There is a world of wisdom in the remark, no matter what its source, or to whom it applies. It is, indeed, a misfortune to rise in the world more rapidly than one's knowledge and experience warrant. Let me, as a matter of advice, caution you against the ambition of getting ahead too fast. All the schools in Christendom, cannot take the place of experience. They can at best, only prepare and furnish the mind, so as to make experience *scientifically* usable. It is, therefore, and in fact necessary, for a professional beginner to start low in the ranks, so that he may be familiar with the manner in which things are done, if he ever expects to take a high position in the command of men, and in the management of things. In other words, he must *know* how things are done, before he can *instruct* others to do them. In conclusion, let me remind you, that you go forth under the standard of an Alma Mater that has made a record in almost every state in the Union—see to it, that in your lives and practice, you honor her, as she this evening honors you.

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American War Materiel.—Eight steamer cargoes of arms and ammunition have been shipped to Turkey from the United States. Five or six similar cargoes have gone by sailing vessels.—*Engineering.*

AN ACCOUNT OF AN EXCURSION ON THE MARNE RIVER, INCLUDING A DESCRIPTION OF DESFONTAINE'S DRUM WEIRS.

By Prof. WILLIAM WATSON, late U. S. Commissioner.

In the spring of 1874, while at Paris, I made a number of excursions for the purpose of examining the admirable systems of river improvements which had attracted so much attention at the Vienna Exhibition. Having expressed a desire to see their practical working on a large scale to an eminent hydraulic engineer, I received from him a few days after a note, stating that one of his comrades, engineer of roads and bridges, stationed at Chateau Thierry, superintendent of a portion of the navigation of the Marne, intended to make a tour of inspection from Chateau Thierry to La Ferté.

I have spoken to him, said he, of your wish to visit the movable dams of the Seine and Marne, and he desires me to say that, if you will join him on Tuesday, he will be most happy to show you the works under his direction.

I accordingly left Paris in the evening, spent the night at Chateau Thierry, and early in the morning received a visit from M. J——, who proved to be one of my classmates at the School of Roads and Bridges in 1861; and to him I am indebted for the detailed explanations which enabled me to adequately appreciate this most remarkable system of inland navigation.

We embarked on a little steamboat, made entirely of steel, propelled by a small engine of five or six horse power, and soon arrived at the Azy dam. This dam has a fall of $2^{m}.10$, and may be thus described. It consists of:

1° A submersible lock, $7^{m}.80$ wide, 51^{m} long between the mitre sills, and placed on the bank used for towing.

2° Of a navigable pass, with Chanoine's system of falling gates. (To be described hereafter.)

3° A drum weir.

4° A pier, an abutment and other accessory works.

Each navigable pass has an opening of twenty-five meters. It is provided with twenty gates, like those of the upper Seine. Each gate has a width of $1^{m}.20$ and a height of 3^{m}.

The height of the upper bay above the sill is $3^{m}.31$.

The height of the lower bay above the sill $1^{m}.21$.

The sill of each pass, placed at first $0^{m}.60$ below low water, has been raised to $0^{m}.53$ by placing a cover $0^{m}.07$ thick upon the original sill, with a view of better protecting the gates when they are lowered. The space between two adjacent gates is $0^{m}.05$. When the gates are raised, their tops are $0^{m}.05$ below the normal level of the upper bay, thus permitting a slight overflow.

On the up-stream side of the twenty gates of each pass, are placed twenty fermettes $2^{m}.60$ in height, which can be raised or lowered into a recess in the floor or platform. These fermettes have a triple object, viz.:

1st. To support the lower story of a service bridge raised 2^{m} above low water, and upon which rolls a windlass to raise the gates.

2d. To receive a second story 1^{m} in height, so that the floor may be raised $0^{m}.50$ above the upper bay, and form a communication between the lock and the pier.

3d. To serve as a support for the needles of a Poiree dam. This consists of a screen of wooden battens called needles, $4^{m}.25$ long and $0^{m}.08$ square; the distance between their points of support is $3^{m}.70$; they form above the gates a second dam, and keep the water at its required height. In order not to strain unduly either the needles, or the gates, the force of the fall is equally divided between them.

The division of the fermettes into two parts, one placed above the other, is doubly advantageous; in the first place the fermettes are shorter, and, in consequence, the necessary interval between the last fermette and the pier is less, as is also the recess to be made in the latter to receive the fermette; and finally, the first service bridge can be nearer the water surface, so that the gates' chains can be pulled under an angle more favorable for lifting them.

DESCRIPTION OF DESFONTAINE'S SYSTEM OF DRUM WEIRS IN THE MARNE BARRAGES.

Each weir forty-eight meters wide is composed of a fixed and movable portion.*

The fixed part consists of a mass of béton, faced with masonry, poured between two lines of sheet piles (Fig. 3), with an interval of $7^{\text{m}}.50$ between the lines. This mass rises to within $1^{\text{m}}.05$ of the upper bay level, or what is the same thing, to an average height of $1^{\text{m}}.20$ above low water. This fixed part is surmounted by 33 movable *hausses* or gates $1^{\text{m}}.50$ wide, with their tops, when they are up, at 1^{m} above the permanent portion; that is to say, at $0^{\text{m}}.05$ below the level of the upper bay. These gates were designed by M. Desfontaine, and called by him *hausses à tambour*, or drum gates.

The object of M. Desfontaine was to operate the dam by utilizing the power produced by the fall itself, so that the lockman should only have to direct this power in a simple and easy way. The solution is as complete as it is ingenious.

The moving apparatus (Fig. 3) consists of a series of gates, independent of each other, and turning around a horizontal hinge *a* placed in the middle. The upper half, *ab*, is the *hausse* or gate properly so-called; it is this which forms the upper bay. The lower half, *acd*, called the counter *hausse*, has no other function than to carry along the *hausse* in the movement impressed upon itself. It is inclosed in a quarter of a horizontal masonry cylinder of the same length, whose axis coincides with the hinge, and in which it can consequently make a quarter of a revolution. The limiting planes of this quarter of a cylinder, or drum, do not pass exactly through its axis. The horizontal one is slightly raised parallel to itself, and the vertical one has been similarly moved back, so as to leave the empty spaces *l* and *k* between the drum and the extreme positions of the counter *hausse*.

The counter *hausse* has also been slightly bent downward, in order to diminish the raising of the horizontal bounding surface, and thus prevent it from masking a part of the *hausse*.

Finally, the ends of the drum are closed by two sheet iron partitions in which two rectangular openings, *l* and *k*, have been made, corresponding to the empty spaces just mentioned. The successive drums with their *hausses* are made in the body of the weir. They rest upon the béton contained in the inclosures, and are in close contact with each other.

If we now consider the whole body of drums, we see that by their union below the crest of the weir, and along its whole length, they form a single tube, resting at one of its ends against the face of the pier, and at the other against the face of the abutment, and divided by the counter hausses into two longitudinal compartments.

In each pier, itself just above and below the line of the drums, two vertical wells, V and W, are made, communicating with the upper and lower bays respectively by the culverts E and D. These two wells also communicate with each other, by means of two pipes M and N, built into the masonry and closed at each end by valves *y* and *x*.

These pipes fork in front of the openings *l* and *k*, and passing through the pier, one connects with *l*, and the other with *k*. By the valves *x* and *y* on these two pipes the tube *l* may be put into communication with the upper bay, and *k* with the lower bay, or *vice versa*.

Let us suppose the *hausse* down, and *l* actually put in communication with the upper bay while at the same time *k* connects with the lower bay; the difference of pressure on the counter *hausse* will cause it to take the position *acd* carrying with it the *hausse a b*.

If, on the other hand, the tube *k* is put in communication with the upper bay, and *l* with the lower bay, the system is reversed, and the pressure forces the *hausse* to take the position *b'ac'd'*, that is it lowers the *hausse*.

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* These dimensions are those of the Azy barrage which was personally examined. I am indebted for much valuable information concerning the Marne barrages to MM. Lagrené and Saint Yves; also to M. Jozon, Engineer *des Ponts et Chaussées*, stationed at Chateau Thierry, for a visit to these *barrages*, for the opportunities of seeing them in operation, as well as for numerous other courtesies.

DESFONTAINE'S DRUM-BARRAGE.

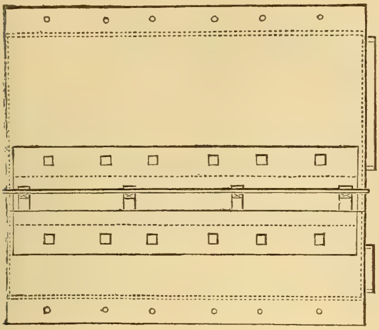


FIG. 1.—Plan of the *hausse* and its drum.

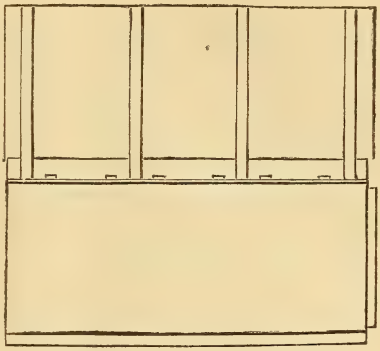
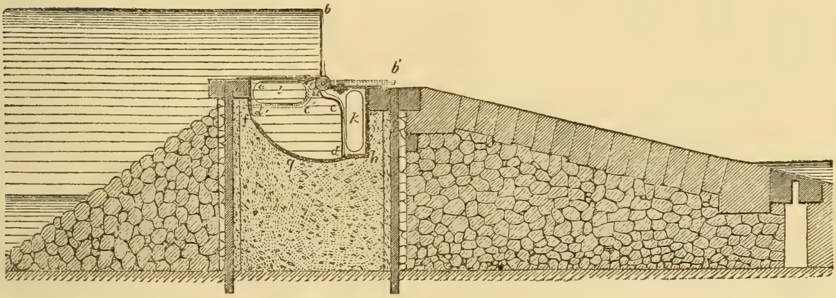


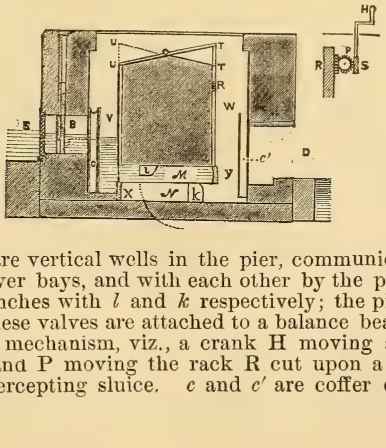
FIG. 2.—Elevation of the same.

FIG. 3.—Transverse Section of the Waste Weir.



EXPLANATION.—*a* is the axle ; *a e f g* the drum ; *a b* a hausse, or gate ; *a c d* a counter-hausse ; *b a c d* is the gate raised ; *b' a' c' d'* is the gate lowered ; *k* and *l* are transverse canals extending the whole width of the dam ; *k* communicates with the lower and *l* with the upper bay, or *vice versa* according as the gate is to be raised or lowered. This communication is made by aqueducts and sluice-gates placed in the piers and abutments of the dam.

FIG. 4.—Transverse Section at a Pier.



EXPLANATION.—V and W are vertical wells in the pier, communicating by culverts E and D with the upper and lower bays, and with each other by the pipes M and N. M and N connect by forked branches with *l* and *k* respectively; the pipes M and N are closed by the valves *y* and *x*. These valves are attached to a balance beam T U and are moved up and down by a simple mechanism, viz., a crank H moving an endless screw S, S gearing into the pinion P, and P moving the rack R cut upon a small portion of the valve rod X U. B is an intercepting sluice. *c* and *c'* are coffer dams to be used in case of repairs.

The exact method by which these communications are made, is shown in Fig. 4.

M and N are the two pipes connecting virtually the upper and lower bays, the upper pipe M communicates with *l*, and the lower, N communicates with *k;* neither of these pipes is continuously open; they are closed by the valves *x* and *y*. When *x* is lowered and *y* raised, *l* communicates with the upper bay, and *k* with the lower bay, and *vice versa.* As these valves should move together and always have opposite positions, *i.e.*, when *x* is up *y* should be down, they are attached to a working beam T O U.

The motion of the beam is effected by means of a crank H, attached to an endless screw *s*, whose pinion *p*, works in a rack R cut upon the rod *x u*.

A very small amount of force exerted on the handle H, is sufficient to open the whole dam; M. Jozon put the handle into my hand, and after making a few revolutions with it I saw the gates begin to move, and in less than five minutes the whole dam 160 feet wide was opened, and the water roaring over its prostrate gates. By turning H in the contrary direction, one after another of the gates rose, as if by magic, against the force of the current, and in about three minutes all were up again, and quiet was entirely restored.

In conclusion, we may say in the words of M. Saint-Yves, that Desfontaine's system of movable drum weirs is certainly very ingenious and very satisfactory; it is reduced to a movement of valves, connected together by a balance beam, which work under a simple and easy impulse, and to gates in one piece which turn around a horizontal axle, without the complication of counterpoise, retaining chains, props or tallon bars.

The gates rise directly against the current, without the aid of detached or outside machinery. This remarkable result is obtained by the simplest means. The force utilized is not that of man increased by mechanical intermediaries, it is the very force to be overcome that assists the engineer, and blindly obeying the intelligent direction which is impressed upon it, contends against itself, and from the enemy it seems to be, becomes a docile instrument. In view of its unity, and of the simplicity of its conception and its working, it is one of the most remarkable inventions that ever originated from the laborious investigations of an engineer.

[*Historical Note.*—We extract the following from the notice which accompanied the model of Desfontaine's Drum Weir, exhibited in the French Department of Public Works: "In Holland from time immemorial fan-gates have been employed to close the irrigation canals. The unequal breadth of this kind of gate renders it susceptible to the power generated by a moderate lift. . . . Probably the idea passed from Holland to America, for about the year 1818 it was applied on the Lehigh river to gates with horizontal axes; it was described by M. Michel Chevalier in 1843, and advocated by M. Mary; the result of which was its trial in France on the Marne by MM. Desfontaine and Fleur-Saint-Denis. Although the work could not have been in more skillful hands, the first expectations were not realized. But this attempt failed not to leave in the inventive mind of M. Desfontaine the germ of an idea which carefully considered and diligently elaborated, has produced the present system; and it is with some pride that we return to the other side of the Atlantic the American barrage perfected under French auspices."]

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The *Journal Officiel* states that the steam engines in France now give an aggregate of 1,500,000 indicated horsepower, representing a force of 4,500,000 horses, or 31,000,000 men—that is to say, ten times the true industrial population; the industrial population of France now amounts to 8,400,000 inhabitants, women, children, and old people included, among whom can be reckoned only 3,200,000 active workers. The first engine which appeared in France came from Boulton & Watt's works, at Birmingham, in 1789, and was used for distributing water in the city of Paris. It was not till 1824 that large works for construction of steam engines were begun. Some idea of the great comparative progress of the last 13 years may be had from the fact that in 1852 there were 6,000 fixed steam engines representing 45,000 H.P.; in 1863, 22,500, representing 618,000 H.P.

ON THE STRENGTH OF COLUMNS.

By E. HATZEL, Royal Architect, Bavaria.

Translated from the German by THOS. H. JOHNSON, C. E.

When a prismatic body is pressed together in the direction of its length, the complete resistance is only called in play when the pressure is uniformly distributed over the cross-section. Such a uniform distribution of the pressure occurs only when the length does not much exceed the least dimension of the cross-section.

On the other hand, when a prismatic body, whose length greatly exceeds its least transverse dimension, is compressed by a force acting parallel to its length, this force at the same time produces a lateral bending, and hence the body must resist compression and flexure at the same time, and is bent out on one side of the prism.

Prismatic bodies subjected to such compressive forces, may be classified as follows:

I. A short prism, whose length does not much exceed its least transverse dimension, and which is called upon to resist a uniform compression only.

II. A prism of greater proportionate length, which is called upon to resist flexure also; but in which the whole cross-section is exposed to pressure only.

III. A prism of so great length-ratio, that the flexure produces a strain of *tension* in a portion of the cross-section.

The resistance of a prismatic body depends chiefly on the strength of the material and area of the cross-section; but also on the ratio of length to least transverse dimension, and on the form of the cross-section; all of which factors must be included in any correct formula for the strength of such body.

In establishing such a formula we should consider also the kind and condition of the supporting body, and in what manner the load is distributed over its upper surface; in which connection it should be observed, that for a load unequally distributed over the upper end surface, the resistance to compression and flexure must be made greater than for an uniformly distributed load. It is, however, generally assumed that the supporting column is vertical, that the load is uniformly distributed over the upper surface, and that the compressive force acts vertically downwards.

§ 1. When a short prism standing erect, is loaded with a uniform load, the pressure is uniformly distributed over the whole area of each cross-section. In consequence of this uniform compression, the material of the prism undergoes a shortening of its height, and an enlargement of its section, in a horizontal direction. In this resultant change of form, the shortening of the material in its height is uniform throughout the mass; but the horizontal enlargement is dependent on the form of the cross-section. Thus in a cylinder under a uniform compression, the enlargement is distributed from the center of the circular section outwards, through uniformly concentric circles, and results in an uniform enlargement in the direction of all the radii. In all other forms of cross section in which the diameters through the centers of gravity of the section are unequal, it is found that the increments in the lengths of the several diameters are inversely proportional to lengths of the diameters themselves.

The resulting change of form may be readily observed, when we assume a strong pressure, and a short cylinder or prism of some material which is very elastic, and receives a considerable change of form from the pressure.

In this change of form the upper and lower end surfaces sustain but very slight enlargement, because they are prevented from spreading by reason of the friction existing there. This enlargement increases from the ends towards the middle of the height in such ratio as to produce a curved profile, and the original outline of the cylinder is changed to the form h,A,giBk, (Fig. 1).

In one and the same cross-section of a cylinder, the enlargement is wholly uniform, and is of equal size in the direction of all radii, forming the new outline A′C′B′D′, which is similar to the original section and concentric with it.

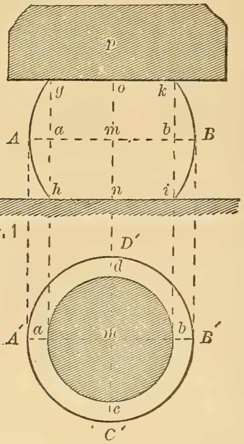


Fig. 1

The resistance of a short cylinder to a uniform force is proportional to the cross-sectional area, and we have

$$P = KA \quad . \quad . \quad . \quad . \quad (1)$$

in which P = the compressive force

K = the resistance per sq. unit of section

A = the area of the cross-section.

§ 2. The resistance which the square cross-section *efgh* (Fig. 2), offers to expansion along the different transverse lines *amb*, *cmd*, *emg* and *fmh*, is proportional to the lengths of those lines; and in the resulting enlargement, the shorter line *amb* sustains the greatest extension, $aA + bB$; and the diagonals *emg* and *fmh* sustain the least enlargement. In consequence of this the enlarged cross-section takes the form of a quadrilateral bounded by curved sides, convex outward.

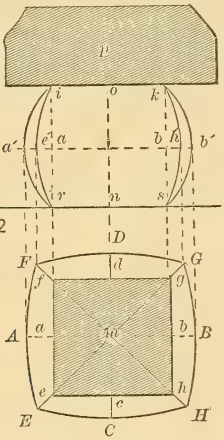


Fig. 2

Because of the unequal increments, the several cross-sections are made to differ, just as the lines of one and the same cross-section become different; so that each of the four sides becomes a warped surface.

When a square cross-section becomes extended to the limit of resistance of its shortest transverse line, a separation of its parts occurs on these lines, before the resistance in the direction of the diagonals has been exhausted, and hence the compression resistance of the whole cross-section is not made available. Therefore, the compression resistance K, of the unit surface of the square cross-section, will not be exactly the same as for a circular section.

Let this breaking resistance $K_0 = \frac{1}{n} K_2$; denote the moment of resistance of the prism by P_1, and the side of the square by a. We then have

$$P_1 = K_0 A = \left(\frac{1}{n} K_2\right) a^2 \quad . \quad . \quad (2)$$

When the cylinder and prism have equal cross-sectional areas, their respective resistances are limited to that which their cross-sections oppose to enlargement and separation in the direction of their least transverse diameters, and which is proportional to the lengths of those lines. But when a circle and square have equal areas, the least transverse diameter = the side = $a = 0.8864\ d$.

Substituting this in eq. (2) we have for a *square* prism

$$P_1 = 0.8864\ KA = \frac{KA}{1.12} \quad . \quad (3)$$

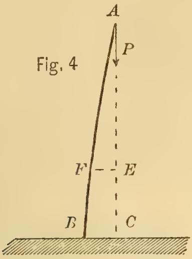
LONG COLUMNS.

When a prismatic body, whose length is considerably greater than its least transverse dimension, is compressed by a force parallel to its length, then that force causes at the same time a shortening of its length, and a lateral bending of the prism, and calls into play the resistance of the material both to compression and to bending.

When a prismatic body whose longitudinal axis AB, (Fig. 4) is originally vertical and fastened at its base B, is subjected to a vertical force, P, at its upper end A, it becomes bent to the line BFA. The amount of bending is, however, so little that the length of the curved line

AB, may be considered equal to its abscissa AC.

Fig. 4



Let l_1=AC=AB=the length of the bent line.

f=CB=the ordinate of the lower end B.

x=AE and Vy=EF=the ordinates of a point F.

δ=radius of curvature at the point F.

A=cross-sectional area of the prismatic body.

d=diameter of its circular, or least side of its rectangular cross-section.

I^d=moment of inertia of the cross-section, when its whole surface is exposed to compression.

I=moment of inertia of the cross-section when a part of its surface is in tension.

E=modulus of elasticity of the material.

$\lambda=\frac{K}{E}$=greatest possible shortening which the material can sustain per unit of length.

The force P, acting vertically downward, exerts on each cross-section a compression which is equal to P, and has the same value for all cross-sections, and at the same time each cross-section is subjected to a bending force, of which the moment in reference to any chosen point F is Py, and which calls into play the bending elasticity of the cross-section.

For the condition of equilibrium between the force P, and the compression and flexure produced by it, we have the following limiting equations. With reference to the uniform pressure on the cross-section it is

$$P=KA \quad . \quad . \quad . \quad . \quad (a)$$

And with reference to the bending alone,

$$Py=\frac{KI_d}{d} \quad . \quad . \quad . \quad . \quad (b)$$

From (a) we have $K=\frac{P}{A}$ and from (b), $K=\frac{d}{I_d}Py$, multiplying each of these expressions by $\frac{1}{E}$ we have

$$\frac{K}{E}=\frac{P}{AE} \quad . \quad . \quad . \quad . \quad . \quad (c)$$

$$\frac{K}{E}=\frac{d}{I_d\,E}Py \quad . \quad . \quad . \quad (d)$$

Again, we have $\frac{K}{E}=\lambda$, the greatest admissible shortening per unit of length, and P and Py are the forces by which this shortening is produced. Now then, both forces act simultaneously, and the shortening resulting from both of them taken together cannot be greater than $\frac{K}{E}$ its limit, and the sum of these values as found in equations (c) and (d) must be supposed equal to $\frac{K}{E}$.

We have, therefore, $\frac{K}{E}=\frac{P}{AE}+\frac{d}{I_d\,E}Py$.

from which $$KA=P\left(1+\frac{d\,A}{I_d}y\right) \quad . \quad (4)$$

$$KA=P\left(1+\frac{d\,A}{I_d}f\right) \quad . \quad (5)$$

The latter formula applies to the lower end B of the curve when y becomes $=f$.

The ordinates y and f can be determined from the co-ordinate equation of the line. This equation for an elastic line AB, Fig. 5, in which the direction of the force passes through both end points A and B, and the greatest lateral deflection CD=f occurs at the middle of its heights is

$$x=\sqrt{E\,I_d}\ \sin.^{-1}\frac{y}{f} \quad . \quad . \quad . \quad (6)$$

$$y=f\ \sin.\left(x\sqrt{\frac{P}{E\,I_d}}\right) \quad . \quad (7)$$

For the middle, D, of the height, where $y=f$ and $x\ \frac{1}{2}\,l$, we have

$$\frac{f}{f}=\sin.\sqrt{\frac{P}{E\,I_d}}.$$

But $\frac{f}{f}=1$, and therefore also $=\sin. 1$;

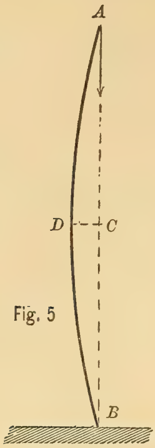


Fig. 5

and sin. 1 = sin. ½ π. So we may put instead of the foregoing equation

$$\sin. 1=\sin. \left(\frac{l}{2}\sqrt{\frac{P}{E I_d}}\right)$$

or

$$\tfrac{1}{2}\pi=\tfrac{1}{2}\, l\sqrt{\frac{P}{E I_d}}$$

from which

$$P=\frac{\pi^2}{l^2}\, E I_d \quad . \quad . \quad . \quad (e)$$

The ordinate f has thus been eliminated, and the force P can be determined without knowing the bending distance f.

The radius of curvature for any point of the elastic line is

$$\delta=\frac{E I_d}{P y} \quad . \quad . \quad . \quad . \quad (f)$$

For the point D, in the middle of the height, when y becomes$=f$, it is

$$\delta=\frac{E I_d}{P f}=\frac{l^2}{\pi^2 f} \quad . \quad . \quad (g)$$

or

$$\frac{E I_d}{P}=\frac{l^2}{\pi^2}$$

which may also be obtained by transposition of eq. (e).

A small part a, b, c, d, of a bent prism, Fig. 6, which in its unbent condition had the sections ab and cd parallel, and the length $ad=bc=1$, in consequence of the bending is compressed together through the height ed, and is changed into the form c, e, a, b.

Fig. 6

Let $\delta=c\,\mathrm{M}$=radius of curvature.

$\lambda=d\,e$=greatest admissible shortening which the material can sustain in the length $ad=1$.

In the triangles $b\,\mathrm{M}c$ and $d\,c\,e$, we have the proportion

$$b\,\mathrm{M} : b\,c=c\,d : d\,e \quad \text{or } \delta : 1=\delta : \lambda$$

from which $\delta=\frac{d}{\lambda}$. Substituting this value for δ in eq. (g) we have

$$f=\frac{\lambda\, l^2}{\pi^2 d} \quad . \quad . \quad . \quad . \quad . \quad (h)$$

Substituting this value for f in eq. (5) we have

$$A=\frac{P}{K}\left(1+\frac{\lambda\, l^2}{\pi^2}\frac{A}{I_d}\right) \quad . \quad . \quad (8)$$

$$P=\frac{K A}{1+\frac{\lambda\, l^2 A}{\pi^2 I_d}}=\frac{K A}{1+0.1013\,\lambda\, l^2\frac{A}{I_d}} \qquad (9)$$

When a prismatic body sustains a considerable bending and a part of the cross-section is in *tension*, then we obtain for its resistance P, the same expressions as in (8) and (9), except that in this case we must use, instead of I_d (the moment of inertia of the whole cross-section) the actual moment of inertia, I, of that part of the section which is in compression.

If, for instance, the bending of a prism, Fig. 7, is so large that one-half of the cross-section, $h\,c\,i$, is in tension, and the neutral axis passes through the center c, then we have, in this case, $h\,c=c\,d=\frac{1}{2}\,d$, and we have then, instead of eqs. (b) and (d),

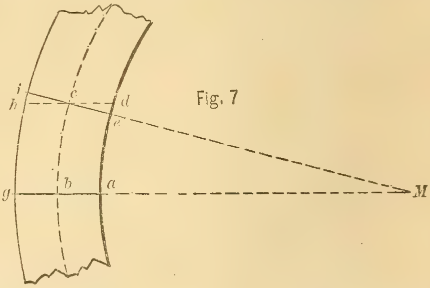


Fig. 7

$$P y=\frac{K I}{\frac{1}{2} d} \quad \text{and} \quad \frac{K}{E}=\frac{\frac{1}{2}\,d}{I A}P y.$$

Again, in the triangles $b\,\mathrm{M}\,c$ and $d\,c\,e$ (Fig. 7) we have the proportion

$$\delta : 1 = \tfrac{1}{2} d : \lambda,$$

From which $f = \dfrac{l^2 d}{\frac{1}{2} d \pi^2}$

Substituting these values of $\frac{K}{E}$ and f in eqs. (5) and (9) we obtain

$$P = \frac{K A}{1 + \dfrac{\lambda l^2 A}{\pi^2 I}} \quad . \quad . \quad . \quad (10)$$

If a prismatic body is subjected to a considerable bending, a part of its cross-section may be in tension; but such strain of tension is only possible when the result of the bending force is greater than that of the uniform pressure; that is to say, when in eqs. (9) and (10) the expression $\frac{l^2 \lambda}{\pi^2} \cdot \frac{A}{I} > 1.$

For the force P_0 which produces tension in the cross-section, we have

$$P_0 = \frac{K A}{\dfrac{l^2 \lambda A}{\pi^2 I} - 1} \quad . \quad . \quad . \quad (11)$$

From which it follows:

1st. If in the foregoing equation the expression $\frac{l^2 \lambda A}{\pi^2 I}$ is *less* than 1, then P_0 has a *negative* value, and the cross-section is, at no place, in tension.

2d. If the expression $\frac{l^2 \lambda A}{\pi^2 I} = 1$, then is the tensile strain $P_0 = 0$. In this case the section is subjected to *compression only*, and the neutral axis passes through a point of the circumference; or is coincident with a side of the cross-section.

3d. If this expression is *greater* than 1, then some part of the cross-section is in tension; but that part sustaining compression is always greater than that part sustaining tension. The position of the neutral axis depends on the proportion in which the value of this expression is greater than 1, which in turn depends on the "length ratio" $\frac{l}{d}$, and the greatest shortening $\lambda = \frac{K}{E}$, which the material can "sustain per unit of length, and also on load per unit of surface."

4th. When the expression $\frac{l^2 \lambda A}{\pi^2 I}$ is very great in proportion to 1 then is that part of the cross-section subjected to *tension*, nearly as large as that part subjected to compression; and in proportion as the material offers equal resistance to both forces will the neutral axis more nearly approach the center of gravity of the section.

5th. A prism composed of horizontal layers, in which the pieces only rest upon each other, and are not united together, can only be loaded within the limit of $\frac{l^2 \lambda A}{\pi^2 I} < 1$. When such a prism is subjected to pressure beyond this limit, then there will be a part of the cross-section in tension, against which the joints of the cross-section can offer no resistance.

A prism AB (Fig. 4) with its lower end fastened to the floor is bent in the same proportion as the upper half AD (Fig. 5). Hence the formulae, established in the preceding sections for (Fig. 5), apply also to (Fig. 4) by making the length $AB = l_1$ (Fig. 4) equal to the half length $AD = \frac{l}{2}$ (Fig. 5); or $l = 2 l_1$. Substituting this value for l in eqs. (8) and (9) we obtain

$$F = \frac{P}{K}\left(1 + \frac{4 l_1^2 \lambda A}{\pi^2 I_d}\right) = \frac{P}{K}\left(1 + 0.4052 l_1^2 \lambda \frac{A}{I_d}\right) \quad (12)$$

$$P = \frac{K A}{1 + 0.4052\, l_1^2 \lambda \dfrac{A}{I_d}}$$

For equal lengths, therefore, that part of the area which resists flexure must be four times as great for (Fig. 4) as for (Fig. 5).

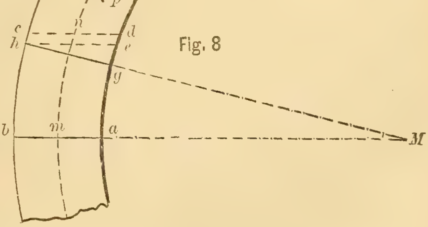


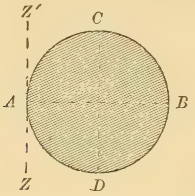
Fig. 8

Let $a b h g$ (Fig. 8) be a small portion of a bent prism, which is subjected to compression and flexure; but exposed to compression only, over its entire section, no part being in tension. This portion which in its original, unbent condition, had parallel sections *ab* and *cd*, is shortened in height to the extent $ch=de$, by the uniform pressure P, and by the bending force, is compressed on one side to the extent *eg*.

The shortening *ehg*, thus caused by the bending moment, forms always a triangle; and the axis around which the unequal shortening occurs, always coincides with the outer edge *h* of the section; and hence for the condition that the entire cross-section shall be in compression, the *moment of inertia*, I_d, *of the section must be computed with reference to an axis tangential to the circumference, or coinciding with one side of the section.*

For the circular section A C B D (Fig. 9) the moment of inertia must be computed with reference to the axis Z A Z′, tangential to the circumference at the point A, and its value is

Fig. 9



$$I_d = \tfrac{1}{4}\left(\frac{d}{2}\right)^2 A + \left(\frac{d}{2}\right)^2 A$$

or
$$I_d = \tfrac{5}{16}\, d^2 A \quad . \quad . \quad . \quad . \quad . \quad (13)$$

Substituting this value in eq. (8) it becomes

$$A = \frac{P}{K}\left(1 + \frac{\lambda\, l^2}{\pi^2},\ \frac{A}{\tfrac{5}{16}\, d^2 A}\right)$$

or
$$A = \frac{P}{K}\left(1 + \frac{16\,\lambda}{5\,\pi^2}\cdot\frac{l^2}{d^2}\right) \quad . \quad . \quad (14)$$

and by reduction

$$\left.\begin{aligned} A &= \frac{P}{K}\left\{1 + 0.3242\,\lambda\left(\frac{l}{d}\right)^2\right\} \\ P &= \frac{K A}{1 + 0.3242\,\lambda\left(\frac{l}{d}\right)^2} \end{aligned}\right\} \quad (15)$$

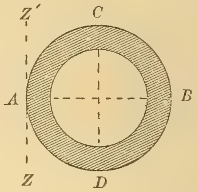
And for a cylinder in the condition of Fig. 4

$$\left.\begin{aligned} A &= \frac{P}{K}\left\{1 + 1.2968\,\lambda\left(\frac{l}{d}\right)^2\right\} \\ P &= \frac{K A}{1 + 1.2968\,\lambda\left(\frac{l}{d}\right)^2} \end{aligned}\right\} \quad (16)$$

From these equations we may compute the resistance of a cylinder of known sectional area, or the required sectional area for a known force P, if we know the modulus of resistance to compression K, and the modulus of elasticity E, for the material; and we may then determine at once the maximum length-ratio $\left\{\frac{l}{d}\right\}$ in which the cross-section is in compression only, and this gives the limit within which the formulae may be applied.

For the hollow cylinder (Fig. 10) the moment of inertia with reference to the axis Z Z′ is

Fig. 10



$$I_d = \tfrac{1}{4}\left(\frac{D}{2}\right)^2 A_0 + \left(\frac{D}{2}\right)^2 A_0 - \left\{\tfrac{1}{4}\left(\frac{d}{2}\right)^2 a + \left(\frac{D}{2}\right)^2 a\right\}$$

or
$$I_d = \tfrac{5}{16} D^2 A_0 - \left[\tfrac{1}{16} d^2 + \tfrac{1}{4} D^2\right] a \quad (17)$$

in which D=diameter of outer circle, d=diameter of inner circle, and A_0 and a respectively the areas of the outer and inner circles.

If we take, for example $d=\tfrac{3}{4}D$, then the preceding equation becomes

$$I_d = 0.1521\, D^2 A_0$$

And the area A of the annular section is

$$A = A_0 - a = \tfrac{7}{16} A_0$$

Substituting these values of I_d and A in equation (9), we have

$$P = \frac{K A}{1 + 0.2913\,\lambda\left\{\frac{l}{D}\right\}^2} \quad . \quad . \quad (18)$$

In a prism of square section A B C D (Fig. 11) the moment of inertia with reference to the axis ZZ′, coinciding with the side AB of the section, is

$$I_d = \tfrac{1}{3} h^2 A,$$

in which h=the side of the square.

Substituting this value in eq. (9) and at the same time modifying it from eq.

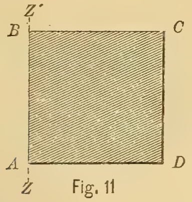


Fig. 11

(3) for the uniform compression on a square section, it becomes

$$P=\frac{K\,A}{1.12+0.3039\,\lambda\left\{\frac{l}{h}\right\}^2} \quad . \quad . \quad (19)$$

When a square and circle have equal areas the side of the square $a=0.8864d$; and substituting this value in the preceding equation we have

$$P=\frac{K\,A}{1.12+0.3857\lambda\left\{\frac{l}{d}\right\}^2} \quad . \quad . \quad (20)$$

therefore, under otherwise equal conditions, the resistance of a cylinder column is greater than that of a square column of the same area, in the ratio of the divisions in the preceding equations, which is for compression as 0.886 : 1, and for flexure as 0.84 : 1.

APPLICATION OF RESULTS.

Wrought Iron.—To apply the foregoing formulae to columns of different materials, it is only necessary to know the values of K and E for the material, and from them the value of $\lambda=\frac{K}{E}$.

For wrought iron, as shown by experiment K=42,500 lbs., and E=28,200,000 lbs.; hence

$$\lambda=\frac{42{,}500}{28{,}200{,}000}=0.0015.$$

Substituting this value in eq. (15) we have the expression for the *breaking weight* of a wrought iron cylinder, in the condition of Fig. 5

$$P=\frac{K\,A}{1+0.000486\left\{\frac{l}{d}\right\}^2} \quad . \quad . \quad (21)$$

In this eq. the expression $0.000486\left\{\frac{l}{d}\right\}^2=1$ when $l=45.4\,d$, and until the height reaches this limit the section will be in compression only.

From the foregoing equation we find that when $l=45.4\,d$ the breaking load for a wrought iron cylinder is

$$P=\tfrac{1}{2}\,K\,A,$$

and when $l=d$, $P=K\,A$. Hence the strength of a cylinder of length $l=45.4d$ is only one half that of a short cylinder of equal area; or the section of the first must be twice that of the second, to sustain an equal load.

For a wrought iron cylinder in the condition of (Fig. 4) the limit of length $l_1=\frac{1}{2}\,l=22.7\,d$ within which no part of the section will be in tension.

In practice only a portion K′ of the breaking load K per unit of surface is used, because the structure must not be loaded to breaking. In this case we have the shortening per unit of height $\lambda'=\frac{K'}{E}$ less than $\frac{K}{E}$, and for the corresponding load from eq. (15)

$$P'=\frac{K'A}{1+0.3242\,\frac{K'}{E}\left\{\frac{l}{d}\right\}^2} \quad . \quad . \quad (22)$$

When the resistance of the material to flexure is taken at about the elastic limit, and $K'=\frac{2}{5}K$, we have

$$\lambda=\tfrac{2}{5}\,\frac{K}{E}=0.0006$$

In compression, ordinarily one-fifth the breaking load is the largest allowed on iron structures. Using these values, the safe load on a wrought iron cylinder becomes

$$P'=\frac{8500\ A}{1+0.0001945\left\{\frac{l}{d}\right\}^2} \quad . \quad (23)$$

In this eq. the expression $0.0001945\left\{\frac{l}{d}\right\}^2=1$ when $l=72\,d$, and within this limit the section is in compression only, and no part in tension.

If we use one-fifth the bending resistance as well as for the crushing resistance, $\lambda'=0.0003$ and the limit of length becomes $l=101.4\,d$, [or $l'=50.7\,d$ for the condition of Fig. 4] in which the section sustains compression only.

Cast Iron.—The resistance of cast iron to compression is K = 106000 lbs. per square inch, and its modulus of elasticity E=14133000. Therefore,

$$\lambda=\frac{106000}{14133000}=0.0075$$

and eq. (15) becomes

$$P=\frac{K\,A}{1+0.00243\left\{\frac{l}{d}\right\}^2} \quad . \quad . \quad (24)$$

In this, the expression $0.00243\left\{\frac{l}{d}\right\}^2=1$ when $l=20.3\,d$ [or $l'=10.15\,d$] which is the limit of length without tension.

If we use, as before, one-fifth the resistance of the material in both respects, we have

$$P=\frac{21200\,A}{1+0.0004863\left\{\frac{l}{d}\right\}^2} \quad . \quad . \quad (25)$$

in which

$$0.0004863\left\{\frac{l}{d}\right\}^2=1 \text{ when } l=45.45\,d.$$

The resistance of a hollow cylinder of cast iron is as determined in equation 17. Taking, for example, the inner diameter $d=\frac{3}{4}D$, and making $\lambda'=\frac{\frac{1}{2}K}{E}=0.00375$, equation 18 then becomes

$$P'=\frac{K'A}{1+0.00109\left\{\frac{l}{D}\right\}^2} \quad . \quad . \quad (26)$$

In this eq. $0.00109\left\{\frac{l}{D}\right\}^2=1$ when $l=30.28\,D$, within which limit the section is subjected to compression only, when the load P' is not more than one half the breaking load.

Wood.—For the resistance of a cylinder of wood substituted in eqs. (15) and (16) the proper value of λ, which is, for breaking,

$$\lambda=\frac{K}{E}=\frac{7000}{1413000}=0.005$$

and the resistance of a wooden cylinder, therefore, becomes

$$P'=\frac{KA}{1+0.00162\left\{\frac{l}{d}\right\}^2} \quad . \quad . \quad (27)$$

in which $0.00162\left\{\frac{l}{d}\right\}^2=1$ when $l=25\,d$.

If we take $\lambda'=\frac{1}{3}\frac{K}{E}=0.001666$, and the compression resistance at $K'=\frac{1}{8}K$, the resistance then becomes

$$P'=\frac{850\,A}{1+0.00054\left\{\frac{l}{d}\right\}^2} \quad . \quad . \quad (28)$$

in which $0.00054\left\{\frac{l}{d}\right\}^2=1$ when $l=43\,d$ (Fig. 5) or $l'=21.5\,d$ (Fig. 4).

When the elastic modulus is taken at $\frac{1}{6}$, and $\lambda=0.000833$, we find $l=60.8\,d$, or $l'=30.4\,d$, as the limits within which the section is in compression only.

When the length-ratio exceeds the designated limits, a portion of the section will be in tension, and the foregoing formulae are no longer applicable.

When a cylinder, in consequence of greater length-ratio and load, is subjected to so great a bending that a part of the section is in tension, then will the neutral axis assume some position ZOZ′ (Fig. 12) intermediate between the point A in the circumference, and the center point, *m*, of the section. When a cylinder of a material of nearly equal tensile and compressive resistance, as wrought iron or wood, has a length very great in proportion to its diameter, and owing to excessive load, is very much bent, then will that part of the section which is in tension be nearly as great as that part in compression, and the neutral axis will therefore fall nearly or on the central axis C *m* D, of the section. When the neutral axis falls in the central axis of the section, then is its moment of inertia

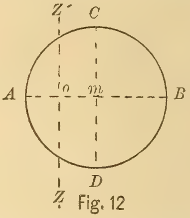


Fig. 12

$$I=\tfrac{1}{16}\,d^2A.$$

Substituting this value in equation (10) we have for the cylinder in the condition of Fig. 5

$$P=\frac{K\,A}{1+1.62\,\lambda\left\{\frac{l}{d}\right\}^2} \quad . \quad . \quad . \quad (29)$$

from which we find the breaking load of a cylinder of wrought iron, for which $\lambda=0.0015$

$$P_1=\frac{K\,A}{1+0.00243\left\{\frac{l}{d}\right\}^2} \quad . \quad . \quad (30)$$

This formula applies only to such a cylinder as has a very great length-ratio, and is so much bent, that the neutral axis falls in the central axis of the section. For a less important bending in which the neutral axis Z Z′ (Fig. 12) lies between the points m and A, we get a different value for P, the breaking load. Thus, for example, when the neutral axis lies in the middle between A and m, and the distance $Ao=o\,m=\frac{1}{2}r$, then for this special case, the moment of inertia $I=\frac{2}{16}d^2A$. Hence we have the expression,

$$\frac{\lambda l^2}{\pi^2}\cdot\frac{A}{I}=0.001215\left\{\frac{l}{d}\right\}^2$$

only half as great as in the above eq. (30), and making P_1 correspondingly greater.

Eqs. (21) and (30) give the limiting values of the resistance of a wrought iron cylinder. The strength of a cylinder whose length is equal to, or less than 45.4 d, is found from eq. (21), the neutral axis being tangential to the circumference; while eq. (30) applies only to the extreme case in which the neutral axis coincides with a diameter. For intermediate positions of the neutral axis, the resistance P_0 is greater than P_1 eq. (30), and less than P eq. (21). The real value of P_0 in this case cannot be accurately determined, so long as the position of the neutral axis is unknown.

The breaking load of a very long cylinder of wood, which sustains so great a bending that the neutral axis falls in the central axis of the section, is found from equation 29, by substituting the value of $\lambda=0.005$,

$$P=\frac{KA}{1+0.0081\left\{\frac{l}{d}\right\}^2}\quad . \quad . \quad . \quad (31)$$

Ordinarily, the strength of a cylinder of wrought iron or of wood, as used in construction, can be determined from eqs. (23) and (28), since, in practice, these numbers would not be allowed to sustain so great a bending as that a portion of the section would be in tension.

When a long prism of square section sustains so great a bending that the neutral axis falls in the central axis of the section, then there results in this case, under otherwise equal conditions, a greater resistance than in a cylinder.

A cast iron cylinder or prism should be allowed only so trifling a bending that the section would be in compression only, because of the comparatively feeble resistance of this material to tension.

A cast iron cylinder of great length should therefore only be loaded to such an extent that the resulting bending shall produce no tension in the cross-section.

Denote the compression resistance of the material by K, and the admissible portion of it by m; we then have from eq. (15)

$$P_n=\frac{m\,K\,A}{1+0.3242\,\frac{m\,K}{E}\left\{\frac{l}{d}\right\}^2}\quad . \quad (32)$$

The section will be subjected to compression only, and not to tension, when, in the foregoing equation,

$$0.3242\,\frac{m\,K}{E}\left\{\frac{l}{d}\right\}^2=1$$

putting for $\frac{K}{E}$ its value 0.0075 we have

$$m=411.5\left\{\frac{d}{l}\right\}^2$$

Substituting this value for m in the above equation, and observing that under the assumed conditions

$$1+0.3242\,\frac{m\,K}{E}\left\{\frac{l}{d}\right\}^2=2$$

we then have

$$P_n=205.75\left\{\frac{d}{l}\right\}^2 K\,A\quad . \quad . \quad (33)$$

making the safety modulus $K=\frac{1}{5}$ the crushing strength; viz. $\frac{1}{5}$ of 106,000 lbs. =21200 lbs., then

$$P=4{,}361{,}900\left\{\frac{d}{l}\right\}^2 A$$

Hence we have for cast iron cylinders of greater length-ratios than $l=20.d$, the following values for the allowed load P_n, in lbs. per square inch.

When $l=20\,d$ then $P_n=10910\,A$
“ $l=25\,d$ “ $P_n=6967\,A$
“ $l=30\,d$ “ $P_n=4848\,A$
“ $l=35\,d$ “ $P_n=3562\,A$
“ $l=40\,d$ “ $P_n=2728\,A$
“ $l=45\,d$ “ $P_n=2148\,A$
“ $l=50\,d$ “ $P_n=1738\,A$
“ $l=60\,d$ “ $P_n=1215\,A$
“ $l=70\,d$ “ $P_n=890\,A$
“ $l=80\,d$ “ $P_n=678\,A$
“ $l=90\,d$ “ $P_n=537\,A$
“ $l=100\,d$ “ $P_n=438\,A$

Hodgkinson found from numerous experiments on cast iron columns, the following mean values for the breaking load per square inch:

For $l=30\,d$ $P=24607$ lbs.
For $l=60\,d$ $P=7292$ lbs.
For $l=100\,d$ $P=3646$ lbs.

For these length-ratios the foregoing computed values amount to:

$\frac{1}{5}$ the breaking load for $l=30\,d$
$\frac{1}{6}$ the breaking load for $l=60\,d$
$\frac{1}{8}$ the breaking load for $l=100\,d$

The strength of a cast iron column, whose length is greater than $l=20\,d$, can therefore be determined with safety from the foregoing formulae.

STONE COLUMNS.

A free standing column or prism of stone or brick masonry cannot be fastened at its base as a standing support of wood or iron may be, but must hold its position chiefly through its weight and stability, because the cement used in the joints can only offer a slight resistance to tension.

But when a column A B (Fig. 4) which can be moved horizontally at its upper end A, sustains so little bending that no part of its section is in tension, then will the vertical compressive force produce the same effect in a column with the lower end not fastened as in a column fastened to the floor. Within this limit, therefore, the resistance of such a column can be computed from eq. (16).

The bending condition (Fig. 5) in which the direction of the force passes through both end-points, can only occur in a stone column which is so strongly constructed, and so firmly united to other parts, that it cannot yield laterally at its upper end. But ordinarily, only the smaller columns in the interior of a building are found so immovably fixed; while in a large, free standing column the upper end cannot well be so strongly united to other parts of the building. We can, therefore, for security, assume that in all stone or brick columns, the bending is of the class shown by Fig. 4, and hence we must compute the strength or dimensions from eq. (17).

For many varieties of stone the modulus (K) of resistance to crushing, is known from numerous experiments made with cube-shaped stones or short prisms. But this modulus (K) is not sufficient, in itself, to enable us to compute the bending resistance of a prism whose length is much greater than its least transverse dimension, because the modulus of elasticity (E) of the stone used, must also be known, but which hitherto has been determined for only a few kinds of stone. Besides, this modulus for the various kinds of stone shows such important variations, that no general value can be taken for it, as with iron and wood.

For one variety, namely, a white, hard sandstone, according to experiment, the modulus of resistance to crushing $K=5600$ lbs., and the modulus of elasticity $E=508800$ lbs.

From these values we find $\lambda=0.011$, and hence eq. (16) becomes for the breaking strength,

$$P=\frac{K\,A}{1+0.01426\left\{\frac{l_1}{d}\right\}^2} \quad . \quad . \quad (34)$$

In this, the expression $0.01426\left\{\frac{l_1}{d}\right\}^2=1$ when $l_1=8.38\,d$, within which limit the cross-section will be subjected to compression only.

In practice, stone is not allowed to be loaded beyond $\frac{1}{10}$ to $\frac{1}{15}$ of its breaking strength. Taking the factor of safety as to flexure at $\frac{1}{3}$, then $\lambda'=0.00366$, and hence

$$P\frac{K'A}{1+0.00475\left\{\frac{l_1}{d}\right\}^2}$$

whence $0.00475\left\{\frac{l_1}{d}\right\}^2=1$ when $l_1=14.51d$.

Taking the factor of safety as to flexure at $\frac{1}{6}$ then $\lambda=0.00183$, and the limit of length becomes $l_1=20.51\,d$.

In practice, a stone column of greater length ratio than $l=15\,d$ or $l=20.5\,d$ is not used, and the latter ratio could not be used because it would not have the requisite stability. The resistance of a stone cylinder may, therefore, be computed from eq. (16).

BRICK COLUMNS.

I have made several experiments with brick pillars of various heights. These pillars had a square section of 4.7 inch on each side; and the bricks were united with mortar joints 0.2 inch in thickness, composed of lime and sand with an admixture of cement.

From ten experiments with such pillars of various lengths, I obtained the following breaking loads:

When
$l = a \therefore K = 710$ to 735 lbs. per sq. in.
" $l = 6a \therefore K = 517$ to 455 " "
" $l = 9a \therefore K = 435$ to 452 " "
" $l = 13a \therefore K = 346$ " "

From one experiment with a brick pillar of 13.64 ft. height, and $12\frac{5}{8}$ in. $\times 6\frac{5}{16}$ in. section, in which the length-ratio was $l = 26a$, the crushing resistance was found to be 95 lbs. per sq. in.

This pillar was bound together at its upper end, and there secured against displacement. The bending of the pillar must, therefore, follow the manner of Fig. 5, and the crushing and bending resistance of the column can be computed from eq. (10). Substituting in eq. (10) the value of the moment of inertia $I_d = \frac{1}{3} a^2 A$, which it has in a rectangular section in reference to one side, we then have

$$P = \frac{KA}{1 + 0.3039\,\lambda \left\{ \frac{l}{a} \right\}^2} \quad . \quad (36)$$

From the above cited experimental results, it appears that a pillar whose length is about $11\frac{1}{2}$ times its breadth, will be crushed by one-half the load that a pillar whose height $l = a$ would bear. Putting, now, $l = 11.6a$ as the limit at which $0.3039\,\lambda \left\{ \frac{l}{a} \right\}^2 = 1$, we then have $\lambda \times 0.3039\,(11.6)^2 = 1$, whence $\lambda = 0.0244$.

Hence $$P = \frac{KA}{1 + 0.0074 \left\{ \frac{l}{a} \right\}^2} \quad . \quad . \quad (37)$$

From which we find the breaking load of a brick pier of the following length-ratios, when the modulus of resistance $K = 700$ lbs. per sq. in.:

When $l = 3a \quad \therefore \quad P = 662\,A$
" $l = 6a \quad \therefore \quad P = 558\,A$
" $l = 9a \quad \therefore \quad P = 442\,A$
" $l = 12a \quad \therefore \quad P = 342\,A$

The breaking load of a brick pier whose length l is greater than $12a$, cannot be determined from the foregoing equation, since, in that case, a part of the section will be in tension, and therefore the moment of inertia will have a value different from that here taken.

But when a brick pier is loaded with only one-fifth the breaking weight, then

$$\frac{K'}{E} = \frac{\frac{1}{5}K}{E} = 0.00488$$

and eq. (36) then becomes

$$P = \frac{KA}{1 + 0.001483 \left\{ \frac{l}{a} \right\}^2} \quad . \quad . \quad (38)$$

In which $0.001483 \left\{ \frac{l}{a} \right\}^2 = 1$ when $l = 26a$ the limit of length, within which the section sustains compression only.

When the upper end is free to move laterally, then the eq. becomes

$$P = \frac{KA}{1 + 0.00593 \left\{ \frac{l}{a} \right\}^2} \quad . \quad . \quad (39)$$

in which the limit is $l = 13a$.

Since a column or pillar of stone or brick masonry must sustain only so small a bending that the whole cross-section is in a state of compression, and no part in tension, we may compute the greatest allowable load in the same manner as was done for cast iron in eq. (33).

If m is the factor of safety, and K' the safe resistance to crushing, then from eq. (36), the greatest allowable load

$$P_n = \frac{m K' A}{1 + 0.3039 \frac{m K'}{E} \left\{ \frac{l}{a} \right\}^2},$$

or since for brickwork $\frac{K}{E} = 0.0244$

$$P_n = \frac{m K' A}{1 + 0.0074\, m \left\{ \frac{l}{a} \right\}^2}$$

in which we will have compression only when

$$0.0074\, m \left\{ \frac{l}{a} \right\}^2 = 1$$

Hence $$m = 135 \left\{ \frac{a}{l} \right\}^2$$

And since under the assumption made

$$1 + 0.0074\, m \left\{ \frac{l}{a} \right\}^2 = 2$$

then will

$$P_n = 67.5 \left\{\frac{a}{l}\right\}^2 K'A \quad . \quad (40)$$

If we make $K' = \frac{1}{10}K = 70$ lbs. per sq. in., then will

$$P_n = 4725 \left\{\frac{a}{l}\right\}^2 A.$$

From which we have computed the following table of greatest allowable load for brick piers whose length l is greater than $12a$:

When $l = 12\,a \quad \therefore \quad P_n = 33.07\,A$
" $l = 14\,a \quad \therefore \quad P_n = 24.31\,A$
" $l = 16\,a \quad \therefore \quad P_n = 18.61\,A$
" $l = 18\,a \quad \therefore \quad P_n = 14.70\,A$
" $l = 20\,a \quad \therefore \quad P_n = 11.91\,A$
" $l = 25\,a \quad \therefore \quad P_n = 7.63\,A$
" $l = 30\,a \quad \therefore \quad P_n = 5.30\,A$
" $l = 35\,a \quad \therefore \quad P_n = 3.89\,A$
" $l = 40\,a \quad \therefore \quad P_n = 2.96\,A$
" $l = 50\,a \quad \therefore \quad P_n = 1.91\,A$

When a brick pier is not confined at its upper end, but is free to move back and forth, then the greatest allowable load on such a pier, when l is greater than $6a$ is

$$P_n = 1192 \left\{\frac{a}{l}\right\}^2 A \quad . \quad . \quad (41)$$

from which we have obtained the following values:

When $l = 6\,a \quad \therefore \quad P_n = 0.475\,K'A = 33.25\,A$
" $l = 8\,a \quad \therefore \quad P_n = 0.254\,K'A = 17.78\,A$
" $l = 10\,a \quad \therefore \quad P_n = 0.168\,K'A = 11.76\,A$
" $l = 12\,a \quad \therefore \quad P_n = 0.117\,K'A = 8.19\,A$
" $l = 15\,a \quad \therefore \quad P_n = 0.075\,K'A = 5.25\,A$
" $l = 20\,a \quad \therefore \quad P_n = 0.042\,K'A = 2.94\,A$
" $l = 25\,a \quad \therefore \quad P_n = 0.027\,K'A = 1.89\,A$

The foregoing tables show that the resistance of a pier of masonry diminishes rapidly with the increase of height in proportion to thickness.

THE RELATIVE VALUE OF BLOWS AND PRESSURE.

From "The Engineer."

A most interesting treatise on the above subject has been recently published by Professor Kick, of the Prague Polytechnic, with diagrams of the results of some careful experiments. The whole is too long for us to transcribe *in extenso*, but we give such extracts, translated from the orignal German, as will introduce and illustrate, as nearly as possible in his own words, the Professor's theory and deductions. He mentions the numerous alterations of form in materials effected by a power acting either quietly and constantly, or by means of blows, calling special attention to the forging by means of hammers, in contrast with the Haswell press forge, to coining with the old stamp and fly press, instead of the Uhlhorn machine or hydraulic presses, to the fabrications of rod iron in the old forges, before the introduction of the modern rolling mills; and to the boring of stone with jumpers in contrast with the revolving diamond cutters of the present day. A glance at the above examples shows that the appliances, acting by means of blows, belong to an earlier age, and are undoubtedly more simple in construction than those whereby a quiet and suitably applied pressure executes the same, and ofttimes a better, work with advantage. It is not too much to say that the science of modern times is striving to replace the mechanical power exerted by blows by one acting by pressure. The foregoing illustrations show that a mechanical power, acting by blows, can be advantageously replaced by one acting by pressure, and that the labor expended in shaping materials under a quiet pressure is less than that wasted in using blows. The word pressure is here intended to comprise tension, deflection, crushing, breaking, pushing or torsion.

The work of shaping, be it stretching, shortening, bending, tearing, breaking, &c., amounts to bringing the smallest particles of any body, or a certain portion of them, into a different relative position. Every movement of the smallest particle, which by an alteration of form must alter its relative position, will meet with a certain amount of latent resistance. Let us now suppose exactly the same alteration of form produced by a quiet pressure and by a blow, the latent resistances are overcome equally in both cases, therefore the amount of work done in each must also be equal, *i.e.*, the mechanical effect produced by the apparatus acting by pressure must be equal to the percussion effect or *vis viva* required of the tool acting by blows in producing the like result. This ideal demand cannot, however, be fulfilled, because the blow must always produce condensation of the direct impinging and impacted surfaces, and vibrations which have nothing to do with the actual intended alteration of form, hence considerable mechanical effect is wasted. If, for example, we allow a weight to fall on the center of a bar of iron supported at both ends, there will follow not only a certain deflection of the bar, but also a compression of the outside strata which come into immediate contact with the blow, and consequently an often very perceptible increase of heat. The mechanical equivalent for one unit of heat is well known to be 424 met. kilogrammes. There results then by a comparatively small increase of temperature a large loss of effort, and, in consequence of depression caused by the blow, also a considerable waste of percussive effect, intended for the alteration of form. To this must be added the almost unavoidable vibration of the supports. We admit that a blow fulfills an immense duty when a rigid substance has to be divided. One might say that the splitting of a block of stone in the usual way with wedges driven in with sledge hammers requires a very small amount of effort compared with what would be necessary to break the block by means of a load acting by pressure. This admits of explanation. By splitting a block with wedges only those particles which lie in the line of rupture require to be affected to obtain a separation of the same, while the whole block remains unaltered; whereas if a rupture be produced by weight acting by pressure, it will not be confined to the line required alone to be affected, but will be attended by expansions and contractions in every particle of the stone, of which the greater part would be useless, if not detrimental.

Redtenbacher's "Principles of Mechanics" says: "The advantage, therefore, of separating stone by wedges lies in this—that a certain *vis viva* acts only just on those portions which must be separated one from the other, whereas by crushing, the whole body is affected unnecessarily."

Professor Kick, without disputing this, says: "The same effect can also be produced without the application of blows, and instances the use, for the same purpose, of blocks of wood saturated with water, and freezing water, also the breaking of glass by first heating and then plunging it into cold water; and then asks, "If an equal result be not obtained with less expense of labor, and still without a blow?"

Quoting again from Redtenbacher:—"The driving of a pile into the earth without the use of blows is, so to say, a practical impossibility. If one were to try to press a pile into the ground in any way whatever, enormous preparations and precautions would be required—either one must place a weight on the pile which would nearly equal that of a house, or if one intended to effect the sinkage by a press, the press would require first of all to be made as fast to the ground as the pile itself would be when sunk; whence one must pre-suppose that that which is to be already exists." Kick answers: "Here too, for the sake of 'useful effect of blows,' the argument is carried a little too far." And proceeds: Without mentioning that screwing piles into the ground is both easy and advantageous, so much is certain, that the power of pressure necessary to sink them cannot be spoken of otherwise than exorbitant. Let us take the example of a house four stories high resting on a single row of piles 3ft. apart, centre to centre, with the usual thickness of walls, according to the Building Act of Bohemia, and we find that the loading of each pile equals about 25 tons. Certainly no one will maintain that a pressure of 25 tons is no longer to be obtained. If, for instance, a pile driving vessel or in driving land piles, a wagon be loaded with this weight, the necessary pressure can be brought into effect in either case. Even if a double or treble security be required, that is, that the piles shall not sink any further under a weight of 50 or 100 tons, the application of such a weight is not accompanied by insurmountable difficulties.

In loose earth (sand) Captain Liernur, and later, others have obtained brilliant results by means of a thin pipe along the pile from whose orifice, in the neighborhood of the toe, a thin but powerful stream of water issues, which clears away and presses upwards the fine sand, and so facilitates the sinkage of the pile. Considered in this light, then, it appears very questionable if the use of blows in driving piles be attended with any advantage.

It certainly happens in the erection of works that a heavy body is moved, or in other words brought into position, by blows, and on account of its simplicity will always here and there be used. In this case, however, the mechanic must be quite aware that a certain amount of work which should be employed in moving, &c., is lost—*i.e.*, is converted into disintegration and heat, and that the lever windlasses are always more effective where they can be applied. As blows used in moving masses are attended by a waste of labor, on account of part of the efforts, accumulated in the substance inflicting the blow, being lost in disintegrating and heating, so when applied to alteration of form they always suffer a diminution of effect by compression of the outside strata, and the conversion of work into heat to a much greater extent than when a quiet and constant pressure is applied. It may be asserted that a fair comparison between the effect of blows and continuous pressure cannot be made, because the alteration of particles is quite different. In many cases this is true; nevertheless, in other and very numerous cases—*e.g.*, the bending of axles, coining, &c.—we may ask with good reason what the quantity of effect is for the same amount of deflection, stamping, &c., when blows are used as the power or when pressure. For such cases we have stated that, generally speaking, the expenditure of effort for the same effect is less with the application than that of blows. In support of the above assertions the following series of experiments, taken partly from publication, is given :—

(1) In a lecture on steel bronze, held on the 10th April, 1874, General Uchatius, the inventor of the new Austrian fieldpiece, published the results of his trials of the tensile of cast iron for cannons to blows and strains. The length of the rod was 75 millimetres; sectional area, 0.5 square centimetres; weight of monkey, 1.15 kilogs.

		Meters.				Met. kilos. effect.
The rods tore asunder with		0.72	Fall with	1 blow,	producing	0.828
“	“	0.63	“	2	“	1.430
“	“	0.54	“	4	“	2.480
“	“	0.45	“	8	“	4.140
“	“	0.36	“	14	“	5.800
“	“	0.27	“	37	“	11.490
“	“	0.18	“	352	“	72.860
“	“	0.09	“	2052	“	213.380

Consequently a breakage was effected with the least expenditure of effort, with a fall of 72 centimeter—namely, 0.828 met. kilogs. It can be taken for granted that a fall somewhere between 0.63 meter and 0.72 meter would have been sufficient, but as with a fall of 0.63 meter two blows were found necessary, the labor expended by each being 0.715 met. kilog., it may be more safely asserted that 0.828 met. kilog. is the minimum effect of a blow necessary to cause rupture. An alteration of form is therefore obtained by strokes or blows with so much less expenditure of effort as the number and strength of the blows applied are fewer and stronger.

Let us now inquire what amount of labor is necessary to tear asunder the same materials by means of a continuous pressure or strain. The trials gave as follows: (*See Table on following page.*)

The effective labor required for tearing asunder a rod of cast iron 1 square centimeter sectional area is about 0.2 met. kilog; and therefore that the effective labor required for tearing asunder a bar of cast iron ½ square centimeter sectional area is about 0.1 met. kilog. If we compare this with the effective labor required for tearing asunder by a blow, the result is in favor of pressure as 8 to 1. The work required for extension up to the limit of elasticity with an area of ½ square centimeter is about 0.003 met. kilog., or equal to a blow of a weight of 1.15 kilog. from a height of 3 millimeters, whereas Uchatius found that with a fall of 30 millimeters with this weight the limit of elasticity was not exceeded.

Since this experiment shows that one strong blow results in a better application of the *vis viva* than a number of

Weight in kilogrammes per square centimeter.	Extension in 0.00001 of length.		Weight in kilogrammes per square centimeter.	Extension in 0.00001 of length.	
	Elastic.	Permanent.		Elastic.	Permanent.
100	2	7	1300	84	14
200	10	0	1400	92	19
300	15	0	1500	101	24
400	22	0	1600	110	30
500	27	0	1700	120	35
600	33	0	1800	130	50
700	38	2	1900	142	65
800	47	4	2000	157	81
900	54	5	2100	—	—
1000	61	6	2200	—	—
1100	68	8	2300	—	—
1200	76	10	2420	rupture.	rupture.

weaker blows, and that an equal effect is obtained with a less expenditure of labor by means of a quietly working pressure than by the one blow, it follows generally that blows are attended by a greater waste of power than pressure in performing the same work; and that this is so, as far as regards cast iron, the above trials certainly go far to prove.

(2) Mr. Robert Lane Haswell, of Vienna, made two comparative trials to bend an axle by means of quiet continuous pressure, and then to straighten the same by blows. If the permanent deflection be not very great, the expenditure of labor necessary to straighten it can be looked upon as being the same as that required to bend it, because the same blow—W × H—which had previously caused a certain deflection was found able when properly applied to straighten a bent axle. In the press used the weight of lever and scale amounted to 241.25 kilogs., the short arm was 210 millimeters, the long arm 4845 millimeters; therefore the transmission was $\frac{4845}{210}$, or 23.07. The distance between the underlying blocks was 1.5 meter. A trial with a wrought iron axle of 132.75 millimeters diameter gave the following results:

Weight applied to lever.	Deflection.		Remarks.
	Elastic.	Permanent.	
kilogrammes.	millimeters.	millimeters.	
250	2.70	0	The weight applied must be added to 241.25 kilos. and the sum multiplied by 23.07 to ascertain the pressure applied to the center of the axle.
300	3.10	0	
350	3.46	0	
400	4.10	0.46	
425	4.60	0.75	
450	5.10	1.30	
475	6.60	2.60	
625	—	17.00	
685	—	24.00	

The same axle was straightened under the blows of a monkey.

Blow	Height of stroke. Meters.	Deflection in millimeters.
1	0.600	12
2	0.600	5
3	0.332	3
4	0.335	—2

The weight of the monkey being 649.5 kilogs., the entire expenditure of labor in straightening the axle—really a slight deflection over and above took place in the opposite direction—amounted to 1212.6 met. kilogs. For the straightening alone of 12 millimeters, 389.7 met. kilogs. were required. The proportion of press work to that of blows is nearly as 1 : 7.

Since, however, as Uchatius also found, that with repeated and weaker blows the expenditure of effort was still greater, we may, in making a comparison of the whole work, safely assert that the entire press work of 125 met. kilogs. is, in proportion to the entire work of blows, 1212.6 met. kilogs., nearly as 1 : 10. A second trial with a Bessemer steel axle of 129.5 millimeters diameter, under the same press, was permanently deflected to an extent of 12.5 millimeters as follows:

Weight placed on lever in kilogrammes.	Deflection.		Weight placed on lever in kilogrammes.	Deflection.	
	Elastic.	Permanent.		Elastic.	Permanent.
	mm.	mm.		mm.	mm.
25	1.50	—	385	3.99	—
50	2.00	—	485	4.99	—
75	2.00	—	585	5.66	0.9
135	2.75	—	760	7.70	1.5
210	3.20	—	885	8.40	3.5
260	3.40	—	1105	9.30	9.3
285	3.50	—	1183½	15.00	12.5

To straighten the same axle by blows the following results were obtained:

Blows.	Height of fall. Meters.	Deflection. millimeters.
1	0.60	7.0
2	0.70	2.5
3	0.60	0

The effective labor here was $649.5\times 1.9=1234$ kilogs. By the application of quiet pressure the deflection between 7 millimetres and 12½ millimetres required an expenditure of effort = 75 met. kilogs. —according to area w, x, y, z, while the first blow expended 389.7 met. kilogs. In this case, therefore, the pressure compared with the blows is as 1 : 5.

(3) Trial of Bessemer steel rails, undertaken by the Locomotive Superintendent of the States Railway of India, published in the German edition of "Engineering" —now Stummer's *Ingenieur*:—

Press trial.			Blow trial. Monkey: 10 cwt.; fall, 5 ft.	
Tons	Deflection in inches		Blow.	Perman'nt deflection.
	Total.	Perman'nt		inches.
6	0.13	—	1	1.25
7	0.15	—	2	1.25?
8	0.18	0.02	3	1.35
9	0.50	0.32	4	1.35?
10	0.90	0.70	5	1.31
11	1.55	1.31	6	1.31
12	2.36	2.10	7	Broke.

The distance between supports was in each case 3 ft., the work required for the permanent deflection of 1.31 in. under the press was about 480 foot-pounds, and the work of the first blow, which caused a deflection of 1.25 in., was 5000 foot-pounds, whence the proportion of pressure to blows is as 1 : 10. In a second trial the results were :—

Press trial.			Blow trial.	
Tons	Total deflection.	Perman'nt deflection.	Blow.	Deflection
	inches.			
6	0.13	—	1	1.19
7	0.16	0.01	2	2.31
8	0.23	0.07	3	4.63
9	0.63	0.45	4	Broke.
10	1.27	1.07	—	—
11	2.25	2.02	—	—

The distance between bearers, fall and weight of monkey, the same as before. If we compare the work of the first blow, which caused a deflection of 1.19., with that which was required of the press to cause the same deflection, we find the proportion of 5000 : 420. The amount 420 is taken from a diagram not shown here. The press work expended here is to that of the blows as 1 : 12. Other trials of rails Nos. 4, 5, and 10, gave similar results; also one of a chilled rail No. 20.

(4) We laid a lath of red deal of the cross sectional dimensions of 13.7 millimetres and 19 millimetres in its strongest position, on two bearers 0.60 metres apart. By means of a quiet pressure—determined by a diagram—with an expenditure of work of about 0.3 met. kilogs. it was broken. A monkey weighing 3.43 kilogs., with a fall of 0.10

metres, was allowed to fall on a similar lath without breaking it, the rupture occurring only when the fall was increased to 0.21 metres, representing a work of 0.72 met. kilog. Here the proportion of pressure to blow is as 1 : 2.4.

(5) A glass rod of six millimetres diameter was fixed at one end in a pair of lead cheeks, and a scale weighing 235 grammes hung on to the other end at a distance of 17 millimetres from the bearing :—

	Kilogrammes.		
The deflection with a load of	0.235	was	2 millimetres.
" " "	0.435	"	3 "
" " "	0.735	"	4 "
" " "	0.985	"	5 "
" " "	1.135	"	it broke.

It has been shown that 0.34 met. kilog. is the expenditure of work in effecting a rupture.

TRIAL UNDER BLOWS.

20 grammes with 10 centimetres. Fall on end of rod represented as work of 0.2 k. centimetres without rupture.
50 grammes with 10 centimetres. Fall on end of rod represented as work of 0.5 k. centimetres without rupture.
100 grammes with 10 centimetres. Fall on end of rod represented as work of 1.0 k. centimetres, broke.

We can therefore deduce from this trial that for the same amount of work —*i.e.*, rupture under blows—a greater expenditure of labor was necessary, the proportion of pressure to blows being as 1 : 2.

It has now been proved, first by No. 1 of the series of trials, that a certain alteration of form can be produced by means of a blow with the least waste of labor, when only one blow, provided it is strong enough, be given; also by the other trials, such as of cast iron, wrought iron, steel, wood, and glass, that quiet, continuous effort gives a more favorable result with regard to the expenditure of labor than any other form of applied power; therefore the assertion made at first, that the "expenditure of effort for the same effect is less by the use of pressure than by that of blows," may be taken as confirmed.

The question will naturally be asked how it happens that the proportion between the effect of pressure and blow varies between 1 : 2 and 1 : 12. These proportions must vary, more owing to the trials referred to than to the character of the materials, because they were not made under entirely similar conditions. The softer and more elastic the supports are on which the materials to be tested are laid, the less effective will be the blow, and the greater the waste of labor transmitted to the supports or lost in vibration. If the weight does not fall directly on the material to be tested, but on some intervening medium, as in trial 1, then the result is affected by this. The great differences even in the same material, such as wrought iron—as shown in trials Nos. 2 and 3—are therefore explained.

We have seen above that with timber and glass the difference between the amount of work for a certain effect by pressure and by blow is not very great; several trials made to show in what proportion the expenditure of work stands which is required to press or to drive (with blows) a nail into wood, showed in their results sometimes in favor of the hammer, and sometimes in that of the press.

From the foregoing Professor Kick draws the following conclusions :—(1) If we use blows for shaping or dividing substances a greater expenditure or waste of labor is necessary than if a quiet pressure be used instead. (2) If we know the mechanical work necessary to separate or break a substance under pressure, we can be quite certain that one blow which requires the same expenditure of mechanical effort *will not* cause the separation or rupture. (3) If the mechanical labor for the transitory alteration of the substance up to its limit of elasticity be known, that labor if applied through the medium of blows will not affect the substance up to its elastic limit.

As Professor Kick intends to continue his investigations and experiments with a view to publishing them, we hope at some not distant date to be able to give our readers the results in a fuller and more tabulated form than the present. The question is one of the deepest interest, more especially when applied to the working of the enormous masses of metal which are now employed in armor plates and big guns, and unless the arguments here put forward can be successfully refuted, the monstrous waste of labor under the method of steam hammers is a subject of serious consideration for manufacturers and calculation for political economists. If it can be proved that the calculations made by the authorities of the Indian States Railways apply equally to the working of metal when hot as to the disintegration of the same when cold under the effect of pressure as against that of blows, viz., as 1 : 12 (and it has already been proved that in the manufacture of wheels, crank axles, axle boxes, &c., under pressure, an immense saving in heats, time, labor, and consequently expense, is effected), then it only depends on the supply of material and the demand of the market, for our manufacturers by adopting this system to turn out in one month the present product of a year, or in a decade the productions of a century.

ON THE RELATIONSHIP OF STRUCTURE, DENSITY, AND CHEMICAL COMPOSITION OF STEEL.

By Prof. JOHN W. LANGLEY.

Read at the Buffalo Meeting of the American Association for the Advancement of Science.

At the present day, much attention is being given to the importance of studying the connection between the chemical composition and physical properties of matter, meaning by physical properties such characteristics as crystalline form, color, hardness, specific gravity, etc. The following paper is offered as a slight contribution to this department of knowledge.

There are two methods of investigating this subject: first, to take bodies whose chemical nature is intimately known, and to commence an examination of their physical character; the second, to take material long known and studied from the mechanical side, and to investigate its chemical composition. The latter is the method here followed.

Steel, from its great industrial importance, is the best known of all alloys, and its behaviour under mechanical forces has been most extensively studied, both by individuals and governments; but, unfortunately, the elaborate tables of tensile strength, elasticity, etc. thus produced, have not been supplemented by correspondingly thorough chemical analyses, because it has only very recently been surmised that slight variations in the composition of steel affect its behaviour more radically than do all the processes of the rolling-mill or the machine-shop. Within the last eighteen months, the United States have appointed a commission to study in detail the connection between the strength, elasticity, etc., of steel and iron, and their chemical composition as shown by analysis. The research, which is the basis of this paper, was commenced before the organization of the U. S. Commission; but after the General Government decided to take up the subject and to explore it for the benefit of engineers, all that part of the original design was, of course, abandoned, and our attention has been chiefly directed to a study of the chemical and molecular structure of steel—a field which it is probable will not be entered upon by the government commission.

In such an undertaking, the number of facts to be acquired and of subjects to be pursued in detail is very great; they are thus far in a very incomplete state, and I am therefore able to furnish a record of a portion only of the work done—that which merely serves as a foundation for future research. In March, 1874, Messrs. Miller, Metcalf, and Parkin, steel manufacturers of Pittsburg, selected eight samples of steel which were believed to form a set of graded specimens, the order being based on the quantity of carbon which they were supposed to contain. They were numbered from one to eight. On analysis, the quantity of carbon was found to follow the order of the numbers, while the other elements present—silicon, phosphorus, and sulphur—did not do so. As the method by which these samples were selected has an important bearing on the subject in hand, it will not be out of place to describe it.

The steel is melted in black-lead crucibles capable of holding about eighty pounds; when thoroughly fluid it is poured into cast-iron moulds, and when cold, the top of the ingot is broken off, exposing a freshly-fractured surface whose plane is approximately at right angles to the axis of the ingot. The appearance now presented is that of confused groups of crystals, all appearing to have started from the outside and to have met in the center; this general form is common to all ingots, of whatever composition, but to the trained eye, and only to one long and critically exercised, a minute but indescribable difference is perceived between varying samples of steel, and this difference is now known to be owing almost wholly to variation in the amount of combined carbon, as the following table will show. This consists of twelve samples selected by the eye alone in April, 1875, and the analyses were made from drillings taken direct from the ingot before it had been heated or hammered:

Table II.

Ingot Nos.	Iron by Difference	Carbon.	Difference of Carbon.	Silicon.	Phosphate	Sulphur.[*]
1	99.614	.302	..	.019	.047	.018
2	99.455	.490	.188	.034	.005	.016
3	99.363	.529	.039	.043	.047	.018
4	99.270	.649	.120	.039	.030	.012
5	99.119	.801	.152	.026	.035	.016
6	99.086	.841	.040	.039	.024	.010
7	99.044	.867	.026	.057	.014	.018
8	99.040	.871	.004	.053	.024	.012
9	98.900	.955	.084	.059	.070	.016
10	98.861	1.005	.050	.088	.034	.012
11	98.752	1.058	.053	.120	.064	.006
12	98.834	1.079	.021	.039	.044	.004
Mean			.071			

Here the carbon is seen to increase in quantity in the order of the numbers, while the other elements, with the exception of total iron, bear no relation to the numbers on the samples.

It has long been known that the structure of cast steel, as visible to the eye, bears some relation to the quantity of carbon present, and a rough classification by this method has been in practical use; but the above analysis show a very close connection between composition and structure, for differences of carbon so slight as seven-hundredths of one per cent. will impress such a change in the crystalline appearance of the metal that the eye of the expert can detect it, rarely ever making a mistake when the total carbon rises to a half per cent. or more. In mild steels the discrimination is less perfect.

The appearance of the fracture, by which the above twelve selections were made, can only be seen in the cold ingot before any operation, except the original one of casting, has been performed upon it. As soon as it is hammered, the structure changes in a most remarkable manner, so that all traces of the primitive condition *appears* to be lost; but although the crystalline form thus seems to be destroyed by heat or pressure, it can again be rendered evident by a special mode of treatment.

Another method of rendering visible to the eye the molecular and chemical changes which go on in steel, is by the process of hardening or tempering. When the metal is heated and plunged into water, it acquires, as every one knows, an increase of hardness, but also suffers a loss of ductility. If the heat to which the steel is raised just before plunging is too high, the metal acquires intense hardness, but it is so brittle as to be worthless; the fracture is of a bright, granular, or sandy character. In this state it is said to be *burned*, and it cannot again be restored to its former strength and ductility by annealing; it is ruined for all practical purposes, but it is in just this state that it again shows differences of structure corresponding with its content in carbon. The general nature of these changes induced by heat and tempering are sufficiently marked to be visible to an untrained eye, and can be illustrated by plunging a bar highly heated at one end and cold at the other, into water, and then breaking it off in pieces of equal length, when the fractures will be found to show appearances characteristic of the temperature to which the sample was raised.

The great molecular changes thus rendered evident are probably accompanied by changes of a chemical character between the iron and the carbon. According to Caron, such combinations do really occur, but the subject has thus far been too little investigated to warrant any decided expression of opinion.

There is a physical property which is

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[*] The determinations of sulphur were made by Prof. A. R. Leeds, of Hoboken, N. J.

well known to be intimately connected with chemical structure, viz.: density, and in the case of union between gases it has risen to be sometimes even the criterion of combination. The specific gravity of steel and iron has been taken many thousand times before this, but not usually in conjuncton with analysis; it is believed that a study of the densities of a series of steels under varied conditions and as a sequel to analytic work, would develop facts of interest. Accordingly, samples were taken from the above twelve ingots by boring out a piece with a crown drill, breaking off the core left by the tool, and then grinding and polishing the surface smooth; also six bars drawn from the ingot were heated to *burning* at one end and were left cold at the other, then plunged into water, thus forming sets like the fractures just alluded to; each bar was then broken into six pieces and the ends rendered smooth so that the specific gravity could be taken.

In the following table the results are given: the upper horizontal line contains the numbers belonging to the ingots, the left hand vertical column gives the order of the pieces broken from the bars:

Table II.

Specific gravities of twelve samples of steel from the ingot; also of six hammered bars, each bar being overheated at one end and cold at the other, in this state plunged into water, and then broken into pieces of equal length.

	1	2	3	4	5	6	7	8	9	10	11	12
Ingot................	7.855	7.836	7.841	7.829	7.838	7.824	7.819	7.818	7.813	7.087	7.803	7.805
BAR. Order of samples from bar:												
Burned 1...........	...	...	7.818	7.791	...	7.789	...	7.752	...	7.744	...	7.690
2...........	...	...	7.814	7.811	...	7.784	...	7.755	...	7.749	...	7.741
3...........	...	...	7.823	7.830	...	7.780	...	7.758	...	7.755	...	7.769
4...........	...	...	7.826	7.849	...	7.808	...	7.773	...	7.789	...	7.798
5...........	...	...	7.831	7.806	...	7.812	...	7.790	...	7.812	...	7.811
Cold 6...........	...	...	7.844	7.824	...	7.829	...	7.825	...	7.826	...	7.825

The temperature to which the densities are referred is 60° Fahr.

It is thus seen that the density decreases with the increase of carbon up to No. 5, which contains eight-tenths of one per cent. of carbon; below this number the influence of various physical conditions, such as rapidity of cooling, degree of fluididity before casting, etc., influence the specific gravity in an apparently erratic manner, though the numbers still continue to vary inversely as the carbon in a general sense; also if the influence of temperature on density is noted with regard to the sets of hardened samples, it will be seen that, taking the numbers from twelve to six (or those containing the highest amount of carbon), the specific gravity is lower the higher the temperature applied. Finally, the lowest horizontal line shows the densities of the metal as it left the hammer, and the upper horizontal ones belonging to the bars the expansion produced by over-heating. By the influence of hammering, all the pieces have been brought to nearly the same density, but as soon as the *burning* point is reached the steel is brought back nearly to the condition which it had at the moment of casting, and now the specific gravity is seen to vary in the same sense as the numbers in the ingot line above it.

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A TYPE foundry in St. Paul has lately furnished the types for the *Framvavi*, an Iceland newspaper, to be published in the Icelandic colony at Keewatin, on the Red River, in British territory about sixty miles from Fort Garry. This will, according to the *New York World*, be the first newspaper published on the American Continent in the Icelandic language. The preparation of the types required the greatest care. They are in the Roman alphabet, but with a great many peculiarities in regard to accentuation, and are of a very antiquated form.

LIGHTING BY ELECTRICITY.*

From "Engineering."

Beyond all question the use of electricity for purposes of illumination is destined in the immediate future to a very wide extension, both to replace in many instances the employment of gas, and in many others, and those of the highest importance, to supply a means of brilliant illumination, where at present only very feeble and imperfect sources of light are available. M. Fontaine, the author of this work, has devoted many years to the development and introduction of the gramme machine, so well known as a powerful means of producing the electric light, and largely employed for that and other purposes; and if we allow at once that he has shown a decided and possibly an undue preference for this machine, it has to be considered that his attention and interests have been concentrated upon it during many years. In this book he has contributed the most valuable addition to our practical knowledge of the subject, and although he, in a preface, gives a long list of other authors from whom he has derived much information, he is none the less deserving of much praise and thanks for the excellent manner in which he has performed his work. Naturally the first portion of the book is devoted to a consideration of the nature of the means of producing the electric light, as well as to a historical review of the various stages through which the science has passed from its inception to the present day. With regard to those subjects we need only say that M. Fontaine has written with much care and without tediousness, and we may pass on to the more practical portion of the book, as being of more interest and value to our readers.

The chapter on carbon points is very complete, and in considerable detail. The tabulated results given are of special value, as they are obtained from a number of careful experiments. We will refer particularly to one series of experiments. The points tried were: 1, of good retort coke; 2, those of M. Archereau; 3, those of M. Carré; and, 4, those of Gaudoin. The Archereau points are made of carbon finely divided and mixed with magnesia, the whole being brought into form under heavy pressure. The Carré points consist of

	Parts.
Very fine coke in almost impalpable powder	15
Calcined lamp-black	5
Liquid	7 to 8

The liquid is formed of thirty parts of sugar and twelve parts of gum. The whole is intimately mixed, and from one to three parts of water are added to make up for loss by evaporation. After being moulded under pressure the points are placed in a crucible and submitted to a high temperature. They are laid horizontally in the crucible, which is of cast iron, on a bed of coke dust, and each layer is separated by a sheet of paper. Between the upper layer and the cover a bed of coke dust, and then a bed of sand is placed. After the first operation, which should last from four to five hours, during which the points have to be maintained at a cherry heat, they are placed for two or three hours in a boiling and concentrated solution of cane sugar, being taken out two or three times to cool, in order to allow the solution to penetrate more freely. They are then drained and washed well in boiling water to dissolve the sugar on the surface.

After drying they are placed in a crucible and again heated, twice or more, until the desired density is attained, and finally they are cooled down slowly. These carbons are extremely tenacious, and can be freely used twenty inches in length and three-eighth inches in diameter without fear of breaking. The Gaudoin pencils are made of carbon produced from tar, resin, mineral oils, &c., and compressed in a mould. The following are the results obtained with the various forms by M. Fontaine during the present year:

Eclairage à l'Electricité. Renseignements Pratiques. Par Hippolyte Fontaine. Paris: J. Baudry.

EXPERIMENTS MADE WITH VARIOUS CARBON POINTS, APRIL, 1877.

Nature of Carbon.	Form and Size.	Consumption per Hour.	Illuminating Power in Carcel Burner.	Length of Arc.	No. of Revolutions of Machine per Minute.	Regularity.
		in.		in.		
Retort, good quality	.354 in. square.	2.362	120	.094	820	Fair (1)
Archereau	.393 in. diameter	2.677	173	.118	820	None (2)
Carré	.354 in. diameter	2.716	175	.118	820	Medium (3)
Gaudoin type No. 1	.440 in. diameter	3.149	203	.118	820	Good (4)
Gaudoin compressed wood charcoal.	.452 in. diameter	3.070	240	.118	820	Very good (5)

1. Numerous scintillations. Projections. Carbons used very irregularly.
2. Very variable intensity of light. Wear of carbons in small facets.
3. Very variable intensity. Good wear of carbon.
4. Light somewhat red but very constant.
5. Light very white. Less fixed than with the Gaudoin carbon No. 6. Small variation.

In proportion to the light produced the wear of the points was for the

Gaudoin wood charcoal points	1.259 in.	per 100 burners
Archereau	1.535 "	"
Carré	1.574 "	"
Gaudoin No. 1	1.574 "	"
Retort	1.968 "	"

Commenting upon the behaviour of these various carbons, M. Fontaine says: The light given by Gaudoin No. 1 was a little less regular than was obtained on a previous occasion; that given by the Carré varied in less than a minute from 100 to 250 burners; it turned around the points in the same way as with alternating currents. The Archereau pencils gave such a variable light that it was difficult to measure it by the photometer. The retort carbons alone preserved their durability, their luminous intensity, and unfortunately their irregularity.

Passing on to a historical review of electro-magnetic machines, M. Fontaine describes with much detail the latest form of the Gramme machine for producing a light equivalent to 2,000 burners. For lighting workshops and other industrial purposes, M. Gramme makes a machine with two magnets and one central ring. The weight of this latter is 396 lbs., its height 23.6 inches, its width 13.7 inches, and its length, including the driving pulley, 25.6 inches. The base weighs 264 lbs., and is 15.75 inches high. The copper wire of the electro-magnets weighs 61.5 lbs. and that of the ring 10.8 lbs. With this machine a light equal to 1,440 Carcel burners was produced, the speed being 900 revolutions per minute. The first workshop permanently illuminated by electricity was the Gramme atelier in 1873. The light was furnished by a single lamp, which took the place of twenty-five gas burners. During four years the work has been regular, and the cost has not exceeded 6d. per hour, including all charges. The space lighted is 40 feet square and 16 feet high. On a larger scale is the illumination of the Ducommun Works at Mulhouse. The main shop is 187 feet long and 92 feet wide. The lights are placed 17 feet above the ground, 69 feet apart longitudinally, and 46 feet transversely. The Gramme machines are placed in one of the boiler and engine houses. The total cost of establishment was £400, and the light obtained is equal to 400 burners. Many other workshops are also illuminated in the same way, among them we may mention those of MM. Sautter, Lemmonier & Co., of Paris, the Menier Chocolate Works at Grenel, Noiseul, and Roye, the spinning factories of Dieu-Obry, Daours (Somme) MM. Ricard fils, Mauressa (Barcelona), and MM. Buxeda, at Sabadell (Spain).

The lighting of the goods station at Chapelle-Paris, is also an important installation. It comprises a building 230 feet by 82 feet and 26 feet high, a shed the same length, 49 feet wide, and 26 feet high, and a court-yard 65 feet wide separating the main station from the shed. The hall is lighted with two lamps about 14 feet above the ground, and the shed and yard with a single light. The cost of lighting is 7.5d. per hour and per lamp. The cost of establishment was £920.

An interesting application of the light has been made at Havre harbor extension works. The works at night were carried on by 150 laborers, over an area of about 300,000 square yards. The lamps here were placed at an elevation of about 50 feet, and each had a power of 500 burners. Minute objects were quite distinct at a distance from the lights of 350 feet, and the works were carried on with perfect ease.

In 1863 the first application of electric illumination was made to a lighthouse near Havre. The Alliance machine was employed, and since that date this method of lighting has been greatly extended in England, France, Russia, Austria, Sweden, and Egypt. Hitherto, however, the most powerful machine employed is equal only to 200 burners, but M. Fontaine states that the French Government is shortly going to experiment with a Gramme machine of 2000 burners. Of equal interest to light-house illumination is the application of the electric light on board ship, and enough has been done in this direction to encourage its wide extension. At the end of March, 1876, the America, of the General Transatlantic Company's line, was fitted with a Gramme machine, and all accessories. The reflectors are placed in the top of a tower made of plate iron, access being gained by an internal stairway. The height of the tower is sixteen feet four inches above the deck, the diameter is thirty-nine inches, and it is fixed in the forward part of the ship. The reflectors illuminate an arc of 225 degrees, leaving the ship almost entirely in the dark; the beam has a depth of about thirty-one inches. The Gramme machine employed has a power of 200 Carcel burners, and is driven by a three-cylinder Brotherhood engine with a speed of 850 revolutions per minute. The commutators controlling the action of the light are placed in the captain's cabin, and the light can be extinguished or produced at will without stopping the machine. The French Admiralty has recently established on board the Richelieu, and will shortly place on the Suffren, 500 burner Gramme machines, driven by a Brotherhood engine. This light it is expected will be of great service for torpedo defence. The light is concentrated in a cylindrical beam, which can be directed upon any desired point, the lamp and reflectors being mounted on a revolving frame, to which any desired inclination can also be given. M. Fontaine gives an interesting chapter on the power absorbed in producing the electric light, and refers at length to M. Tresca's valuable investigations of this subject.

The question of cost is considered, and this, though of secondary importance, when intensity of light is absolutely necessary, is one of the first points to be considered before attempting the application to industrial purposes. In this respect the author makes a most favorable comparison for the Gramme machine, which, while we do not endorse it, we consider of sufficient importance to summarise in this review. He bases his estimate on a machine, motor, apparatus, wires for transmission, &c., complete for 150 Carcel burners, and assumes four such apparatus are required to illuminate a given factory, 500 hours of lighting during the year being allowed for. The points cost 6d. per foot, and are consumed at the rate of 3½ inch per hour, including breakages. The following is the estimate:

	£
4 tons of coal at £1.4 per ton	5.6
523 feet of carbons	12.8
Maintenance of the four apparatus	10.
Amortization on capital, £400 at 10 per cent	40.
Total	68.4

For a single light the cost of maintenance is much higher in proportion, and this decreases rapidly as the number increases. The expense diminishes also with increased duration of use; thus at M. Menier's factory at Noiseul, where work is carried on throughout the night continuously, the cost is as follows:

	£
1046 feet of carbon	25.6
Annual maintenance	8.
Amortization	10.
	43. 6

In this case, however, water power is used to drive the electro-magnetic machines; if steam were used 8 tons of coal would have to be added, which would bring the cost up to £54.4. With the latest type of Gramme machine it is claimed that these prices are considerably reduced. The following are stated as the proportional cost of various illuminating mediums as compared with the electric light:

Wax	75 to 1
Stearine	55 to 1
Colza oil	16 to 1
Gas	11 to 1

As the result of a detailed estimate for illuminating a factory with 415 gas lights and 6 electric lights, it is shown that the annual cost of the latter would be only 33 per cent. of the former, with six times the illuminating power.

The concluding chapters of this book are devoted to the consideration of lighting by incandescence, and on the divisibility of electric light as illustrated by the recent developments of M. Jablochkoff, which have recently been noticed in our columns. As we have already stated, M. Fontaine's book may be open to the objection of pressing too prominently forward the Gramme machine, but it is none the less a very valuable contribution to the literature of practical science, and treats very fully of a subject which is daily growing into increased importance.

REPORTS OF ENGINEERING SOCIETIES.

AMERICAN SOCIETY OF CIVIL ENGINEERS — TESTS OF AMERICAN IRON AND STEEL.— At a recent meeting of the Society, Prof. Robert H. Thurston was called upon to report the status of the "United States Board appointed to test Iron, Steel and other Metals." He gave an account of the origin of the movement to obtain the appointment of such a Board, and described the organization of that body, its plans and methods of work, its present position and its purposes for the future.

He remarked that the Board, having apparently reached very nearly the end of its history, it might be well to look to the beginning of the work. He proposed to follow closely the line of discussion pursued when making a similar statement, by request, before the Senate Committee on appropriations, March 6th, at which time he had been asked, as Secretary of the Board, to give an account of operations.

This plan of a systematic and thorough determination of the properties of the materials of construction made in the United States, and the scheme of making a really scientific examination of the composition and value of the metals used in their production, had an origin in two serious needs, and at a period which ante-dated the speaker's entrance into the Society. These two necessities arose from commercial conditions and from the requirements of constructing engineers.

We have been, for years, importing cast iron from abroad while we have domestic products of equal and even greater intrinsic value selling in our markets at lower price. We are importing boiler plate at 11 cents a pound when we can purchase American steel, vastly superior in all respects for the special purposes to which the former is applied, at 8 cents. We import vast quantities of foreign steel tools, when, at Pittsburgh and elsewhere, we make steel fully its equal. In New England and Pennsylvania, we have ores from which are made the finest cast-iron ordnance in the world. In Ohio, we make a metal for car-wheels such as never is seen in Europe, and of such tenacity and elasticity that foreign engineers listen incredulously when it is described. Our Lake Champlain ores make an iron equal to even Swedish for conversion into steel, and around Lake Superior and Missouri we have deposits from which come Bessemer metal vastly superior to the phosphorus-charged metal imported. New Jersey supplies us with zinc which meets with no competition as a pure metal and which can be used without purification for even chemical purposes; and our native copper is, as I know by experiment, absolutely free from admixture with injurious elements.

Yet, notwithstanding the fact that we possess the purest and best ores and make the best metals, we continue purchasing abroad

This fact arises: *first*, from a natural conservatism which induces us to continue to pursue a course to which we have been accustomed even after we knew it to be an improper one; *second*, partly from that unfortunate American habit of self-depreciation which assumes, whatever comes from abroad to be, from that fact, superior to the product of our own country and of our own industry; and *third*, from the fact that our own people do not know and cannot readily be made to believe that our own materials are so excellent.

It was to meet the last difficulty, partly, that this Board was proposed. No private individual can afford to attempt the systematic and only truly economical methods of development of these facts, and no one has interest so general as to make it imperative that he should do so, were it in his power. Even were the work done, and well done, by a combination of private interests, it would still have comparatively little value, as the public invariably looks with distrust upon all statements made by private individuals, and suspects that private interests may have given tone to their reports. The maker of the very best iron, or of the best possible steel, cannot prove beyond cavil that his product is better than any similar metal purchased abroad. Only the General Government can institute an investigation that shall cover the whole field, that shall be systematic and scientifically thorough, and of which the reported results shall be accepted without distrust.

The second of the two classes of necessities leading to the creation of the Board was felt most keenly by our engineers and constructors, and by our manufacturers of machinery and of parts of structures. They knew comparatively little of the strength of our metals in small parts, and were still more seriously ignorant of the effect of making up any material in large sections and into the heavy members of bridges and other structures. They could not predicate dimensions on well ascertained measures of the strength of our iron, and were ignorant of the loads which could be sustained by heavy beams, girders, and columns made of this or of any other metals.

For years, they had been compelled to base their calculations on tables of strength of materials furnished by foreign experimenters, as Hodgkinson, Tredgold, Barlow, Morin, Rondelet, and Muschenbroeck, who gave the results of experiments on Carron iron and other metals, whose names were strange to American engineers, and which our builders never use. Recently, Kirkaldy, Styffe and some German experimenters have given us valuable information, but nothing of any considerable value has been published in reference to our domestic materials.

These facts, and many more which the speaker had not time to consider, led to the appointment by the Society, several years ago, of a committee to secure the inauguration of scientific and exhaustive examination of our American materials by a Government commission. This committee sent a delegation of its own members, and of other members of the Society, before the House Committee on Appropriations, in the spring of 1875, and secured the modification of a bill already in committee, which had originated with the Architect of the Treasury, and its adaptation to the plan proposed. Under the provisions of a bill thus secured from Congress, the President appointed a commission, consisting of two army and two navy officers and three experts from civil life ; and this Board was organized and immediately adopted a very comprehensive plan of research, which was reported to the Society a year ago. Committees appointed to carry out the investigations proposed, issued circulars, which were published in all scientific and engineering periodicals, as well as in the Transactions of the Society, and which detailed these plans of work and asked advice and information. These circulars brought out very little useful material.

The Board contracted for a large testing machine, combining the plans of Messrs. Albert H. Emery and Charles E. Emery—the latter a Member of this Society—which machine was expected to have been long ago completed, but is not yet ready for work. It was intended to test large pieces, as heavy beams, girders, and columns.

While awaiting the completion of the machine, the Committees of the Board conducted their special investigations, where they could do so without the use of the large machine, making use of such other machines as were available.

Some of the Committee have reports, either completed or in progress. The committtee on Wrought Iron had finished several investigations of the methods of making iron, on the effect of impact on metal, on the effect of various strains, &c., &c. The Committee on Chain Cables has been studying the methods and material of cable manufacture. The committee on Tool Steel had completed a series of tests of the value of steels for cutting tools ; analyzing them to determine their composition, and breaking them to ascertain their mechanical properties. The report is in preparation. The committee on Abrasion and Wear had completed that portion of its work formerly reported as in progress. The Committee on Metallic Alloys has determined the tenacity and other forms of resistance of all copper-tin alloys, their ductility, resilience, density, &c. ; and the report is complete and in the hands of the copyist. A similar series of tests of copper-zinc alloys has been completed, and the report is in preparation ; and an investigation of the properties of triple alloys of copper, tin, and zinc is in progress. The speaker described the methods of research adopted and indicated the general nature of results attained. The Committee on the Effects of Temperature has collected a large quantity of materials for test, and is still getting samples. The investigation is planned, but not yet commenced.

As just indicated, the Board has been working steadily for two years while awaiting the construction of its testing-machine—has done a large amount of work, and has completed, or has in preparation, some extended and probably valuable reports. The Board has been criticised because no reports have been yet made public. It should be remembered that where researches require months for their prosecution, the preparation of reports upon them usually requires an equal length of time, or sometimes greater. The speaker has sometimes acquired information in one day's work, which he had been unable to reduce to proper shape for publication in many days. Such criticism is evidently unjust, and never comes from those who have had experience in a kind of work in which the results of weeks of investigation are sometimes expressed in a single paragraph. Furthermore, the Board can only report to the President at the proper time, and the reports can only reach the country through the action of Congress. They can, therefore, not be presented piecemeal or at any desired date.

The speaker was permitted to state ascertained facts, but the Board had no authority to publish the reports in which only those facts could be found in their proper relations, except by presentation to the President, and publication under Act of Congress.

The speaker then described some methods of research adopted, and stated some interesting facts brought out by their application, including the reasons of variation of strength of iron and steel in bars of different sizes, the effect of strain after periods varying from one second to one year, the relation of composition, and of strength and ductility to the value of steel for tools, the methods which had enabled him to determine the mechanical proportion and value for constructive purposes of all possible copper-tin, copper-zinc, and copper-tin-zinc alloys, the purposed methods of determining the effect of temperature, etc., etc.

The speaker stated that reports of progress had been made to the President, and that, a year ago, Congress had been requested to make an appropriation to enable the Board to continue its work, and to do some heavy work with its testing machine.

The appropriation was granted by the Senate, but defeated by the House Committee. The same experience had been met with during the session just closed. No opposition had been met with either in the Senate or on the floor of the House, and every well known member of either party, and especially those of recognized intelligence and standing, had taken real interest in the matter. The House Committee, with but one or two exceptions, had, however, determinedly refused, and had even inserted a provision in the Sundry Civil Bill of 1876–7, extinguishing the Board, when the money in hand should have been expended. That provision remains a law. The appropriation in hand will be expended during the coming year, 1877–8, and this Board, which has been procured and sustained by such earnest action, and so great an amount of hard work on the part of members of the Society, will be disbanded just as its plans and methods are thoroughly settled, and are bringing forth abundant fruit, and just as it is ready to undertake the most important of its researches —that on large parts of structures.

It is possible that earnest and determined action on the part of the Society, may preserve it, but it can only be done by taking steps as well as convince the members of the House Committee on Appropriations of the next Congress:

1st. That this work is of national importance in developing our mineral resources and manufacturing industries, and in securing safety of all large constructions; that it is an absolute necessity.

2d. That no individual can do such work, and that no combination of private individuals can make a complete and satisfactory investigation, even were the results of such work likely to be accepted as authoritative as would be the right of a Government Commission.

3d. That, while this work is as appropriately a matter of general legislation as the support of the Patent office, the Department of Agriculture, or of topographical and hydrographical surveys, it will secure returns of incalculable value, with insignificant expenditure.

If members of the House Committee can be shown these facts, the Board may possibly be continued; but it will only be by such convincing evidence as will fully controvert their preexisting ideas, and convert them to a broader and more liberal faith. The Board has been formally endorsed by this Society, by the American Institute of Mining Engineers, the Iron and Steel Association, by all the technical schools, and by other institutions of learning, and has kept the members of the committees informed of the progress of its work. It has not been successful in securing proper recognition, notwithstanding all this, and notwithstanding the efforts of prominent men of both parties in Congress. It has done all that it, in propriety, can do, and will probably now simply present its reports on Committee work, state its readiness to go on with the greater work assigned it, and leave the matter to be decided as shall be determined by the House Committee on Appropriations, in the light of such evidence as they may thus be given. There is imminent danger that the Board will be discharged before it can report on the strength of a single 15 inch beam, or determine a single law relative to the resistances of parts of structures. It has, however, accepted its duties, and has undertaken them in good faith. Its members have discharged their duty faithfully, so far as they have been permitted, and have devoted, voluntarily, a vast amount of time to special research without compensation, and their only regret will arise from a natural reluctance to see their work interrupted, just when most certain to prove useful, and from the disappointment which, in common with all interested in the movement, they must feel at this premature interruption of a great and needed work.

They will find some slight compensation in the facts that they have, at least, organized a scheme which may, at some future time, be carried out by abler minds, that they have stimulated foreign nations to the consideration of the necessity of doing similar work, and thus, indirectly benefiting the world, and that they have been permitted to collect some valuable information in several important fields.

The speaker concluded by stating his belief that the importance of the subject and the evident and eager interest which had been taken in the matter by nearly all the members of the Society, would justify him in having so fully and freely stated the present status, and probable future of the Board appointed to test Iron, Steel and other Metals.

IRON AND STEEL NOTES.

Silicon Pig Iron.—At a recent meeting of the American Institute of Mining Engineers at Wilkes-Barre, Pa., Mr. Holley gave some account of the manufacture of silicon pig in France. This is a special pig, made as spiegel is in the blast furnace, containing as high as 10 per cent. silicon and some manganese. In using it a bath is made of spiegel, of ferro-manganese, and rail ends, etc., are put in. When it gets to a state of fluidity that would in the ordinary process require the ferro-manganese, the silicon pig is put in. The castings are free from blow holes. The ferro-manganese in the bath serves a very important purpose—to keep out the oxygen. If oxygen is present it is taken up by it. The casting has almost the same physical properties as hammered steel. Another peculiarity is that it has a specific gravity higher than Whitworth'special steel—higher than hammered, and very much higher than ordinary steel. Another feature is that hammering does not seem to improve or injure it.—*Am. Manuf.*, xv, 7.

STEEL FOR SHIPBUILDING.—When H.M.'s ironclad Nelson was launched by Messrs. Jno. Elder and Co., on the Clyde, last November, Mr. Barnaby, the Chief Constructor of the English Navy, was present, and in the course of some remarks he said that that firm were building for the Admiralty six vessels constructed of a new material, viz., steel. The introduction of steel for armor had frequently been referred to. They had made in England some experiments in this line with steel—an extremely "touchy" sort of material, something like flint glass, which would not break although it was thrown down six times, but perhaps if it were thrown down a seventh time it would fly all to pieces. The French had gone ahead of the English in this respect, and had produced an armor plate 12 ft. long, 4 ft. 6 in. wide, and 22 in. thick. That plate cost £2,000. It was put upon the side of a ship, a shot from a 100-ton gun struck it, and it went all to pieces. Steel might perhaps be used for armor some day, and he trusted Britain would not be behind other nations in introducing and using it in a proper manner; but at present he thought they must be very careful how they changed from well-made Sheffield iron plates to steel plates.—*Journal of Iron and Steel Institute*

RAILWAY NOTES.

STEEL RAILS IN THE UNITED STATES.—In the course of 1876 the Lake Shore and Michigan Southern Railroad Company laid 10,500 tons of steel rails upon its system. These rails, the whole cost of which was charged to revenue, covered 112 miles of line. There are now but 263 miles of iron rails in the track of the main line, and these remaining iron rails are to be replaced with steel rails as soon as possible. The use of steel rails appears to have had the effect of largely reducing the permanent way charges last year.

ROLLING STOCK ON LAKE SHORE AND MICHIGAN SOUTHERN.—At the close of 1876 the Lake Shore and Michigan Southern Railroad Company had nine more cars than at the commencement of the year. The aggregate car stock of the company was thus carried at the close of 1876 to 10,546 vehicles. The number of locomotives (495) owned by the company remained unaltered last year. The amount expended by the company last year in the maintenance of its rolling stock was 1,403,835 dols. The amount expended for new equipment in the six years ending with 1875 inclusive was 5,904,087 dols.; with this expenditure the company acquired 223 new locomotives and 4739 new cars.

RAILWAY EXPERIMENTS.—The Springfield (Mass.) *Republican* of recent date says:—A very interesting series of experiments have been in progress on the Boston and Albany road the past few days by means of the dynagraph-car of the Eastern Railway Association, in charge of P. H. Dudley, which has been run between Springfield and Worcester on both frieght and passenger trains to test the relative amount of power required at different points along the road, especial reference being had to the Springfield and Charlton grades. The experiment on the Modoc train, leaving Springfield at 6.30 a.m., which on the day in question consisted of two sleepers, four passenger and baggage-cars and the dynagraph-car, showed power required as follows:—For the first 2920ft. out of the depot the tension on the draw-bar was 6526 lb.; for the next mile 6460 lb., the rate of speed being 32 miles per hour; for the next 6200 lb., the speed being 36 miles, and for the last 1100ft. to the top of the grade 6250 lb. The last mile required the engine to produce 19,625,800 foot-pounds of power per minute. In going up the grade from East Brookfield to Charlton, beginning at the station, the tension on the draw-bar for the first 3880ft. was 5722lb.; for the first full mile, the velocity being 37.5 miles, 4280 lb.; for the second mile, with 37 miles velocity, 5232 lb.; third, with 36 miles velocity, 5450 lb.; fourth, which contains a sharp curve, with 37 miles velocity, 5612 lb.; fifth, with 41 miles velocity, 5230 lb.; and sixth, which ran a little past the summit at Charlton, 4356 lb. The engine had an 18in. by 24in. cylinder, and the track was in excellent condition. The maximum of the Springfield grade is 60ft. to the mile and the Charlton grade 51.47ft. At the sharpest curve the grade is about 49ft. Similar experiments were made on a freight-train of 27 cars drawn by the Adirondack, famous for her trials with the Mogul engine last summer, and showed that the tension on the draw-bar going up Springfield grade at a speed of 5.9 miles per hour was about 16,000 lb.; and the average strain going up Charlton grade at an average speed of about nine miles per hour was 14,500 lb., the power required in the first instance being 84,840,000 foot-pounds. Near the top of the grade the power of the engine was tested by applying the brakes, and it was found that running at four miles per hour, the engine could exert a tension of 17,000 lb. Beyond this point the drivers would slip and little progress was made. Really, the most important experiments in which the association is just now engaged are in testing the quality of iron and steel used for bridges, rails, axles and car wheels. Recent trials of the tenacity of iron used for various bridges and car axles indicate that much of the iron now in use will only stand about two-thirds the strain which it is guaranteed to resist. For instance, some iron now being put into a new bridge at the East, which is supposed to stand a pressure of 60, 000 lb. to the square inch, breaks readily at 40,000 lb., and a car axle supposed to be equal to 110,000 lb. snapped at 70,000 lb. When it is borne in mind that the calculations of bridge-building engineers are based on the guaranteed strength of the iron, the reason for the fall of iron bridges becomes apparent at once, and instead of wondering at an Ashtabula horror, the wonder rather is that it is not repeated. The Eastern Railroad Association, which is making these experiments, represents all the railroads on the Atlantic coast north of Richmond, Va., and east of Pittsburg and the Alleghanies and was organized about ten years ago, has for its object the investigation of the validity of patents and claims to royalties for the use of the same. S. M. Whipple, of South Adams, is the general agent- The scope of the association has naturally broadened, and it has been for the past few years largely engaged in testing the merits of various railway equipments with the idea of getting the best in every department. The dynagraph-car is a curiosity in itself, containing, besides the dynagraph, which is an ingenious instrument registering exactly the amount of power required to pull a train, a chronograph which records the speed of the train every 7½ seconds, an anemometer which registers the velocity of the wind, whether natural or caused by the motion of the cars, and a complete set of instruments for testing the hardness, tenacity, ductility, density, and the amount of carbon in rails, axles, &c.

ENGINEERING STRUCTURES.

Proposed Great Irrigation Works for the Rhone Valley.—For some years the breeding of the silkworm and the cultivation of the vine and of the madder-plant in the valley of the Rhone have suffered from various causes. The result has been a loss in the three branches of industry of about £3,200,000 yearly. It may be guessed what an amount of suffering such a reduction must be causing to thousands of families depending for their support upon them. M. Aristide Dumont, civil engineer, has proposed to cover this deficit by raising the cultivation of the soil and the breeding of cattle, as well as providing a remedy for restoring to health vines attacked by Phylloxera, by means of large irrigation works. His project, including the construction of an irrigation canal 310 miles long, is ably set forth in an article in the *Revue des Deux Mondes*, by M. F. Vidalin, of which we propose to give a short summary.

The importance of the project is evidenced by the facts that it has already been submitted to the Conseil des Ponts et Chaussees, that twenty-four local committees have been formed, instructed to collect data as to the consumption of water and its distribution, and that subscriptions have been received for the irrigation of 13,000 hectares (32,125 acres) of soil. It is pointed out that those most interested in the great undertaking are the railways, on account of the increased transport of agricultural produce ; the landed proprietors, on account of the greater yield of forests, meadows, and vineyards ; the different communes, by increasing the facilities for the supply of water ; industry generally, by placing larger moving power at its disposal ; trade and commerce, to both of which a new means of communication will be opened ; and finally, the country as a whole, by larger receipts flowing into the public exchequer in the shape of increased tax-paying power.

The cost of construction of the principal canal and its feeders is estimated at £4,800,000., of which the State is to contribute £1,200,000., equal to a third of the outlay for constructing the principal canal. The remaining £3,600,000. are to be raised by subscription, to be paid back in ninety years, in annual instalments of £54, 000, with interest at 4½ per cent.

The canal is to branch off from the Rhone below Vienne, near the rocks of Condrieu, and thence to follow the left bank of the river, with a fall of 0.24 meter per kilometer (15.26 in. per mile). The fall of the Rhone exceeding this ratio, a considerable difference in the two levels would soon be established. The canal is to extend above Valence and Montbeliard, to cross at the defile of Mornas in large syphons to the right shore, and to draw on its further course towards the most important towns of Southern France, Nimes, Lunel, Montpellier, Beziersi, and Narbonne. With a length of 310 miles, it would affect 220,000 hectares, or 543,650 acres, of soil to be irrigated, and offer a water supply to the 500,000 inhabitants of the above-named towns.

It is proposed to take from the Rhone 30 cubic meters (6,600 gallons) per second, and the same quantity from other rivers met with by the canal in its course. It is shown that the flow of water of the Rhone is 400 cubic meters (88,000 gallons) per second, that the Cavour Canal takes 110 cubic meters (24,210 gallons) from the river Po, and that the canal constructed at the foot of the Himalayas takes from the Ganges seven-eighths of its volume, or 44,000 gallons per second, in order to irrigate the so-called Doab for a length of 372 miles. The whole consumption of the Rhone Canal during the summer months, from April 15 to September 15, is estimated to be 930,000,000 cubic meters, and during the remainder of the year 700,000, 000 cubic meters, corresponding to the proportions of the flow of the Rhone, which, as is well known, is mainly fed by the glaciers sending their waters down valley during the hot season of the year. During summer the meadows, during winter the vineyards, are to be irrigated. The yearly closing of the canals would have to take place in winter.

If for a surface representing a hectare 10,000 cubic meters of water are reckoned for irrigation, only 62,200 hectares could be watered in summer, and in case of more being required, the contents of the Rhone alone would have to be drawn upon, as it would not be advisable to withdraw more from its tributaries, on account of the works the power of which they supply. The requirement for a hectare of vineyard is estimated to be at least 6,000 cubic meters.

The rate of payment per 10,000 square meters to be irrigated is fixed at 63 francs, without regard to their mode of cultivation ; in Upper Italy it is stated to be 75 francs. Irrigation would raise the yearly rent of a hectare of dry soil from 50 francs to from 150 to 200 francs. The power to be let out for industrial purposes for fifty years is estimated at 5,000 horse-power, a rent of 200 francs per year and power being expected. It is hoped that, in concequence of the great saving effected (a horse-power, if steam is applied, costing at least 500 francs), a large number of industrial undertakings will spring up between Vienne and Mornas, along the Paris-Marseilles line of railway. The new irrigation canal would offer to navigation between Condrieu and Mornas a way of transport, nearly 50 ft. wide and 10 ft. deep, which, with little extra cost, might be made one of the most perfect waterways in existence, and which it might be desirable to continue as far as Marseilles and the Mediterranean. Its execution would ensure to sea-going ships of not more than 300 tons burthen the traffic between Havre, Paris, and Marseilles, compete with the St. Gothard Railway, and be to France a peaceable revenge for Sedan.

Of the great works to be undertaken in connection with the canal, the great syphon nearly two miles long, and to be constructed of sheet-iron, near Mornas, is especially mentioned. The inlet to the pipes is to be situate in a gigantic reservoir placed 230 ft. above the level of the valley; the pipes are to cross the Rhone on a bridge, and the water conducted in them is to be emptied into a large basin on the other side. The cost of the syphon is estimated at £280,000.

ORDNANCE AND NAVAL.

IMPROVEMENTS IN CASTING SHELLS.—While guns and carriages have been developing, it is satisfactory to find that our projectiles are able to keep pace with them. This is by no means the matter-of-course thing that it might appear. The casting of a shell of 2,000 lbs. weight is not, of course, difficult because of its magnitude as a casting, for it is only a large one as compared with other shells. The difficulty lies in the peculiar requirements of the case. The chilling of a Palliser projectile of this size, for example, is clearly a more difficult matter than chilling a small one; but there are questions connected with shells of greater importance than this because of much wider application. The strains that fall on a shell when in the bore of a gun are so great as to call for a degree of excellence in the metal, and of soundness in the casting, such as has an interest for all those who have to do with foundries. When shells were made with a diameter less than that of the bore of the gun from which they were fired by about 0.08 inches, the gas generated on the explosion of the charge rushed past them, pressing the sides inwards to an extent that would scarcely be credited. We remember one shell of a nine inch gun that was measured very carefully after firing, which had its full diameter of 8.92 inches at the base, where it was completely supported, 8.90 at the head where the support was less direct, and only 8.55 near the middle; the shell having been thus pinched in towards a dumb-bell form to the extent of 0.37 inches, which in a cast iron hollow cylinder, as it is, with walls 1.5 inches thick, argues an amount of pressure that would hardly have been conceived was possible from the gas only escaping by windage, and acting only for the very short space of time during which the shell remains in the bore of the gun. Projectiles from our heaviest guns are now being fired with a copper gas check, which entirely reverses the state of things. For as there is now practically no windage and no escape of gas, the projectile experiences simply the strain of violent setting up which must come upon it, rather more suddenly and violently than when windage existed, other conditions being the same, We should expect a shell fired with a gas check to have its diameter slightly increased at the centre. We may also observe by the way that the soundness of the shell at the bottom is very severely tested both as to its absolute strength and the liability of the gas to force an entrance through any crevice that may exist, especially round the bush of the loading hole of a Palliser shell, which is, as many of our readers know, at the base. The question, however, that we want now to consider, as one of special interest to founders, is that of the actual strength of the iron shell. Until recently it was the practice to turn the external surface off the shells after they came from the foundry, by which any imperfections or scales in the surface were removed, and the shells by one very simple operation were brought to the required dimensions within the desired limits of manufacture. It has latterly, however, been discovered that a great sacrifice of strength was made by the removal of the outside skin of the casting, and means have been found to turn out the shell from the foundry in a condition that left all turning operations unnecessary. Indeed the perfection of the casting is such that no one would compare the turned shells with those that are now issued with their skin untouched after they leave the foundry.

We are now only speaking of what is open to the public. We should think it was probable that if application were made to the Superintendent of the Royal Laboratory, Colonel Fraser, he would be able to supply much information of special value to founders, for no one who saw the castings that are now being turned out in this department could fail to be very much struck by their perfection of form and the soundness of their surface. What is the extent of the gain in strength by the retention of the skin in place of its removal cannot be said, but by inference we should think it considerable. Now comes the question, by what means is this result achieved? We always feel on delicate ground when speaking on questions of Government manufacture. We think, at all events, there can be no objection to pointing out what is open to any visitor who has eyes, ears, and a certain amount of knowledge. Our attention was called first to a change in the system of casting by a very simple thing. We happened to be asked to take a friend to visit the shell foundry, and we pointed out some metal in a ladle, speaking of its probable quality from the appearance of the break on its surface, when we became aware that the metal was not being poured as we expected into Palliser shell moulds but into those for common shell. Consequently it was clear that the breaking had not yet begun to take place rapidly, and we had been deceived as to its quality by the fact that it was being poured in a much hotter conditon than was formerly the case. We believe that this will be noticed to be the state of metal that is being cast throughout the foundry, and this may probably have much to say to the results that are being achieved. At all events the fact is plain that castings of extraordinary excellence are being turned out, and we should call the attention of iron-masters to the fact, with the suggestion, that in all probability, if they considered it worth their while to apply to the Secretary of State for War, they might obtain information that might be very valuable, and we cannot suppose there would be any objection to supply such information on the part of the authorities.—*Engineer.*

BOOK NOTICES.

NEW CONSTRUCTIONS IN GRAPHICAL STATICS. By HENRY T. EDDY, C. E., Ph. D., Professor of Mathematics and Civil Engineering in the University of Cincinnati. Illustrated by ten engravings in the text and nine folding plates. New York: D. Van Nostrand. Price $1.50.

Readers of this Magazine do not need to be reminded of the value of this work, which is a reprint of the articles published in the Magazine from January to August. We quote the author's preface:

"At a meeting of the American Association for the Advancement of Science, held in August, 1876, at Buffalo, the writer read two papers, entitled respectively, 'A New Fundamental Method in Graphical Statics,' and, 'Certain New Constructions in Graphical Statics.' The latter paper furnishes the basis of the following pages.

"Most of the problems proposed have, it is thought, never been solved heretofore by graphical methods, though partial solutions have been obtained in certain cases.

"The possibility of obtaining a direct and complete solution of the various forms of the stiff arch rib is found to depend upon a theorem not hitherto recognized, as to the manner in which the equilibrium curve due to the applied weights is made to coincide as nearly as possible with the curve of the arch, which itself acts as a partial equilibrium curve. It is the difference in position of these two curves which is the measure of the bending moment in the arch. The solution of the arch is further simplified by showing that it depends upon that of a straight girder of the same cross section.

"The theorem above referred to, which may be properly named the 'Theorem Respecting the Coincidence of Closing Lines,' may be considered to occupy in relation to this subject, a place analogous to 'Gauss' Theorem of Least Constraint' in Dynamics, or 'Moseley's Theorem of Least Resistance' in Statics, and we may perhaps add to that of 'Legendre's Method of Least Squares,' in the Theory of Observations.

"Those who are acquainted with the intricate formulæ used in the analytic solution of this problem are aware that the actual relations are so covered up by these complications that from them a clear understanding of the manner in which the thrust, moment, and shear depend upon the applied weights is difficult, perhaps impossible. But it is hoped that the graphical investigation, which affords a pictorial representation, so to speak, of these quantities and their relations, may present no such difficulties. And further, the thrust, moment, and shear due to changes of temperature, or any cause which alters the span of the arch, are, it is believed, here for the first time obtained by a graphical process.

"A new general theorem is also enunciated, which affords the basis for a direct solution of the flexible arch rib, or suspension cable, and its stiffening truss.

"These discussions and constructions have led to a new investigation of the continuous girder in the most general case of variable moment of inertia. This investigation furnishes a complete graphical solution of the problem, and is accompanied by an analytic investigation in which the general formulæ appear for the first time in simple form.

"Another problem treated is that of the arch having block-work joints, such as are found in stone or brick arches, a case intermediate between the stiff and the flexible arch. A complete graphical solution of this problem was proposed by Poncelet, which the reader will find given by Woodbury in the case where the arch and load are symmetrical about the crown. The solution proposed is far simpler, susceptible of greater accuracy, and is not restricted by considerations of symmetry. Woodbury states that the solution given by him is correct for an unsymmetrical arch, but in this he is mistaken.

"The graphical construction for the stability of retaining walls is the first one proposed, so far as known, which employs the true thrust in its real direction, as shown by Rankine in his classic investigation of the thrust of homogeneous solids. It is in fact an adaptation of that most useful conception, 'Coulomb's Wedge of Maximum Pressure' to the results of Rankine's investigation.

"It has been found possible to obtain a complete solution of the dome by employing constructions analogous to those employed for the arch; and in particular it is believed that the dome of masonry is here investigated correctly for the first time, and the proper distinctions pointed out between it and the dome of metal.

"Finally, it may perhaps be said with truth, that neither of these problems can be solved with the same generality by analytic processes as by a graphical construction. The analysis almost always demands some kind of law or uniformity in the loading and in the structure sustaining the load, while the graphical method treats all cases without increase of complexity; and especially are the cases of discontinuity, either in the load or structure, difficult by analysis but easy by graphics."

The following is the table of contents: Introductory Formulæ and Theorems; Arch Rib with Fixed Ends; Arch Rib with Hinge Joint at the Crown; Temperature Strains; Arch Rib with End Joints; Arch Rib with Three Joints; Arch Rib with One End Joint; Arch Rib with Two Joints; Suspension Cable and Stiffening Truss; Continuous Girder with Variable Cross Section; Theorem of Three Moments; Flexible Arch Rib and Stiffening Truss; Arch of Masonry; Retaining Walls and Abutments; Spherical Dome of Metal; Spherical Dome of Masonry; Conical Domes of Metal and of Masonry.

NOUVELLE THEORIE DE LA POUSSEE DES TERRES. Par J. CURIE. Paris: Gauthier-Villars. For sale by D. Van Nostrand. Price $3.00.

Besides the Pressure of Earth, the author, almost of course, treats of the Stability of Retaining Walls. It is rare that so much space is devoted to this topic. It is an octavo volume of 285 pages, and is illustrated with thirty-seven elaborate cuts interspersed in the text, and six folding plates.

PHENOMENES PHYSIQUES DE LA PHONATION ET DE L'AUDITION. PAR J. GAVARRET. Paris: G. Masson. For sale by D. Van Nostrand. Price $4.00.

A new interest attaches to the subjects of sound and hearing, since the invention of the telephone.

The work before us which is a royal octavo volume of about 600 pages treats of the latest theories and especially of modes of experimenting employed by physicists. It covers about the same ground as Tyndall's lectures on sound. The illustrations are of excellent quality and number about one hundred.

BRITISH INDUSTRIES—HORTICULTURE. By F. W. BURRIDGE. London: Edward Stanford. For sale by D. Van Nostrand. Price $2.25.

This is the latest addition to this valuable series of technical works, but it is by no means the least important or least interesting to general readers. The author ably, but briefly, discusses the following topics: Commercial Gardening; Fruit Culture; Vegetable Culture; Culinary Vegetables; Salad Vegetables; Herbs; Decorative Plant Culture; Fruit and Vegetable Preserving; Plant Propagation; Hybridizing and Cross Breeding; Public Gardens; Gardening Industry Abroad; Collateral Industries of Gardening.

Several illustrations embellish the work.

A TREATISE ON ENGINEERING CONSTRUCTION: Embracing Discussions of the Principles Involved, and Descriptions of the Material Employed in Tunneling, Bridging, Canal and Road Building, etc., etc. By J. E. SHIELDS, C. E New York: D. Van Nostrand. Price $1.50.

Many and various subjects are treated in this little book, and all of them with exceeding brevity. It is a collection of rules, directions and formulas for the use of working engines, derived from the experiences of a thoroughly practical man, and are such as have been verified by frequent use.

The whole is presented under four general heads; viz, Foundations, Masonry, Tunnels and Engineering Geodesy.

In the first of these divisions are treated the characters of natural soils, and the various artificial constructions which serve as a base for engineering structures; among these latter are Planking, Concrete, Beton, Caissons, Piles, etc.

Under Masonry are presented the laws of Statical Equilibrium of Stone and Brick Arches, and Retaining Walls, together with the empirical rules sanctioned by long use.

"Tunnels" is made to include brief directions regarding Mortars and Cements; the character of stones both as regards strength and chemical composition. The construction of shafts and tunnels proper has, of course, a fair share of space; so also has quarrying and blasting, and the directions for selecting good timber.

Engineering Geodesy includes some brief rules for solutions of problems in what is called Higher Surveying in the text books; such as Finding the Latitude; Determining the Meridian; Leveling by the Barometer, etc.: also such miscellaneous rules and tables as could not properly be classified under either of the preceding heads.

Forty-two interspersed cuts illustrate the text.

REPORT OF CHIEF ENGINEER J. W. KING, UNITED STATES NAVY, ON EUROPEAN SHIPS OF WAR AND THEIR ARMAMENT, NAVAL ADMINISTRATION AND ECONOMY, MARINE CONSTRUCTIONS AND APPLIANCES, DOCKYARDS, &c. Washington: Government Printing Office.

The scope of this report is well explained by its title, and when we say that the promise of the title has been well fulfilled, it will be evident that Mr. King has produced a volume which, at the present time especially, possesses more than ordinary interest. It may appear somewhat curious to have to refer to a report to a foreign government to obtain information respecting our own naval force, but it is nevertheless a fact that no book published in this country contains so full an account of the vessels of our navy and their armament, &c., as is contained in Mr. King's report. We ourselves have from time to time published accounts of all new vessels added to our navy, and Mr. King has, as he acknowledges, availed himself of our pages, while he has also drawn information from other current publications and Government blue-books, but apart from the facts thus gleaned, he has collected a vast number of other particulars which have not, so far as we are aware, been published at all, while he has supplemented his descriptions by numerous sketches illustrating special points of arrangement and detail.

The whole report consists of 273 octavo pages, and of these 130 are devoted to descriptions of vessels in the British Navy, while about 80 pages more treat chiefly of our marine engines and boilers, our dockyards, and our naval administration, so that the navies of other European countries are dealt with briefly by comparison. Seeing that Mr. King evidently has the capacity for collecting information, the fair deduction to be drawn from his report is that the opportunities afforded abroad were not so great as he met with in this country, but he has nevertheless managed to collate a number of facts relating to foreign navies which just now possess much interest.

In the section of his report treating of marine engines Mr. King points out the overwhelming evidence in favor of the compound system, and says: "As yet, by no satisfactory device except compounding have great expansion and consequent economy of fuel been obtained at sea. In the face of these facts, further discussion on the subject of adopting the compound engine for the vessels of our own navy is as useless as would be the discussion of the relative merits of the screw propeller and paddle wheel for ships of war." Mr. King also devotes a special section of his report to boilers, another to sea-valves and cocks, and steering gear, and others to torpedo boats, &c.

Altogether the report under notice contains much valuable and interesting information, and it forms a volume very creditable to its author.—*Engineering.*

MISCELLANEOUS.

DANUBIAN STEAM NAVIGATION COMPANY.—It appears that at the close of last year, this company possessed 196 steamers of aggregate force of 17,490 horse power. The company's revenue for 1876 amounted in round figures to 1,230,000*l.*; the profits realised for the year were about 140,000*l.*

M. A. BERTRAND has recently experimented upon electro-plating with aluminum, magnesium, cadmium, bismuth, antimony, and palladium. He had obtained deposits of aluminum on decomposing with a strong battery a solution of the double chloride of aluminium and ammonium. A plate of copper forming the negative pole, whitens gradually, and becomes covered with a layer of aluminum, which takes a brilliant polish under the burnisher. The double chloride of magnesium and ammonium in an aqueous solution is readily decomposed by the battery, giving in a few minutes strongly adherent and homogeneous deposits of magnesium upon a sheet of copper. It polishes readily. The battery must be powerful. Cadmium is best deposited from the bromide to which a little sulphuric acid has been added. It is then very coherent and very white, and takes a fine polish. The sulphate, if acidulated, also gives an immediate deposit of metallic cadmium, very adhesive, and capable of a fine polish. Bismuth is deposited from a solution of the double chloride of bismuth and ammonium upon copper or brass by the current from a Bunsen element. It is very adhesive; though mat, it is capable of taking a fine polish, and may be introduced in the decoration of objects of art. Antimony can be deposited from a solution of the double chloride of antimony and ammonium at common temperatures. It frequently serves to replace platinum-black in a number of fine art manufactures. Deposits of palladium are obtained with ease by means of the double chloride of palladium and ammonium, either with or without the battery. The solution must be perfectly neutral.

NOVEL METHOD OF DEEP SEA SOUNDING. —At a meeting of the Royal Society Dr. Siemens exhibited the instrument he has devised to ascertain the depth of the sea by a new means without using a sounding line. He has worked out the requirements, starting with the proposition that the total gravitation of the earth as measured on its normal surface is composed of the separate attractions of all its parts, and that the attractive influence of each equal volume varies directly as its density and inversely as the square of its distance from the point of measurement. The density of sea water being about 1.026 and that of the solid constituents composing the crust of the earth about 2.763 (this being the mean density of mountain limestone, granite basalt, slate and sandstone), it follows that an intervening depth of sea water must exercise a sensible influence upon total gravitation if measured on the surface of the sea. Dr. Siemens showed how this influence can be proved mathematically in considering, in the first place, the attractive value of any thin slice of subtance in a plane perpendicular to the earth's radius, supposing that the earth is regarded as a perfect sphere, of uniform density, and not affected by centrifugal force.

It was in 1859 that Dr. Siemens first attempted to construct an instrument based on these principles. The difficulties he then encountered he has since overcome, and the present instrument is the result of his latest work. He proposes to call it a bathometer, and it consists essentially of a vertical column of mercury contained in a steel tube having cup-like extensions at both extremities, so as to increase the terminal area of the mercury. The lower cup is closed by means of a corrugated diaphragm of thin steel plate, and the weight of the column of mercury is balanced in the centre of the diaphragm by the elastic force derived from two carefully-tempered spiral steel springs of the same length as the column of mercury. One of the peculiarities of this machanical arrangement is that it is parathermal, the diminishing elastic force of the springs with rise of temperature being compensated by a similar decrease of potential of the mercury column, which decrease depends upon the proportions given to the areas of the steel tube and its cup-like extensions. The instrument is suspended a short distance above its centre of gravity in a universal joint, in order to cause it to retain its vertical position, notwithstanding the motion of the vessel; the vertical oscillations of the mercury are almost entirely prevented by a local contraction of the mercury column to a very small orifice. The reading of the instrument is effected by means of electrical contact, which is established between the end of a micrometer-screw and the centre of the elastic diaphragm. The pitch of the screw and the divisions upon the rim are so proportioned that each division represents the diminution of gravity due to one fathom of depth. Variations in atmospheric pressure have no effect on the reading of the instrument, but corrections have to be made for latitude. The instrument has been actually tested in voyages across the Atlantic in the *Faraday*, and the comparisons with Sir W. Thompson's steel wire sounding apparatus showed it was very reliable.

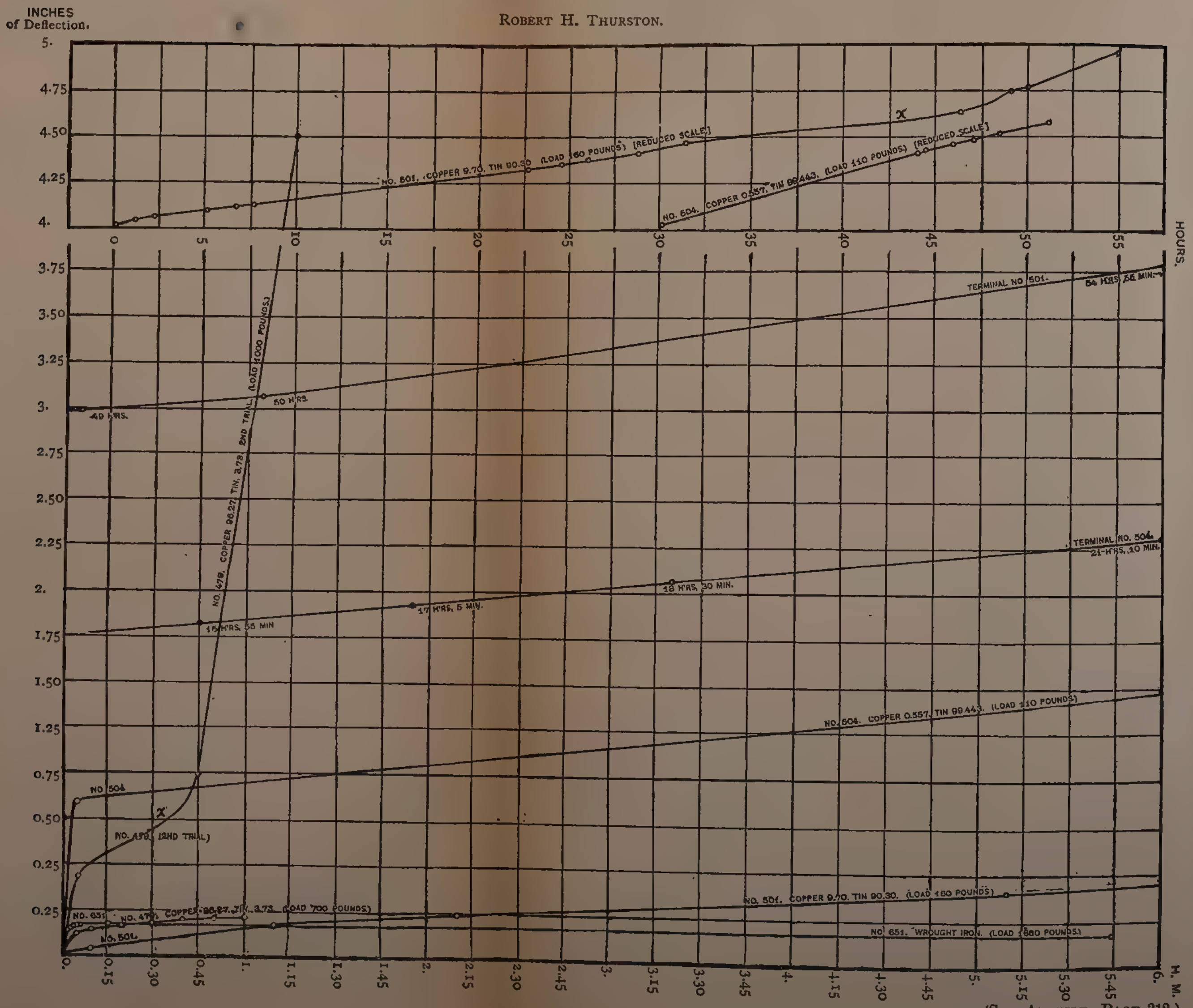
The paper concluded with pointing out many ways in which the instrument might be of use; among others was that of indicating approaching danger if contour lines were first efficiently mapped.

PLATE II.

INCREASE OF DEFLECTION WITH TIME IN TRANSVERSE TESTS OF BARS OF METAL.

RATE OF SET OF BARS, 1 INCH SQUARE 22 INCHES, BETWEEN SUPPORTS.

ROBERT H. THURSTON.



(SEE ARTICLE, PAGE 312.)

VAN NOSTRAND'S

ECLECTIC

ENGINEERING MAGAZINE.

NO. CVI.—OCTOBER, 1877.—VOL. XVII.

CABLE-MAKING FOR SUSPENSION BRIDGES AS EXEMPLIFIED IN THE EAST RIVER BRIDGE.

By WILHELM HILDENBRAND, C. E.

Written for Van Nostrand's Engineering Magazine.

III.

III. THE MATERIAL AND THE MANNER OF WORKING IT INTO THE CABLE.

1. *The Cable Wire.*—In all bridge cables, constructed previous to those of the East River bridge, charcoal iron wire of either No. 10 or No. 9 gauge was used. The cables of the Niagara railway bridge, for instance, contain 3640 wires No. 10 gauge with an ultimate strength of 2658 tons, forming cables of 10 inches diameter. Those of the Cincinnati bridge—until now the largest suspension bridge ever built—are 12 inches in diameter and consist of 5200 No. 9 wires with an aggregate strength of 4212 tons. In comparing with these the cables of the East River bridge, we find that the strength required for the latter exceeds over two and a half times the strength of the former cables. If composed of the same material, their bulk and the time required for making them would be increased in the same proportion, two items, which it is desirable to have reduced as much as possible. It was therefore concluded to manufacture these cables of steel-wire. In order to determine the size of the wire and the most advantageous quality of steel, very thorough experiments, continued over a series of years, were made with all kinds and grades of steel-wire of different sizes in regard to their tensile strength and ductility. As the result of these tests a wire was decided upon, as described in the following extract from the specification :

"The wire must be made of steel of superior quality, must be hardened and tempered and, lastly, must be galvanized. The size of the wire shall be No. 8 full Birmingham guage. A length of 14 feet must weigh exactly one pound before it is galvanized.

"Each must have a breaking strength of no less than 3400 lbs. This corresponds in wire weighing 14 feet to the pound, to a rate of 160000 pounds per square inch of solid section. The elastic limit must be no less than $\frac{47}{100}$ of the breaking strength, or 1600 pounds. Within this limit of elasticity it must stretch at a uniform rate, corresponding to a modulus of elasticity of not less than 27,000,000, nor exceeding 29,000,000 pounds.

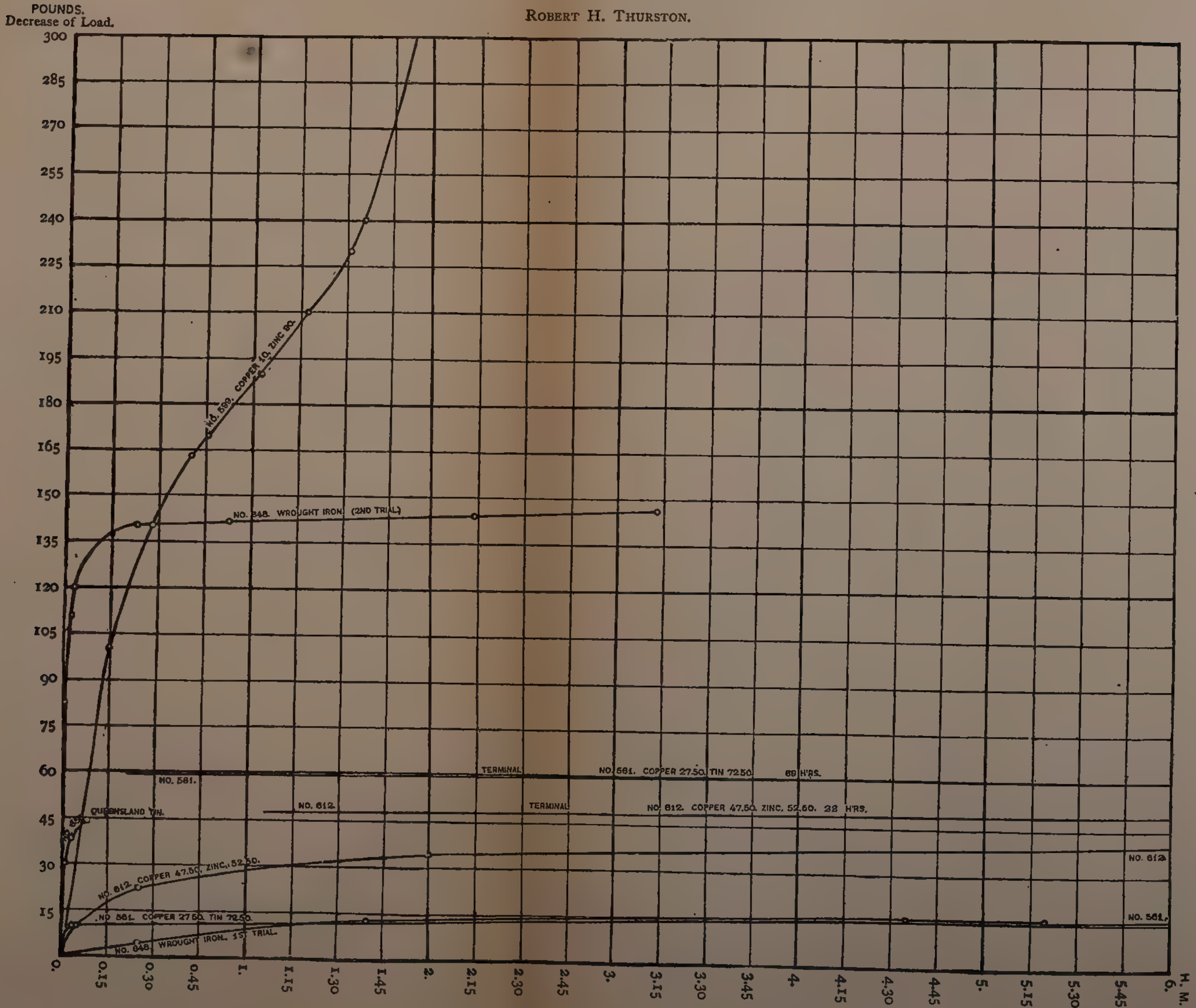
"All wire must be 'straight' wire;

PLATE_I.

DECREASE OF RESISTANCE WITH TIME IN TRANSVERSE TESTS OF BARS OF METAL.

RATE OF SET OF BARS, 1 INCH SQUARE, 22 INCHES BETWEEN SUPPORTS

ROBERT H. THURSTON.



(SEE ARTICLE, PAGE 312.)

that is to say, when a ring is unrolled upon the floor, the wire must lie perfectly straight and neutral, without tendency to spring back in the coiled form, as is usually the case. This straight condition must not be produced by the use of straightening machines, but by a patented process, which consists in leading the wire, from a point within the galvanizing trough, in a straight line under considerable tension to the guide sheave or winding drum, and in locating the drum at such a distance as to permit the wire to be cooled and set before it is coiled thereon.

"There will be two kinds of tests :

"*First.* A piece 60 feet long, from every fortieth ring, is placed in a vertical testing machine which has a vernier guage, capable of being read to $\frac{1}{10000}$ of one foot, so attached as to indicate the stretch of 50 feet of the wire. An initial strain of 400 pounds is now applied, which by successive increments of 400 pounds is increased to 1600 pounds. The amount of stretch for each of these increments shall be the same, and the total stretch between the initial and terminal strain shall not be less than $\frac{97}{1000}$ of one foot equal to $\frac{194}{100000}$ of the 50 feet. And furthermore, on reducing the strain to 1200 pounds, there shall be a permanent elongation not exceeding $\frac{1}{100000}$ of its length.

"The minimum strength of the wire, if subjected to a breaking strain, shall be not less than 3400 pounds, and the minimum stretch, when broken, shall have been two per cent. in fifty feet, and the diameter of the wire at the point of fracture shall not exceed $\frac{15}{100}$ of one inch.

"A piece 16 inches long, cut from every ring and a piece 6 feet long from one ring out of five, will be subjected to the same tests and must come up to the same standard.

"*Second.* Every ring will be subjected to a bending test, by cutting off from each ring a piece of wire one foot long and coiling it closely and continuously around a rod one-half inch in diameter, when, if it breaks, it will be rejected."

The contract, based on the above conditions, was awarded to J. Lloyd Haigh, of South Brooklyn, for 8$\frac{7}{10}$ cents per pound, with the distinct understanding that none but crucible cast steel shall be used for the wire.*

The wire is delivered in rings of 60–70 pounds, containing 800–1000 feet of the smaller, and 600–800 feet of the larger size.

Before being used, each ring receives three coats of oil, by dipping it first in a tank with raw, and then twice in one with boiled but cold linseed oil. Each coat of oil must be perfectly dry, before a new one is put on. After the oil has thoroughly hardened, the rings are ready to be spliced.

The splice used formerly for iron wire consisted in tapering the ends of the wires for about 2½–3 inches, laying the flat sides upon each other and wrapping the whole with thin wire. By a blow on a specially prepared steel mould, the circumference of the wire received a number of small nicks or notches, which prevented the wrapping wire from slipping. This splice proved fully equal to the strength of No. 9 iron wire; but applied to heavy steel wire it averages, if well made, only a strength of about 24–2700 pounds, hence is insufficient for wire exceeding 3400 pounds. Experiments made with different kinds of splicing gave the preference to a screw-coupling, which by the smallest bulk had the greatest percentage of strength, and finally the splice, as illustrated in Fig. 29, was adopted for the East River bridge cables.

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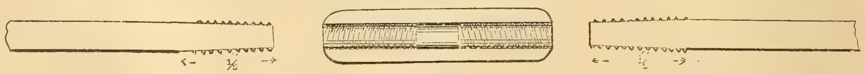
* The wire furnished by the contractor exceeded considerably the required ultimate strength, being in average 172,000 pounds per square inch. After two strands were completed with this wire, it was concluded to increase the size of the wire to 11 feet to the pound, equal to No. 7 Birmingham guage. This wire possesses a little smaller breaking strength, averaging about 170,000 pounds per square inch, but greater ductility. The principal advantage however, is, that instead of 332 wires, only 282 are necessary for one strand, which gives a saving in the time of manufacture of nearly one-sixth.

Fig.29



nat.size

The screw threads at the end of the wires have this peculiarity, that the thread instead of being cut in the rod, is raised above it, so that between each turn there is a short straight piece of rod. This result is obtained by tapering the ends of the wires a little and turning a cylindrical screw on this cone. By a screw length of $\frac{1}{2}$ inch for each wire, this coupling gives about 95 per cent. of the strength of the wire. After the ends of the wires are secured in the ferrule, the coupling is cleansed with a solution of potash, then dipped in molten zinc mixed with a little tin and finally painted with red lead.*

2. *Strandmaking.*—After a number of wire rings are spliced together, the wire is wound on the wire drum, passing on its way through a piece of sheepskin saturated with oil. Every thing else pertaining to traveling rope, shoe and leg connection, etc. being prepared, we are ready to stretch the wires for the strand.

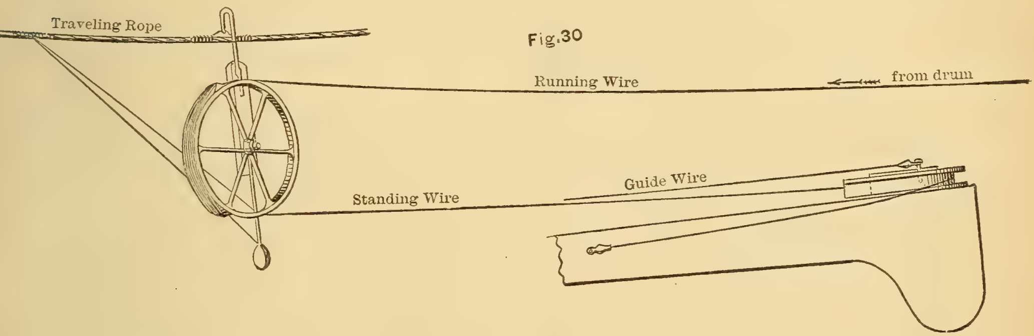
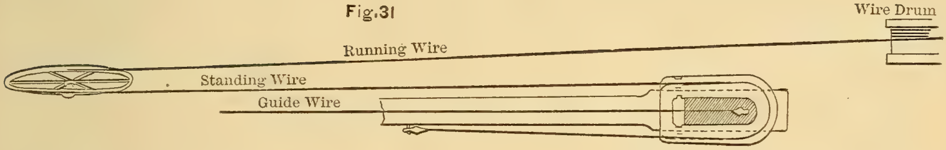


Fig.30

Fig.31



Figs. 30 and 31 show the rear part of the leg with the shoe and guide wire in position.

The end of the wire, with which the strand is started, is fastened to the side of the leg, passes around the shoe over the traveling sheave and back again to the drum. The lower wire, leading from shoe to traveling sheave, is called "standing wire," the upper, which uncoils from the drum and travels with twice the speed of the working rope, "running wire." In order to avoid confusion, it is necessary to preserve this distinction throughout the strand, and to keep each set always on the same side of the shoe. The standing wire occupies the inside, next to the traveling shoe, the running wire the outside of the sheave. The sag in the wires, while traveling across (see Fig. 1) is regulated by a brake on the wiredrum. As soon as the traveling sheave has passed the first tower, the standing wire is placed in the saddle and regulated in the first landspan. This is done by simply pulling it over the tower until it hangs parallel to the guidewire, and holding it in its place by a temporary twine lashing to some convenient object on or near the saddle. The running wire in the meantime travels on, supported by small wooden rollers outside of the saddle. After the traveling sheave has passed the second tower, the standing wire in middle span is immediately regulated in the foregoing manner, so that after its arrival at the second anchorage, only the last land span remains to be adjusted. This being done, the running wire is next thrown on the opposite side of the strand, and then regulated in the same way as the standing wire, but working in opposite direc-Brooklyn anchorage is the starting point; tions. At the East River bridge the hence all standing wires are regulated from Brooklyn towards New York, and the running wires from New York to-

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* To avoid the unscrewing of the wire from the ferrule during its passage across the river, which sometimes happened, the ferrule receives a little notch at each end, which being filled with zinc adhering to the wire, prevents its turning.

wards Brooklyn. For adjusting the wires a pair of nippers as illustrated in Figs. 32 and 33 are used, and the hauling is done with a small block and fall fastened to some convenient place on the anchor-chain. While the regulation of these two wires is going on, the traveling sheave, which carried them over, returns empty and another sheave fastened to the opposite side of the working rope, brings two wires for the strand of the second cable.

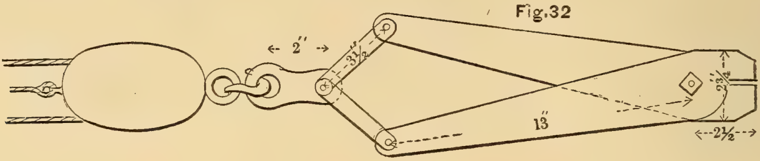


Fig.32

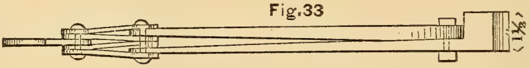


Fig.33

Scale ½ inch = 1 inch

About nine or ten days are required to lay a strand of 280–300 wires, if no delays happen. After the last wire of the strand is in place, it is cut and spliced to the end of the first wire so that the whole strand is formed of one continuous wire. The latter operation is a delicate one, because the ends have to be cut so that the deflection and length come exactly right. A few trials with a temporary splice are therefore necessary to be made.

The next operation consists in tying the two parts of the strand together so as to form a round, solid little cable, which at the East River bridge has a diameter of 3½ inches. For this purpose a so called "carriage," as illustrated farther below in Figs. 43 and 44, is placed on the strand on top of the towers, in which a few men slowly descend towards the anchorages and center of river, tying during the passage the strand every 16–24 inches with four turns of No. 14 annealed galvanized wire. Before being tied, the wires are sqeezed together by means of a pair of tongs, on the claws of which an iron sleeve, by knocking it towards the end of the arms, exercises great squeezing power.

The strand is now ready to be "let off."

This consists in relieving the shoe from its temporary seat on the leg, and to let it forward in its final position, at the end of the anchor-chain. It is an operation, which, with heavy strands, requires great care and foresight, because an accident here would not only be a loss of many thousands of dollars, but also would cause immense damage to property and shipping, and likely great destruction of life. It is therefore advisable, not only to make careful calculations, but also to have every rope and iron used in this operation thoroughly tested.

Fig. 34 represents the top view of the anchorage, with the full letting off arrangement for one of the outer strands of the East River Bridge.

Two heavy iron bars *LL*, fitting closely to the sides of the shoe, and bolted to a double steel plate in front of it, form the grasping arrangement. Figs. 35 and 36 show this on a larger scale.

At the end of the iron bars, are four 8 inch sheaves, which are attached by means of eight 1⅝ inch iron wire ropes to the four-grooved small sheave of a pair of blocks *B* and *A*. One of these is illustrated in Figs. 37 and 38.

Block *A* is the standing block, fastened stationary to a heavy log in the rear of the anchorage, by means of four 1⅝ in. wire ropes. It contains six iron sheaves of 23 in. diameter, connected to the sheaves of the running block *B* by means of a twelve folded 1¼ in. steel rope, the fall of which after passing around several rollers *C*, *D* and *E*, is attached to a four sheaved wooden block *G* at the other end of the anchorage. This wooden block is worked by a 1½ in. manila rope, which connects with the drum of a steam engine. This arrangement of course, may be changed to suit different local requirements. In the present case, its advantages are: first, that the wooden block *G* moves only in the middle line of the anchorage for letting off any strand of the four cables, this middle part being the only one free from obstructions; second, the length of the

Fig. 34

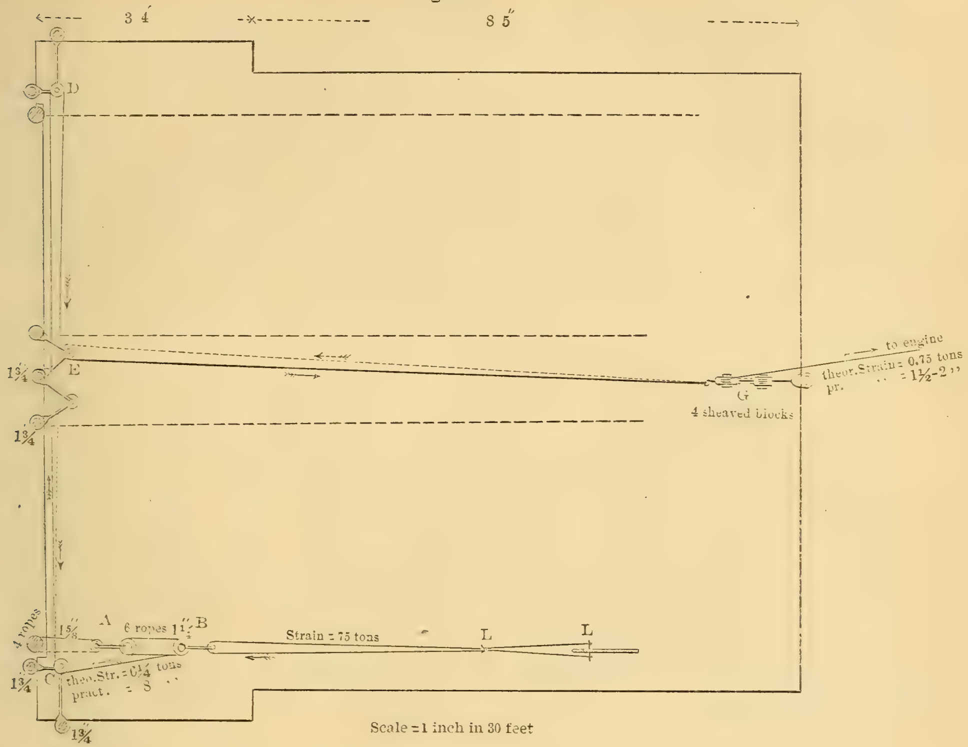


Fig. 35

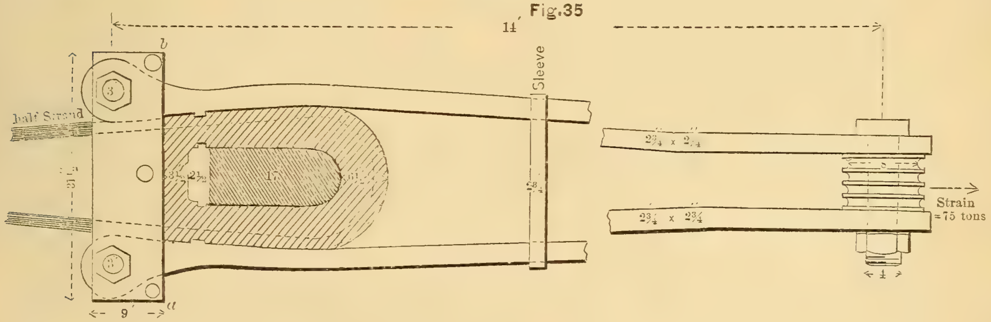
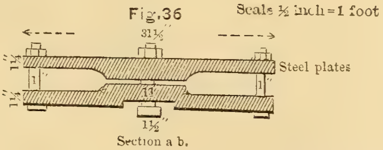


Fig. 36



anchorage not being sufficient for moving the shoe in one process the entire distance, it is necessary to do it in two processes, viz., to arrest the motion as soon as the wooden block arrives at the rear end, to take the fall rope from the intermediate sheave *E*, and to place it from *B* directly over *C*, occupying in Fig. 34 the position of the dotted line. With this the double distance *C E*, is gained in the length of the fall rope, and the wooden block can be placed again so much back, to continue the operation. An arrest of the motion and a relieve of strain in the fall rope, can easily be procured, by lashing the twelve ropes, between standing

Fig. 37

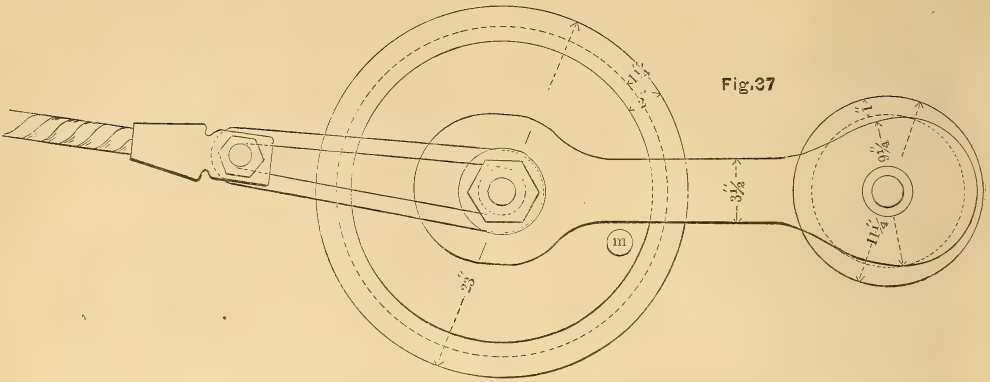
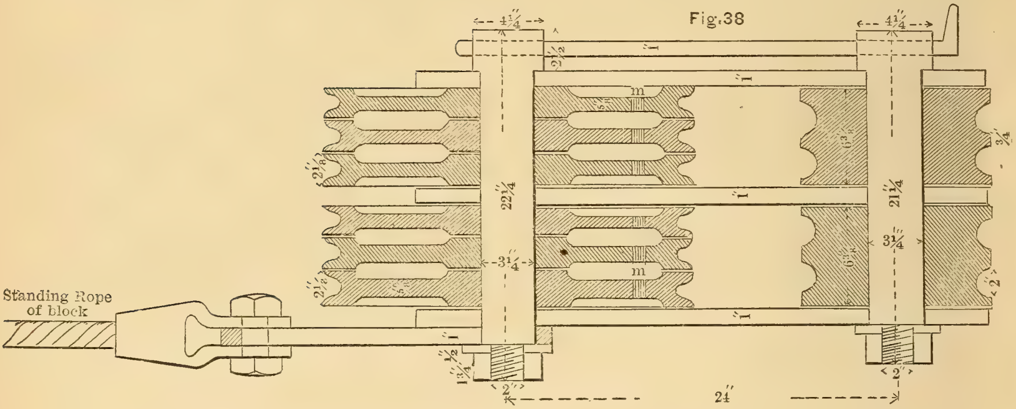


Fig. 38



Scale 1 inch = 1 foot.

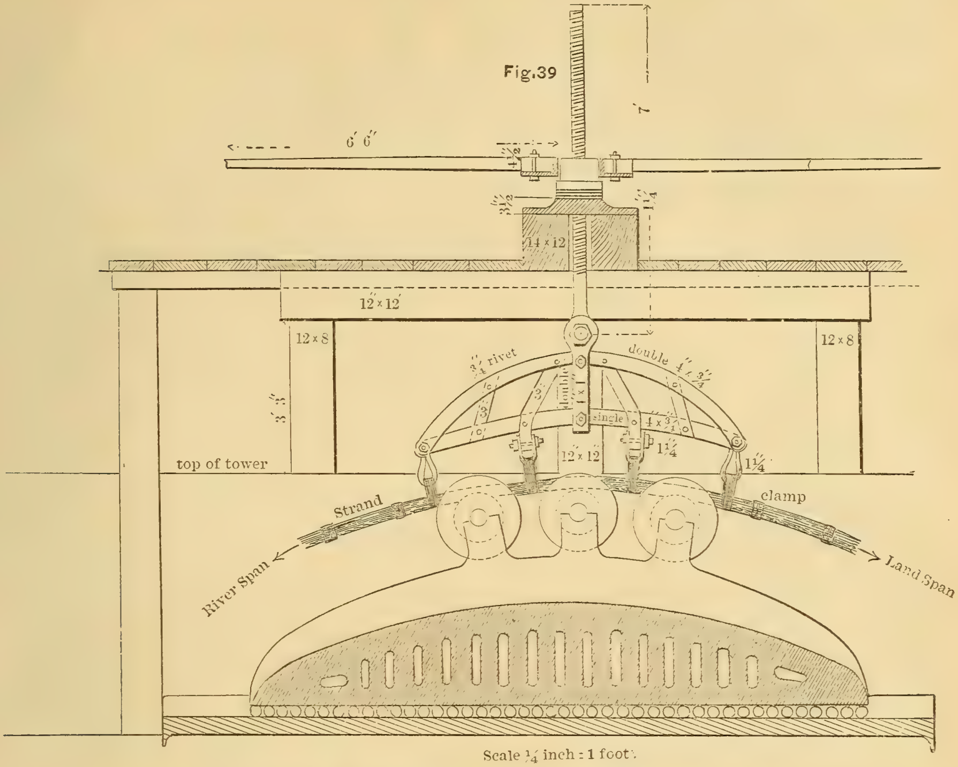
and running block, tightly together. The tension in the strand is 75 tons, that of the fall rope, consequently $\frac{75}{12} = 6\frac{1}{4}$ tons + friction, equal together to about 8 tons, and that of the manilia rope $\frac{6\frac{1}{4}}{8} = \frac{3}{4}$ tons + friction, or about $1\frac{1}{2}$ to 2 tons. The shoe must move 12 feet, hence the wooden block 12 × 12 = 144 feet, and the manila rope 144 × 8 = 1152 feet.

The operation commences in pulling the shoe ⅛ of an inch back from its seat on the leg, and to raise it above the latter in order to get it free. As the strand itself pulls upward, this rise of the shoe requires no extra force. The arrows in the diagram (Fig. 34) show the direction in which the different ropes move during the first operation. As soon as the shoe is free, and the motion of the engine reversed, it slowly travels forward until it reaches the eyes of the anchor links, through which a short pin is driven for the shoe to rest against. After having the shoe secured in this way, the tension in all the ropes is slackened, but the whole letting off apparatus is left intact, because it is needed again for regulating the strand.

Lowering the Strand in the Saddle.—This operation is illustrated in Figs. 39 and 40.

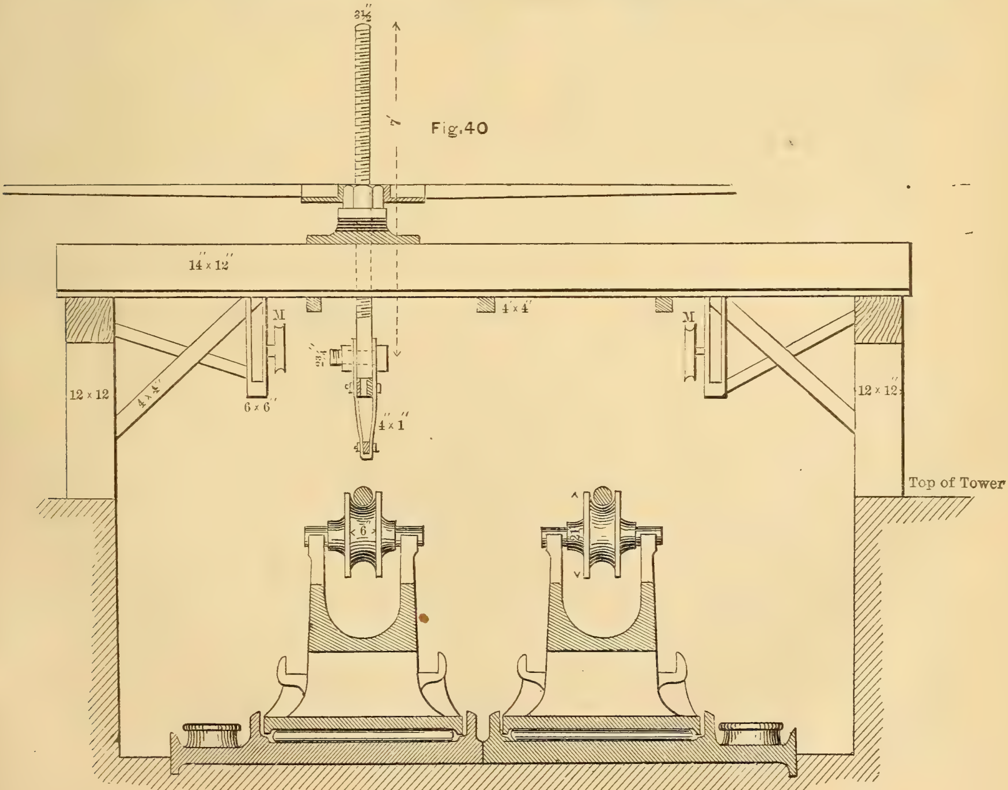
Two 14 × 12 inch timbers, supported on posts, resting on top of the masonry, bridge over the pit in which the saddles are placed. They give support to the nut of a 3½ inch screw of hammered iron, to the lower end of which a yoke-shaped iron frame is attached, which takes hold of the strand at different points, extending over a length of about six feet. The nut, shown on a larger scale in Fig. 41, is turned by means of a cast iron spider (Fig. 42), which fits over the nut and which is provided with six open troughs for the insertion of wooden levers. As soon as the strand is lifted clear over the flange of the rollers, the latter, which rest in open bearings, are

Fig.39



Scale ¼ inch = 1 foot.

Fig.40

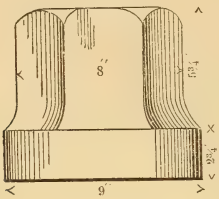


Scale ¼ inch = 1 foot

taken away and the strand is slowly lowered in the saddle.

Fig. 41

Nut.



This operation finishes the work on the single strand, which is ready now to be regulated. The first strand of each cable theoretically needs no regulation, being parallel to the guidewire throughout. But practically there will always be some slight differences in their relative position, owing to inevitable small inaccuracies. It is, therefore, necessary to select the shortest as the guidestrand, and to adjust the others accordingly. The shortening is done in pulling the shoe back from the pin, and inserting between the two an iron segment of the necessary thickness.

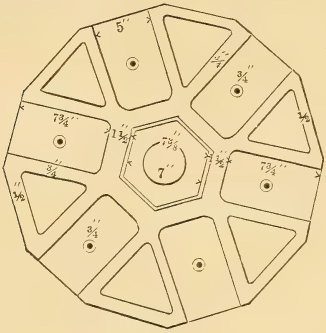


Fig. 42

The strands of one cable, after being regulated, must occupy a relative position as shown in Fig. 5. In the middle span, where all strands hang parallel, there is no particular difficulty in doing this, but in the landspans, where the strands diverge from tower to anchorage, some special considerations are necessary for a successful execution. The leading idea must be, to adjust each single strand so that they all come just right if squeezed together and combined into a solid cable. As the land cable must form a balancing curve to the river cable, it is necessary that each strand shall do the same. Calculating, therefore, to each strand of the river span the equivalent curve of the landstrand, which passes through a certain fixed point, (the point where the cable leaves the anchorage), we are able to compute the length of the strand between this point and the point of intersection. To this length must be added the distance from the face of the anchorage, to the point of attachment to the chain. These two together will give the total length of the landstrand, which will balance the corresponding riverstrand. Instead of measuring actually this length, to do which accurately would be impossible, we establish again tangent lines for the regulation, in the manner described for the guidewire. It is therefore necessary to find an equation between the coördinates of the curve and consequently the following problem must be solved: A parabola of a given length passes through to two fixed points, which have the horizontal distance B and vertical distance h; the position of the vertex shall be determined. Calling the coördinates of this imaginary vertex in regard to the upper fixed point y and x, we have the following two equations:

$$y\left\{1+\tfrac{2}{3}\left(\frac{x}{y}\right)^2\right\}+(B-y)\left\{1+\tfrac{2}{3}\left(\frac{x-h}{B-y}\right)^2\right\}=S=$$

known length of curve, (1)

$$\frac{y^2}{x}=\frac{(B-y)^2}{x-h} \qquad (2)$$

(equation of the parabola).

In (1) are the higher powers of the small fractions $\frac{x}{y}$ and $\frac{x-h}{B-y}$ neglected.

It follows from it:

$$\frac{x^2}{y}+\frac{(x-h)^2}{B-y}=(S-B)\tfrac{3}{2}=m$$

or $x^2B-2xyh+h^2y=mBy-my^2$

from (2) follows:

$$x=\frac{hy^2}{2By-B^2}$$

The value of x substituted in the foregoing equation, we obtain:

$$\frac{h^2y^4B}{(2By-B^2)^2}-\frac{2h^2y^3}{2By-B^2}+h^2y=mBy-my^2$$

or:

$$y^3+y^2\left\{\frac{6h^2B^2-8mB^3}{-3h^2B+4mB^2}\right\}+y\left\{\frac{-4B^3h^2+5mB^4}{-3h^2B+4mB^2}\right\}+\left\{\frac{h^2B^4-mB^5}{-3h^2B+4mB^2}\right\}=0.$$

The expressions in the brackets are constant, and we may call them a, b and c, so that the equation will obtain the form:

$$y^3+ay^2+by+c=0.$$

This is a simple equation of the third degree, which can be solved by introducing a new unknown quantity z, which is determined through the equation:

$$y=z-\frac{a}{3}$$

Calling for abbreviations:

$$b-\frac{a^2}{3}=u \text{ and } \frac{2}{27}a^3-\frac{b}{3}a+c=v$$

and substituting all these values in the above equations it receives the following shape:

$$z^3+uz+v=0.$$

From this follows as one of the roots:

$$z=\sqrt[3]{-\frac{v}{2}+\sqrt{\frac{u^3}{27}+\frac{v^2}{4}}}+\sqrt[3]{-\frac{v}{2}-\sqrt{\frac{u^3}{27}+\frac{v^2}{4}}}$$

Calling the two parts of this expression k and n, the three roots of the equation are:

$$z_1=\quad k+n$$

$$z_2=-\frac{k+n}{2}+\frac{k-n}{2}\sqrt{-3}$$

$$z_3=-\frac{k+n}{2}-\frac{k-n}{2}\sqrt{-3}$$

If two of these roots should be found irrational, of course the rational one is the correct value of z. But if all three are rational, only that value is correct which satisfies equations (1) and (2). Having determined z it is easy to find x and y or the equation of the curve, by means of which the tangent lines can be fixed. The actual calculation is very laborious as it contains large multiplications and figures in the 4th and 5th, power, but, on the other hand, the successful regulation of the strands is at stake, without which united action in the cable would not exist, or at least would be very uncertain.

3. *Wrapping the Cables.*—As soon as the last strand is in position, the cable will be wrapped. The whole wrapping apparatus consists of three parts: the wrapping carriage (Figs. 43 and 44);

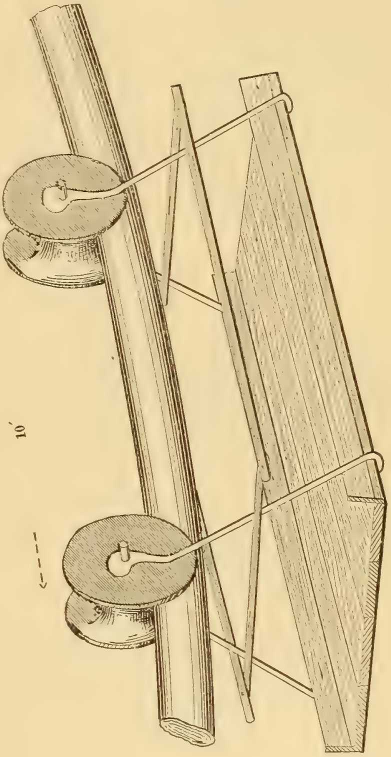


Fig.44

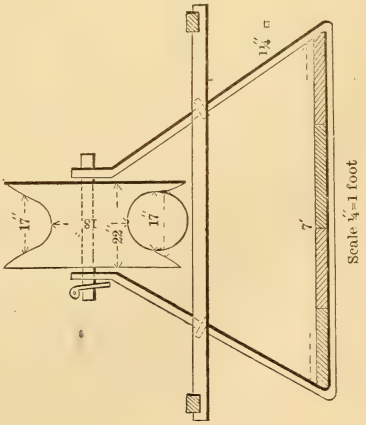


Fig.43

the squeezers (*ss* in Fig. 45) and the wrapping machine proper (Figs. 45 and 46).

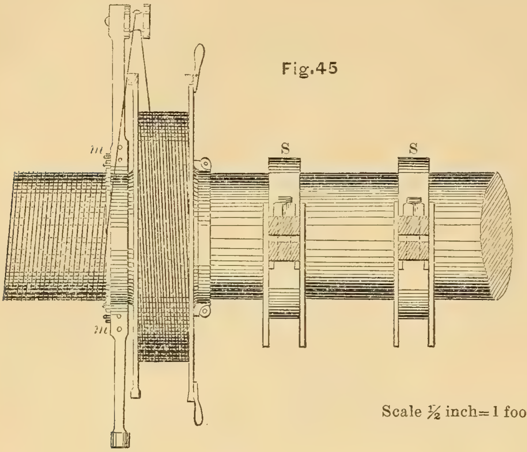


Fig.45

Scale ½ inch=1 foot

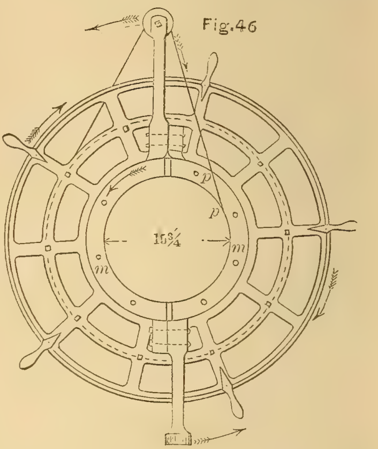


Fig.46

A "carriage" is first placed on each cable at both sides of the towers. It is similar in construction to the one used for tying the strands, but having rollers wide enough to run on the cable. The object of this carriage is to serve as working platform for the men who handle the wrapping machine.

The squeezer consists of two half round steel-bands $1\frac{7}{8}$ inch thick by $3\frac{1}{2}$ inches wide, which fit on the cable or rather which, when placed on the loose strands, force the wires of the cable to assume a circular form. The upper part of the squeezer is provided with four ears, screwed to its side, which diverge somewhat towards their lower end in order to facilitate its slipping on and to catch easily all the wires. The squeezing is done by means of two 2 inch screw bolts which pass through the flanges of the band. Using long wrenches it is possible to exercise great power. Before applying the squeezers, the temporary wrapping of the strands is removed, at least from all within reach. At the East River bridge cables this squeezing and wrapping will be done in two processes. First the seven inside strands will be treated like a separate cable, squeezed and wrapped, before the other strands are manufactured. Afterwards the operation will be repeated with larger squeezers and wrapping machines. In this manner the temporary wrappings of all the strands, save the middle one, can be cut and removed, which would not be possible if the loose strands of the whole cable had to be wrapped. Besides, this double process has the advantage of allowing tighter squeezing with less power, and altogether makes surer work. The cutting of the strand lashings is done a few feet in advance of the two squeezers, which are placed about 10 or 12 inches apart, the first one as near as possible to the saddle. Before tightening up the screws of the squeezers, the cable is once more saturated with oil, so that all the little spaces between the wires are completely filled and hermetically closed. The wrapping machine is now placed a short distance behind the first squeezer, and when the wrapping has reached the latter, this squeezer is put 10 inches in advance of the second one, which in turn is placed the same distance ahead as soon as the wrapping has proceeded so far, etc.

The wrapping machine (Figs. 45 and 46) consists of a drum, formed of two light cast iron frames connected by wooden shrouding, which revolves on a cast iron barrel, that under considerable pressure can slide on the cable, but otherwise is immovable. On the same barrel, but independent from the drum, a ring $\overline{mn}$ with a steel facing revolves, which has two arms, one having at the end a little roller, the other a weight. In the steel face of this ring, there is a small groove $\overline{pp}$, which runs in the line of the tangent from the circumference of the cable to the just mentioned roller. The whole apparatus is in two halves, which are screwed together after being placed on the cable. The wrapping wire, which is coiled on the drum, passes from here over the roller, through the groove, to the cable where its end is fastened. Now the drum is turned in direction of the arrows and the two armed ring is turned in opposite direction. The first motion uncoils the wrapping wire, while the second winds it on the cable and at the same time keeps it by the momentum of the counter weight under great tension. As the wire passes through the groove in the steel ring, it squeezes itself, on account of the spiral lay, between the finished wrapping and the barrel and, consequently, pushes the latter ahead for a distance equal to the thickness of the wrapping wire. The ends of these wires are spliced, so that the whole wrapping in one span consists of one continuous wire. When the two wrapping machines meet in the center of the span, they are taken off and the two wires joined. Care must be taken, to wrap in opposite direction, so that the ends of the two last wires are on opposite sides of the cable and can be spliced under tension.

The wrapping wire generally consists of No. 11 or No. 10 annealed iron wire. For the East River bridge No. 10 galvanized wire will be used. Near the saddle, where the wrapping machine cannot be placed, the cable is bound together with iron bands about five inches apart. At the point in the anchorage, whence the different strands diverge, a heavy iron ring in addition will be placed, in order to lessen the strain on the wrapping wire. The single strands are wrapped by hand from this ring close to the shoe. Immediately after having wrapped the cable for a certain distance, it should be painted with several coats of good oil paint, so that the little spaces between the wires are completely filled up, giving to the cable a smooth cylindrical appearance and preventing the moisture to penetrate.

This operation finishes the cables, which are now ready to receive the superstructure. This should be put on symmetrically to the towers, and simultaneously in centre and land spans, in order to avoid uneven tension in the cable, and strain on the towers.

After the greater part is suspended, the blocking, which held the saddles in place, is removed, giving to the cable free chance to move back or forth, and to assume its natural position of balance.

We have shown that under a certain temperature, this motion will be about two inches towards the river. The temperature under which the first strand of the East River bridge cable was regulated, happened to be 75—80° F. If the saddle blocking is removed on a colder day, the above motion will be less owing to the greater tension in the land cable, caused by greater contractions, than the river cable. The reverse will take place if the saddles are set free on a warmer day. Condition, however, always is that in each span, the weight of the superstructure is the same, and symmetrical to the tower.

The lengths of the suspenders must be calculated according to the final curve of the cable, which is attained when the stays are put in place. Before this the floor will have an irregular shape, provided all suspenders have their exact length. A continuous grade of the floor will, however, be established as soon as the stays are put under tension, which will cause the cable to assume that curve for which the suspenders were calculated, bringing thereby, the latter also under their proper tension.

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At a meeting of the Engineering Society, King's College, London, held lately, Mr. C. D. Marr read a paper "On Marine Engines." The author in commencing expressed regret at being unable, on account of the short time at his disposal, to say anything about the early history of the marine engine and the respective merits of the various kinds of propellers, and other points closely connected with the subject. He first described the various kinds of paddle engine, dwelling particularly on the oscillating engine; and spoke also of injection and condensation. He then passed on to the screw engine, and described the most recent examples of inverted and horizontal return connecting rod engines, noting especially the vertical engines of the screw steamship San Francisco, belonging to the Pacific Mail Steamship Co. of the United States of America, treating of the surface condensers. He then drew a comparison between the injection and surface condensers.

TRANSPORTATION OF HIGH EXPLOSIVES.

Written for VAN NOSTRAND'S MAGAZINE.

THIS subject has now come to be one of acknowledged importance; and, owing to peculiar circumstances, its serious consideration is pressingly demanded.

On the one hand, we find these explosives are being manufactured and used on a large scale, and their transportation on public conveyances openly and freely and generally is very desirable; while, on the other hand, the public and common carriers dread the consequences.

Nitroglycerin in the liquid state is justly deemed too dangerous for anything like general transportation; and its compounds, the high explosive powders, are supposed to be almost equally dangerous.

The laws both of the United States and of the several states on this subject are in a crude, uncertain and inappropriate condition. The result is that proper transportation is greatly restricted and hampered, and improper transportation practiced.

We therefore propose to examine the entire subject with some care.

In doing this we will first consider the nature and character of the explosives themselves, and next, the present state of legislation in regard to them, and finally the general features of a proper law.

But first of all a few words as to the industrial importance of these powders, and the strong necessity for their transport by common carriers. This is already pretty generally understood, but a few facts will not be inappropriate.

These powders vary in strength—the strongest being estimated to be ten times stronger than common blasting powder for disruptive purposes. Hence their great value in hard blasting.

Their use has resulted in what may be properly called a revolution in the art of blasting. The cost of hard rock work is reduced by them fully one-third, and much time is saved besides. With them many undertakings become successful which, without them, either have been, or would be, failures.

No important hard rock work is now done without them. The great silver mines of Nevada and many other mines are worked exclusively by them.

They are also the leading explosives in many of the mines of California, Utah, Colorado, Arizona, Missouri, Michigan, Pennsylvania, New Jersey and Northern New York; and are extensively employed in quarries, railroad cuts and tunnels, canals, aqueducts, river and harbor obstructions, and for the removal of stumps and boulders on farms. Their consumption at the present time in this country is not far from five million of pounds per annum.

Their transport long distances and over a wide range of territory is an imperative necessity. Their manufacture to any considerable extent on the ground where used is impracticable. The materials for their production are found at the great centres of trade and manufacture, and weigh four times as much as the product, while the chief consumption takes place at great distances from these centres.

To prohibit their transportation by common carriers of freight, or to so encumber or restrict their transport by these means as to make it as expensive as to manufacture on the ground, would have the effect to greatly diminish their consumption and so far check the development of that class of industries which deserve special encouragement.

NITROGLYCERIN.

Nitroglycerin, the basis of what are generally known as the high explosive powders, is a light colored, oily liquid, about fifty per cent. heavier than water. It is not soluble in water, and, therefore, when poured into a bore-hole filled with water it sinks to the bottom displacing its bulk of water.

The ordinary mode of making it is—first to mix two and one half parts, by weight, of strong nitric acid with five parts of strong sulphuric acid. After allowing this mixture to cool, there is gradually stirred into it one part of pure glycerin, care being taken to keep the temperature below 70° Fah.

The nitroglycerin is then separated from the mixture and carefully washed in cold water.

This is a mere outline. There are

many important details, not appropriate here.

We shall discuss only one of the qualities of nitroglycerin—its explosiveness.

This we do, because it is absolutely necessary to an intelligent explanation of the structure and nature of the powders, and the difference between it and them.

ITS COMBUSTION.

The first fact to be noticed in this connection is that it does not explode by fire like gun-powder, gun-cotton and the fulminates.

With fire it behaves very much like whale oil. At ordinary temperatures it does not burn by itself. It must have something in the nature of a wick to aid combustion.

Touch it with a match, live coal or a red hot iron, and it burns so long as the contact is maintained, but when these things are withdrawn the combustion ceases.

It must be separated—brought out of its compact form before it will burn.

If mixed with almost any dry substance, whether the substance be itself combustible or not, it will burn.

Mixed with ashes, sand or brick dust, it burns almost as rapidly as when mixed with charcoal, sawdust or even gunpowder, if the gun-powder is saturated with it.

The gases formed by its combustion are materially different from those formed by its explosion.

They are much more offensive to the smell and injurious to health. It sometimes occurs that the exploder fails to explode a high explosive powder, but sets it on fire.

When this takes place in a mine not well ventilated the smoke is almost intolerable, while the gases of complete explosion are far from being so.

More will be said as to the combustion of the powder hereafter. As to the combustion of liquid nitroglycerin, it is enough to say that it is a totally different thing from its explosion.

Its burning is so slow that no more violence comes from it than from the burning of whale oil.

ITS EXPLOSION.

It is a powerful explosive, more so than any other disrupting agent in use. It has, in a high degree, the great requisite for disruptive force, to wit, the ability to furnish abundance of gas rapidly.

However, the superiority does not lie so much in the abundance of gas as in the rapidity of its formation which is done not by fire but by force suddenly applied to all parts of the charge.

This will be fully explained further on.

It is estimated to be from ten to twelve times more powerful than gunpowder. But this mode of expression is loose and liable to mislead. One pound of nitroglycerin will not do what ten pounds of gunpowder can, and ten pounds of gunpowder will not do what one pound of nitroglycerin can. It cannot be used in ordnance or fire arms, as even small charges burst the gun.

In all kinds of hard rock work; also in sub-aqueous blasting; in breaking iron or steel, and in all positions where disruptive rather than ballistic effect is required, nitroglycerin is wonderfully efficient.

But how is it exploded? The ordinary means of exploding it at will, in use, is a heavy percussion cap or exploder.

This consists of a copper shell, like a common gun cap containing ten grains or more of fulminate of mercury. The exploder is not fired by percussion, but by a fuse or electric wires inserted in it, in contact with the fulminate.

It is also readily explodable by mechanical percussion, concussion, jar, shock, blow or vibration.

This is why it is so extremely dangerous.

Its handling in the liquid form at ordinary temperatures has to be performed with extreme caution. Perhaps no better illustration of its dangerous character can be given than by quoting the Rules laid down by Geo. M. Mowbray Esq., and which his customers are warned to obey in its use.

"INSTRUCTIONS FOR HANDLING AND USING MOWBRAY'S TRI-NITRO-GLYCLRIN.

"1. Handle carefully, avoiding a sudden jar or concussion, and be very careful, if any is spilt outside the can, to avoid striking it against any hard substance.

"2. When solid, thaw out by placing the can in a tub of warm water, not hotter than the wrist can bear, first pouring warm water into the can, and always remove the can before adding more hot water to the tub.

"3. To fill cartridges, &c.: hold the cartridges to be filled over a tray, say two feet by three feet, the bottom of which should be covered with plaster of Paris (which will not readily explode when saturated with nitroglycerin). The soiled plaster of Paris should be frequently renewed.

"4. If the nitroglycerin in a liquid state is kept in store or magazine for some time, the cork should be loosely inserted, and a pint of cold water poured into each can, to be frequently poured off and replaced with fresh cold water in warm weather, taking care to retain the bladder under the cork. It is preferable when ice can be procured to congeal the nitroglycerin.

"5. Use funnels (gutta percha if they can be had) for filling water holes. Under no circumstances whatever attempt to tamp the drill holes; it is unneccessary, and may kill the man who attempts it.

"6. Hot irons to warm the water, or for soldering the cans, will be sure to cause explosions.

"7. Never sledge or attempt drilling in a hole or seam where nitroglycerin has been spilled; fire an exploder, which will effectually clear it up.

"8. Never pour nitroglycerin into a hole unless perfectly sure that it is a sound hole, or will hold water; if seamy always use cartridges.

"9. To obtain the best results with niroglycerin, drill deep holes, six feet or more. Use powerful exploders and well insulated wires. It is cheaper to fire by electric battery with simultaneous explosion than to fire several holes with tape fuse.

"10. Look out after a blast for any unexploded cartridges lying around.

"11. Never allow any but the most careful persons to handle or have charge of the nitroglycerin, and insist upon the use of every precaution to prevent an accident or explosion.

"12. Never allow empty glycerin cans to be used for any other purpose, but destroy them by a fuse and exploder, or by building a fire under them, first, however, removing them to a safe distance.

"13. Examine your cans from time to time, and notice if, at the level of the nitroglycerin, any pin holes have eaten through; in such case procure a new can, or stone jar, and empty the contents out, not trusting your hold to the upper part of the can lest it may give way.

"14. When solid, or congealed it is absolutely safe; if possible, therefore, any surplus should be stored surrounded with ice, since no explosion can take place when it is solid."

To this may be added a few cases of accidental explosion. At Yonkers, a boy threw a stone at a can of it, and was killed by the explosion.

At Aspinwall, a can of it fell the distance between decks and exploded, doing immense injury. In opening a package of it by Wells, Fargo & Co., in their office in San Francisco, it exploded, killing many persons and wrecking the building. At Worcester, Mass., a quantity of Dualin in a car exploded doing great mischief. By some it is thought that this explosion was caused by exploders, which were in the same car with the powder, while others suppose it was due to a leakage of nitroglycerin from the powder amongst the springs and axles of the car and upon the rails.

While being transported in a wagon from Titusville, Pa., it exploded, destroying horse and driver. While opening a tin cartridge of it last summer at Hell Gate, it exploded, and killed three men.

In Greenwich Street, New York, a can of it exploded from voluntary decomposition.

While being transferred last year in a pail from the tank to the scales and mixing pan, it exploded, killing two men and destroying the factory.

There is also more or less danger from its instability. When it contains acids it is liable to change. A chemical action sets in which may result in combustion and, under confinement, in explosion.

The rapidity of the change depends upon the quantity of acids present and the temperature.

The greater the proportion of acids and the higher the temperature the more certain and rapid the action.

A failure to thoroughly cleanse the oil from the acids used in its manufacture may lead to its voluntary decomposition.

This was doubtless the cause of the

disastrous explosion in Greenwich Street. The can was observed to be smoking, and was taken from the house and placed on the sidewalk, where it soon exploded.

Many other like proofs might be added, but these are enough for our purpose, to wit, to show that liquid nitroglycerin cannot be handled and transported with any reasonable degree of safety. *They justify our conclusion that nitroglycerin ought not to be stored, transported, or used in the liquid state.*

Nobel made known his method of exploding nitroglycerin by an exploder in 1863, but the transportation and use of the liquid was not really commenced until 1865, and was discontinued almost completely in 1867. In less than two years' time, in spite of every precaution, it had destroyed so many lives and so much property as to alarm the whole civilized world. Both in this country and abroad stringent laws were enacted, designed—some of them to regulate, and others to suppress, its transport and use. None of these laws were too stringent; many of them not enough so. The plain truth is that while the liquid form is retained no amount or kind of packing can render it secure against the shocks, jars, vibration and breakages incident to transportation.

THE DYNAMITE INVENTION.

This consists broadly in mixing nitroglycerin with any dry, pulverized, solid substance, in such proportions that on the one hand the mixture will be an effective explosive powder, and on the other hand will not leak or part with any portion of its nitroglycerin. This is known by the generic name of Dynamite. In this country it is called by different names by different mannfacturers.

Giant Powder, Rend Rock, Hercules Powder, Vulcan Powder, Jupiter Powder, Neptune Powder, Thunderbolt Powder, Dualin Powder, Titan Powder, Titanite Powder, Potentia Powder, Vigorite Powder, are some of the names it bears.

All these powders, known as the high explosive powders, are essentially the same thing.

Nitroglycerin is in them all and is the leading element. The difference between them consists in the different proportions of nitroglycerin which they contain, and the different solid materials used, and the different proportions of these solids.

These solid materials when prepared for admixture are called the "absorbent," "dope," or "dust," and are always in the dry and pulverized state.

Let us first consider the general features of these powders, and afterwards some of the circumstances by which they are affected; and first of all let us take up the question of safety, as this is the all-important one now before us.

THEIR SAFETY.

We might content ourselves with the external proofs on this subject, such as actual tests by experts and experience in transportation and use, but there is a natural craving to know how a thing is constituted—a laudable desire to look into the structure and see whether there can be found there any internal and intrinsic evidence in support of the external testimony.

Besides, as all these powders contain nitroglycerin, and it is admitted that nitroglycerin is extremely dangerous, the mind irresistibly carries the danger with the nitroglycerin into the powders, and especially is this the case when all agree that the powders are explosive, and solely so by reason of the nitroglycerin they contain.

How is it that the nitroglycerin retains its explosive power and not its sensitiveness—its danger? If it will explode in a bore-hole, why not out of a bore-hole?

If it will explode for use, why not for mischief? How can it be sensitive when you wish it to be so, and not so when you do not wish it?

Here is a seeming paradox.

These are pertinent questions, and ought to be answered, not by mere assertion but by logical deductions.

To be clear to the unprofessional reader, we must proceed by degrees slowly.

Let us suppose we mix water with gun-powder. What is the result? The gun-powder is still there in the mixture, but is it dangerous? It is wholly inexplosive, not even combustible.

This answers the general assumption that what is dangerous alone, is so in mixture. Again, let gun-powder be mixed with fine sand or ashes or pulverized glass. What is the result? This mixture is incombustible also.

This was the celebrated Gale invention.

It was proposed to transport gunpowder in this mixture and carry it in this form upon the battle-field and on ships of war until required for use, and then to sift out the fine, inert material and leave the gun-powder clear.

But old war dogs preferred the extra danger to the uncertainty and delay of the separation, and the scheme died.

But it shows what mixture will do.

Now let us suppose we mix an *ounce* of nitroglycerin into a *pound* of pulverized infusorial earth, charcoal, chalk, ashes, or any other absorbent. What is the result?

Like the gunpowder mixtures it is wholly inexplosive and wholly incombustible.

Here is nitroglycerin but where is the danger? No means on earth can explode such a mixture.

Now let us increase little by little the proportions of nitroglycerin, and keep trying to effect an explosion; and for this purpose, make use of the most certain means, to wit, the exploder already described.

We shall at last obtain a mixture which is explodable by means of this exploder. The mixture will be comparatively dry. It will be a powder. It will not be a liquid or pasty, nor wet or damp; it will not leak or lose its nitroglycerin, however long it may stand. It will also be a powerful explosive. Now try to explode it by blows upon it. Hit it in mass with hammer and sledge. Let it fall from great heights. Drop heavy weights upon it from great heights. Set it on fire and burn it. Abuse it; turn it; throw it; heat it; freeze it; wet it; dry it; do almost anything you please with it, except to put the strong exploder to it, and you cannot explode it.

This is dynamite—or a high explosive powder.

Now let us go a little into details. The first inquiry is, Will a powder made on this plan be a really powerful explosive?

One would naturally suppose, that if it contained no more nitroglycerin than is sufficient to make it explosive at all it would be very weak and ineffective.

Here appears one of the wonderful peculiarities of nitroglycerin.

In mixing inert pulverized substances with any of the old fire explosives its explosive power is diminished *in proportion to the adulteration*—the direct effect being to slow the combustion. The greater the proportion of the adulterant the slower the combustion until the mixture becomes not only inexplosive but even incombustible, as in the Gale invention already noticed.

On the other hand, the absorbents used in nitroglycerin powder do not strictly slow its explosion at all.

This difference is due to the different laws under which the two classes of explosives explode.

The gunpowder class or fire explosives decompose by combustion progressively—the charge burning from the point of ignition step by step until all parts are reached and consumed.

It is apparent that in this kind of explosion its speed depends entirely upon the facilities for the passage of flame, and anything which obstructs its passage slows the explosion. The fine sand or pulverized glass used by Gale had this effect.

It filled up the interstices between the grains of powder and separated them from each other, and thus prevented the spread of the flame and heat, and rendered the mass wholly inexplosive when the proportion of inert matter was sufficient and slowed—retarded the action in the exact proportion to the amount of dust present.

On the other hand, nitroglycerin does not thus explode by the application of fire, but by the application of force. Whether we call it shock, jar, a blow, percussion, concussion or vibration or the heat produced by these, one thing is plain, that force is necessary in some form, whereas in gunpowder explosion it is not.

This force is applied not gradually or slowly or to a part of the charge at a time, but at once, and suddenly, and to all parts of the charge simultaneously. Perhaps it may be said that this cannot be strictly, theoretically true where a small exploder is used in a large charge —that the exploder cannot affect directly the entire charge but explodes a part of it and this explosion explodes the balance. For the purposes of the present inquiry this is immaterial. All that is essential to be sure of in this connection is, that the explosion is produced by force whether applied directly by the exploder or indirectly by propagation.

We will therefore proceed.

In order to the explosion of nitroglycerin at all, whether in the liquid or powder form, a certain degree of this force is requisite; and when this proper degree of force is applied to its particles they explode however situated.

The fact that these particles are separated one from the other is of no consequence so long as they are made to feel the requisite force.

For example; If a thin film of the oil be placed upon the face of an anvil, or on sheet metal, and be struck so as to compress the particles of the oil they explode. Now if we have a series of these plates one above the other, to any extent with the oil between them all, and a blow be struck upon the top of the pile all the nitroglycerin between the plates throughout the pile will be exploded, substantially simultaneously, and yet the charge thus exploded was completely divided into as many wholly separate parts as there were plates.

Whereas if gunpowder had been placed between the plates instead of nitroglycerin and fire had been applied at the top of the pile, the explosion would have been limited to the burning of the powder between two plates only.

Suppose now, we use plates of india-rubber, or wood, or cloth, or leather, or any other compressible material, instead of the rigid plates of iron. What is the effect?

A blow upon the top of the pile does not produce the percussive, sharp, ringing and violent effect requisite to explosion. The blow is deadened, the nitroglycerin is cushioned and protected and no explosion follows as in case of the iron plates.

Here is the whole secret of the dynamite invention. When an absorbent is used it acts like these soft plates in separating, cushioning and protecting the nitroglycerin, and making it safe against mechanical blows; but when sufficient force is applied to solidify the absorbent, as when an exploder is used, it acts like the iron plates in communicating the force to the different parts of the charge.

The absorbent renders the oil less sensitive, that is to say, more force is required to explode the nitroglycerin in the powders than to explode it in the fluid form.

While in the fluid form it is compact and incompressible, and slight force will give it the requisite compression; but when in the powder form it is cushioned by the absorbent, and this cushion has to be compressed and made solid, and this requires additional force precisely as it requires a more forcible blow to drive a nail whose head is cushioned. The cushion must be compressed to solidity before the nail will start.

However, notwithstanding the explosion is more difficult when the nitroglycerin is thus cushioned, yet, when the force is sufficient to effect it at all, the decomposition takes place from the same cause and proceeds in the same way and is accomplished in substantially the same time as when not so cushioned; and therefore the intrinsic power developed by the nitroglycerin in the powder is substantially the same as when the same amount of it is exploded in the fluid form.

Thus we find that from the peculiar nature of nitroglycerin a powerful powder can be made, and at the same time be extremely difficult to explode, and therefore safe.

For instance: A powder made fifty per cent. of nitroglycerin and fifty per cent. of infusorial earth is very dry and cannot be exploded except by a triple force exploder, and when the charge is strongly and tightly confined.

On the other hand, a powder made fifty per cent. of nitroglycerin and fifty per cent. of mica scales or fine sand, is very wet and leaky and explodes almost as easily as the liquid oil. Nevertheless the earth powder is in every way as strong as either of the others. This confirms our position, that a powder which is very dry and difficult to explode, and therefore safe, is substantially as good as a wet and therefore dangerous one, when the proportion of nitroglycerin is the same in each.

Having now shown the principles upon which these powders are constructed, and having demonstrated thereby that they may be so made as to be safe for transportation, handling and use, let us consider the chief circumstances by which they are affected.

CHARACTER OF THE ABSORBENT AND PROPORTION OF NITROGLYCERIN.

From what has been already said, it is plain that these powders may be so made as to be either safe or unsafe according to the proportion of oil they contain. But it must be borne in mind that this proportion is to be measured not arbitrarily by percentage merely, or weight, but relatively according also to the absorbent capacity of the solid substances used.

Of course, different substances have different absorbent capacities. Some will hold safely seventy-eight per cent. of nitroglycerin, while others will not safely hold two per cent.

Wetness or dryness, then, is the true test of safety. If it leaks it is dangerous; if not it is not dangerous.

Here it may be asked whether a powder, which is as damp with nitroglycerin as it can be without leaking, is not dangerous?

We answer, no. A moment's thought will settle this, as it has been settled by experience. All mixtures so fully saturated as to have no vacancies will leak.

The fact that it does not leak shows that it has pores, interstices, vacancies; that it is not compact like the liquid, but compressible and yielding. This shows it to be safe. Its safety does not depend so much upon the *amount* of vacancies in the powder as upon the fact of their existence at all. A slight compressibility destroys the fatal rigidity and gives safety.

Infusorial earth with eighty per cent. is saturated, and almost as compact as pure nitroglycerin and almost as dangerous, while with seventy-five per cent. it is abundantly safe for the highest torrid zone temperature. Even a powder which will leak will be safe against blows upon it.

Its danger lies in the escape of the liquid.

The oil drained from powder is as dangerous as if it had never been in it.

Here, then, we find the exact spot upon which the law should lay its hand.

No leaky powder should be transported at all. It should be classed with liquid nitroglycerin. To determine whether a powder is dangerous is the simplest of all things. If it will leak it is dangerous, if not, it is safe.

Whether it is leaky or not is settled by an examination.

It would be folly to rely on the proportion by actual weight of the ingredients used, because, with the same proportions, but with absorbents of different capacity, one powder would be dry and safe and another wet and dangerous.

Actual inspection and test of the powder is the only reliable criterion.

Here it should be noted that too much importance is liable to be attached to this point of leaky powders. The actual danger from this source, in the absence of all legal regulations, is very slight. No manufacturer or dealer in these powders would, knowingly, send abroad a leaky article—it would be condemned on sight. This matter is now thoroughly understood, and the practice of making the powders dry and safe is settled beyond danger of disturbance; and the likelihood of their being made and sent abroad in a leaky condition by accident is very remote.

Self interest is a security that may generally be relied upon with confidence.

Only one leaky powder has ever been heard of, and that doubtful. We refer to the Worcester case. However, this state of facts is no excuse for neglecting to pass laws against leaky powders.

TEMPERATURE.

Other things being equal both nitroglycerin and its compounds explode easier as the temperature is higher.

At any temperature below zero Fah. liquid nitroglycerin is not explodable by any ordinary means. At 32° Fah. it explodes with great difficulty. This is the means employed mainly by Mr. Mowbray for making his nitroglycerin safe for handling and transport. He freezes it. However, frozen nitroglycerin *can* be exploded. All that is necessary is to intensify the means. Use a sufficiently strong exploder or confinement or both, and it can be exploded at any temperature.

So much for the effect of cold. The other extreme is about 360° Fah. At this temperature it either burns or explodes. If free from all pressure, jar, vibration or force in any form, it burns, otherwise it explodes.

Placed in a film on tin, and held over a spirit lamp, it smokes away or takes fire and is consumed. But heated to this temperature when of any considerable depth, say over a quarter of an inch, it explodes.

When heated to any degree less than this, it is exploded by a cap, or blow, or jar, or vibration, with an ease proportionate to the temperature. At 350° the fall upon it of a dime will explode it.

Temperature has another special influence on the powder, to wit, they are more liable to leak as their temperature is raised.

A powder which would be dry and safe at 50° may be leaky at 100°.

The powders, therefore, should be made with reference to the highest temperature to which they are to be exposed, and when tested they should be at this temperature.

CONFINEMENT AND COMPRESSION.

Other things being equal, nitroglycerin and its compounds are more easily exploded, as they are more closely confined and strongly compressed. If they be enclosed without pressure, and an exploder applied, the enclosure aids the exploder in applying the pressure, so that a charge which a certain exploder will not explode in the open air may be exploded, by this same exploder, in a bore, hole or under water.

So also, if they be not only tightly but also strongly inclosed, as in gas pipes, with a cap screwed on each end and set on fire by a fuse through a small hole or otherwise, they will explode. The gases from the combustion cause a pressure on the part of the charge not burned which, unless they escape, will finally be sufficient to cause explosion.

Compression of the charge makes it more sensitive.

Let an iron tube be filled with nitroglycerin.

A blow, which will not cause explosion when struck against the upper end of this tube, will cause explosion when struck against its lower end, the difference being that the lower part of the charge is under pressure from the upper part. But this pressure from superincumbency has no material influence on the powder for reasons too obvious to be mentioned.

On the other hand, if spread upon an anvil and struck, only so much as is hit, explodes; the balance does not, because it is not sufficiently confined or held so as to receive the requisite blow or pressure from that part which does explode. The reason why a small quantity of nitroglycerin in a large mass of absorbent cannot be exploded at all, is, that the absorbent cushions the liquid so deeply that the requisite pressure is not felt.

The practical lesson from these facts is that the powder for transportation ought not to be packed in strong and tight vessels.

METALLIC CASES.

The rigid character and peculiarly forcible vibration of steel and iron, and especially when in the sheet form, seem to be particularly favorable to the explosion of nitroglycerin and its compounds.

What will not explode a charge in wood or paper will explode it in iron.

Place a three inch charge of dynamite in the bottom of a hole bored into a log, and fire three inches of gunpowder well tamped above it and the dynamite will not explode; but in iron, it will.

Strike nitroglycerin in a leather bag and it will not explode by a blow which will explode it in a tin vessel.

Nobel deemed it of great importance that the solid form of dynamite allowed it to be packed in wood; this because the metallic vessels in which the liquid was forced to be carried were so dangerous.

Mr. Mowbray gives the following instance; A part of a can of nitroglycerin was placed 300 or 400 feet away from a blast in the Hoosac Tunnel. The upper edge of the can was in contact with the iron rail leading from the vicinity of the blast. The blast exploded the nitroglycerin in the can. This was undoubtedly from the vibration along the rail. If the oil had been in wood or paper, it would not have exploded.

This teaches us not to use metallic cases for the transportation of nitroglycerin or its compounds.

THAWING AND INCLOSURE.

So far as known there are but two

ways in which there is any danger from fire.

One is where the powder is completely inclosed in some strong vessel and set on fire, as already explained. The vessel must be much stronger than any in which the powders are ever transported, and must be so tight that the gases can not escape as fast as they form.

There is no practical danger from this source in transportation.

The other way is in roasting, toasting and baking the powder, when frozen.

When frozen cartridges are put into a hot oven, upon stoves or boilers, or in kettles over a fire, or placed before a hot fire and allowed to remain long enough to thaw, and become so hot as to smoke, the result is that in about nineteen cases out of twenty the powders take fire and burn up, or all the nitroglycerin is evaporated, and they are ruined.

But in the twentieth case there will be an explosion. If all the powder is equally exposed to the heat, as the evaporation commences long before the exploding point is reached, the powder is weakened, and the explosion is correspondingly weak—often a mere pop or puff.

But if there is a large quantity of powder, some of which is thus heated, and the balance left unaffected, the explosion may extend to the full strength powder, and be violent accordingly.

But as this mode of heating frozen powder cannot take place during transportation, it does not belong in the discussion, and is only mentioned for the sake of having the review complete.

SPONTANEOUS DECOMPOSITION.

Liquid nitroglycerin, as already stated, unless completely freed from the acids employed in its manufacture, is liable to voluntary decomposition, especially in warm times and places, resulting in most cases in combustion, but under favorable circumstances, in explosion.

By the oversight, or carelessness of a workman, this danger may be incurred. But this instability is not found in the powder. No instance of such decomposition has ever been known.

Besides, it has been shown by repeated experiments that nitroglycerin impregnated with acids, and which will decompose while in the liquid form, may be made into powder, and subjected to all the conditions most favorable for decomposition, without that result.

The powder form, that is, the solid form, seems to be an effectual bar to the peculiar chemical action requisite for spontaneous decomposition.

EXPERIMENTAL TESTS.

Having considered the structure of the high explosive powders and the circumstances which affect them, let us briefly review some of the actual experiments touching their safety.

We believe there has never been any question but these powders are abundantly safe from explosion by fire. The following are common tests which may be repeated by any one at any time.

If set on fire in piles, large or small, either loose or in cartridges, it burns up rapidly like chaff when loose, but slowly like rosin, tar or sulphur, when in cartridges.

Set fire to one end of a cartridge and it burns much like a Roman candle, without the pop but with less speed.

When partly burned it may be extinguished by water or the foot.

As packed for transportation in boxes of inch boards strongly nailed and set on fire by a fuse through a gimlet hole, its gases spring the boards apart and the flame issues. A box of one-hundred pounds is burned in from two to five minutes, according to the composition of the powder. Those who know the powder stand upon the box while it is burning.

Poured upon a red hot iron it burns.

A red hot iron thrust into it sets it on fire.

The Steamer "Meteor" took fire from its furnace and burned to the water's edge on Lake Erie consuming 8,000 pounds of Giant Powder on board without explosion.

The only source from which the enemies of these powders profess to apprehend danger during transportation, is percussion, concussion, shocks and blows.

The following experiments have been repeated many times:

A minute quantity is placed on an anvil and struck with a hammer. A snapping sound is produced like the breaking of a stick, and the particles between the points in contact are exploded and no more—the balance is scattered.

If the quantity be increased, and the blows repeated until the powder is made solid, a greater quantity can be exploded; but if the quantity is so large that the blows are deadened and the percussive action prevented there will be no explosion.

A cartridge of powder laid upon a rail and passed over by cars will be cut in two, and the small amount between the wheel and rail will be exploded, if not by the first wheel, by subsequent ones, but the balance of the cartridge will not explode.

Pounding it upon wood will not explode it.

A box of it thrown from any height upon rocks will be broken in pieces, but no explosion can be thus caused, nor by dropping heavy weights upon it. A box of it between car buffers, meeting at a speed of sixty miles an hour, will not be exploded.

If special preparations are made it can be exploded in quantity by percussion.

By putting it into an iron cylinder, fitted with a piston, it can be exploded by blows on the piston sufficiently numerous and heavy. But, of course, this cannot occur in transportation.

A car loaded with iron rails at full speed collided with a car of powder, and the rails were driven through the boxes and powder without exploding it.

ACTUAL EXPERIENCE.

The manufacture of dynamite commenced in 1867 and the trade in it has gradually increased until it now amounts in this country and Europe to about 15,000,000 pounds per annum.

Its transportation has taken place by all the ordinary means, on vessels and vehicles of every description. It has met with such kinds of treatment as would naturally befall an article supposed to be safe in every respect and handled roughly. Nevertheless, there has never been an accidental explosion from any cause while being transported either in this country or abroad, except the Dualin at Worcester already mentioned.

There have occurred several explosions supposed, at least to some extent, to have been due to these powders but the supposition is unwarranted.

To avoid all misunderstanding we will enumerate them. The most notable case was the Bremerhaven explosion. This was not of dynamite proper but of a compound resembling it, called Lithofracteur. However, the powder was not to blame, as the explosion was caused by an exploder placed in the powder by design, and fired by clock work.

The Bergen Hill explosion was of what is known as Rend Rock powder, and was caused by exploders applied by workmen during a strike.

An explosion of dynamite took place in San Francisco while being prepared for a large blast under water. The loose powder which was being packed into cartridges was set on fire from the pipe of a workman, and before it could be extinguished the fire reached exploders in other cartridges near by and of course, caused their explosion.

The explosion last year at Drakesville was of liquid nitroglycerin, while being weighed and transferred.

The fact that it occurred at a dynamite factory led to the report that it was dynamite.

AUTHORITY.

As would be naturally expected with a substance so novel, powerful and useful as these powders, they have received careful attention and study, and been subjected to a variety of tests in all quarters with reference to their safety.

Men of science; experts in explosives; men in charge of works requiring their use; committees on behalf of transportation companies; military gentlemen and government commissions, and numerous others have examined the subject, and reported upon it,—some of them elaborately.

It would be inappropriate to reproduce these reports here or even extracts or their substance in detail. They are before the public and can be referred to by those who desire to do so.

We must content ourselves with a brief summary of the conclusions from all of them, to wit:

1. These powders are the most powerful of all the disrupting agents now in general use.

2. When properly made and economically transported they are, for hard rock work and sub-aqueous work, by far the most economical explosives in use.

3. They are the safest of all explosives both in transportation and use, many times safer than gunpowder, and when properly made and a few simple precautions taken, are as practically safe for transportation as if they were wholly inexplosive.

4. There is no good reason why, under proper regulations, they should not be transported in freight conveyances as freely as any ordinary merchandise.

STATE OF PRESENT LEGISLATION.

The congressional enactments now in force on this subject were passed July 3, 1866, before dynamite was invented, and are found in secs. 4,278, 4,279, 4,280, 5,353, 5,354 and 5,355 of the Revised Statutes.

Their design was to regulate the transportation of nitroglycerin, which is a liquid, but by accident they were so drawn as apparently to embrace all compounds containing nitroglycerin. Nevertheless, in a case before the U. S. District Court for the Northern District of New York, it was settled that dynamite or Giant Powder, was not within the law. But there is no certainty that a like result would be reached in all cases.

This uncertainty is itself a serious objection, and ought to be removed.

If the law applies to the powders they cannot be conveyed on public conveyances unless packed in metallic cases, and these cases be surrounded by plaster of Paris and their outside be conspicuously marked, "Nitroglycerin—Dangerous."

From what has gone before it is plain that each one of these requirements is the exact reverse of what it ought to be.

As to the metallic cases, they were necessary for liquid nitroglycerin but are not so for the the powders; besides, as already shown, they are more dangerous than wood or paper.

As to the plaster of Paris, its purpose was to absorb the nitroglycerin in case of leakage from the metallic vessel. But in making these powders this absorption has already taken place fully, and to guard against leakage from a dry thing is simple folly.

It will be noticed that this law recognizes the dynamite principle as the basis of safety.

As to marking the powders "*Nitroglycerin—Dangerous,*" it would be a double falsehood.

They are not nitroglycerin, nor are they dangerous, any more than wet gunpowder is gunpowder and dangerous.

This death's head and cross bones sign, marked conspicuously on a box of the powders, or on a dry goods box would prevent its reaching its destination as effectually as if marked "*Death to him that touches.*"

The practical result of these absurd and useless requirements has been their own *felo de se.* Not a pound of these powders has ever been carried in metallic cases, or surrounded with plaster of Paris, or marked "*Nitroglycerin—dangerous.*"

They are carried either by private conveyance at immense extra expense, or on public freight conveyances without being packed or marked as the law requires, or smuggled on passenger conveyances and by express under false names.

This shows that the law, so far as it affects these powders is worse than useless. But, on the other hand, if it does not apply to these powders there is no law of Congress that does, and they are free to be carried at pleasure on passenger conveyances, and in the midst of dense populations dripping with nitroglycerin—a state of things too alarming to require further comment.

This subject has already been considered by the Legislatures of several States, some of which have passed laws upon it substantially like the congressional one, and others have not, mainly because of their limited application. State laws are limited in their operation to such cases of transport as begin, continue and end within the State, while almost all transportations of these powders extend beyond a single State, and are, therefore, exclusively within congressional jurisdiction.

The great vice of the laws now in force is that they recognize no difference between liquid nitroglycerin and these powders.

Under the law as it now stands, the liquid can go where the powder can, and the powder cannot go where the liquid cannot.

Such a law as this ought not to stand another day.

The practical world has, for ten years

past, thoroughly recognized a difference—why should not law makers do so?

If the claims now made against nitroglycerin are true, ought it longer to be permitted to be transported in the dangerous manner laid down in the law?

And if the claims made in behalf of the powders are true, ought they to be classed with liquid nitroglycerin?

And if the law does not apply to the powders, and they are free to be carried on passenger conveyances, even when in a leaky and dangerous condition, ought not a new law against this to be enacted forthwith?

There never was a case where the necessity for immediate and intelligent action was more apparent and pressing than in this.

If, what we have written is true, our conclusions are undoubtedly just and proper; if, what we have written is not true, it can be easily disproved; but until it is so disproved the duty of at least a careful examination of the subject is palpably clear.

GENERAL FEATURES OF A PROPER LAW.

We now come to the practical question as to what should be the nature of the regulations on this subject.

They are few, simple and obvious:

First: As liquid nitroglycerin is known to be dangerous—so much so that no prudent manufacturer asks or attempts to transport it—and especially as there is now no necessity for it—its transportation on public conveyances and by common carriers should be absolutely prohibited, not only on passenger conveyances but also on freight conveyances.

Second: Leaky powders ought to be classed with liquid nitroglycerin, and wholly debarred from transportation in like manner.

Third: The dry and safe powders ought to be carried on all public freight conveyances.

The test of safety should be this, that when at the highest temperature to which they are to be exposed during their transit, they will not leak or part with any portion of the nitroglycerin which they contain when so placed that it can drain therefrom if it will.

Fourth: These powders, even when dry and safe, ought not to be transported on passenger conveyances. The ground on which this regulation ought to be made is that the powders are *explosive* and the persons of passengers ought not to be subjected to *any* risk however remote from this source.

However safe the powders there would be *fear.* To the timid and ignorant this fear would amount to intense pain. Passengers ought not to be thus tormented.

Besides there is no necessity for transport on passenger conveyances if one by freight is open.

Fifth: The powders ought not to be packed in metallic cases. The fact that metallic cases add almost nothing to the danger and that no accident has ever occurred from this cause, is no good ground against the regulation.

Sixth: Large percussion caps or exploders, or other things whose explosion by fire will explode the powders ought not to be transported in conjunction with them. If the Worcester explosion was caused by the exploders on board it is a case illustrating the propriety of this regulation.

Seventh: Each separate package ought to be marked on the outside with the name of the contents so as to be legible to those who handle it.

These are all the regulations which an experience of ten years has suggested as necessary.

Some of these are almost useless but none of them absurd like most of those now in force. They are suggested out of abundant caution and because of the extremely sensitive state of the public mind on the subject of nitroglycerin.

Under these regulations these powders will be a hundred fold less dangerous than gunpowder and we may confidently hope and expect there will be no accidents whatever in their transport.

◆

Draining of the Zuyder Zee.—A dam 40 kilometers ($24\frac{3}{4}$ miles) long, 50 meters broad at its base and $1\frac{1}{2}$ meters above the usual level of high water, is to be carried across the gulf. Upon this will be erected pumping engines capable of discharging 1,716,000,000 gallons a day. Estimating the average depth of the gulf at about $4\frac{1}{2}$ meters, it will make steady pumping for 16 years to empty the enclosure.

THE RATE OF SET OF METALS SUBJECTED TO STRAIN FOR CONSIDERABLE PERIODS OF TIME.

By Prof. ROBERT H. THURSTON.

Transactions of American Society of Civil Engineers.

Section I.—On the Observed Decrease of Resistance at a Fixed Distortion.—The writer has, in a preceding paper shown, by reference to experimental researches in which he had then engaged, that some classes of metals, as ordinary iron and steel, when subjected to strain and distortion by a force exceeding the resistance of the material within the elastic limit, take a set and are stiffened by that act, and exhibit an exaltation of the elastic limit. It was also shown that other classes, like tin, and similarly viscous and ductile materials, exhibit flow and a depression of their limits of elasticity when similarly treated. It was further shown, that the former class when subjected to loads, even approaching their ultimate strength, took a certain set and remained apparently indefinitely without further distortion; while the second class, under very moderate loads, frequently exhibit a gradual yielding, a progressive distortion, until fracture took place, sometimes under stresses which were but a fraction of those which were found required to break such metals quickly, and when time was not allowed for flow to occur. It was noted that increase of rapidity of distortion and fracture produced increase of resistance in the latter, or "tin-class," and decrease of resisting power in the first, or "iron-class," and *vice versa.*

The writer has since instituted experiments upon metals of both classes to determine how rapidly set, in each class, took place; the earlier experiment just referred to, having confirmed a suspicion long existing among engineers and experimentalists, that the phenomenon was a molecular change, as well as of the mass, and that time was required for its complete development. Prof. Norton has also shown by experiment that this set is partially temporary, the bar relieving itself of distortion in some degree, on removal of the load. Both that experimenter and the writer had detected some peculiar variations of form during this recovery, and the experiments of the latter, as detailed in the preceding paper, exhibited at times a gradual recovery of straightening power in a confined and flexed bar. The following will be found interesting, and perhaps, important, as showing how these molecular changes progress.

Bars were prepared of square section, 1 inch in breadth and depth, and 22 inches in length, between bearings. They were flexed in a machine for testing the resistance of materials to transverse stress, as described in the preceding paper and the load and deflection carefully measured. As the bars were retained at a constant deflection, their effort to resume their original form gradually decreased, and the amount of this effort was, from time to time, noted. When this effort or resistance had become considerably decreased, the bar was released, and the set measured. This operation was repeated with each, until the law of decrease of elastic resistance was detected. Curves were constructed, illustrating graphically this aw, and exhibiting in more satisfactorily and more plainly than the tubular record.

The following is the record for the bars of iron, of tin, and of two alloys. The iron bar No. 648, was subjected to a load 1,003 pounds, somewhat less than one-half its maximum, and its deflection was found to be 0.0995 inch. Removing the load, the set was 0.0049 inch. Restoring the load (1,000 pounds, + 3 pounds due to the weight of the bar), the deflection was 0.1001 inch, and the bar was held at this deflection and the decrease of resistance observed. In 25 minutes, it had become 999 pounds; in 1 hour 40 minutes, 991 pounds; in 4 hours 35 minutes, 987 pounds, and in 5 hours 20 minutes, 987 pounds. The set was then found to be 0.007 inch under the weight of the bar itself.

Restoring the last observed load, the deflection was 0.0991 inch, and the original load of 1,003 pounds increased it to 0.1003 inch.

A second trial of the same bar under a load of 1,603 pounds gave a deflection of 0.2548 inches, and a set, on removal, of 0.1091 inch. Restoring the load, the deflection became 0.287 inch, and the resistance to flexion decreased in 6 hours 3 minutes, from 1,603 to 1,457 pounds, at which latter time the set was found to be 0.1451 inch. Restoring the load of 1,457 pounds, the deflection was 0.2863 inch, and the original load, 1,603 pounds, being brought upon it, its deflection increased to 0.3016 inch, an increase nearly twenty per cent. above the original deflection.

In the first trial the loss of stiffness, as measured by the decrease of effort to straighten itself, and which is here taken to measure the *rate of set,* is seen to have been nearly proportional to the time at first, becoming constant after 4½ hours. On the second trial, after a considerable set, produced by a heavy load, the set became constant after about one hour, and so remained to the end of the trial.

No. 655 was a bar of Queensland tin, received from the Commissioner of that country at the Centennial Exhibition, and which was found to be remarkably pure. A load of 100 pounds gave a deflection of 0.2109 inch, and produced a set of 0.1753 inch. The same load restored, deflected the bar 0.2415 inch, which deflection being retained, the effort to regain the original shape decreased in one minute from 100 to 70 pounds, in three minutes to 62, and in eight minutes to 56 pounds. The original load of 100 pounds then brought the deflection to 0.3033 inch, nearly fifty per cent. more than at first.

A bar, No. 599, of copper-zinc alloy similarly tested, deflected 0.5209 inch under 1,233 pounds, and took a set of 0.2736 inch after being held at that deflection fifteen minutes, the effort falling, meantime to 1,137 pounds. Restoring the load of 1,137 pounds, the deflection became 0.5131 inch, and the original load of 1,233 pounds brought it to 0.5456 inch. The bar was now held at this deflection and the set gradually took place, the effort falling in fifteen minutes to 1,133 pounds—four per cent. more than at the first observation—in twenty-two minutes to 1,093, in forty-six minutes to 1,063, in sixty-three minutes to 1,043, in 91½ minutes to 1,003, and in 118 minutes to 911 pounds; at which last strain the bar broke three minutes later, the deflection remaining unchanged up to the instant of fracture. This remarkable case has already been referred to in an earlier paper, when treating of the effect of time in producing variation of resistance and of the elastic limit.

Nos. 561, copper-tin, and 612, copper-zinc, were compositions which behaved quite similarly to the iron bar at its first trial, the set apparently becoming nearly complete in the first after one hour, and in the second after three or four hours.

In all of these metals, the set and the loss of effort to resume the original form, were phenomena requiring time for their progress, and in all, except in the case of No. 599—which was loaded heavily—the change gradually became less and less rapid, tending constantly toward a maximum.

So far as the observation of the writer has yet extended, the latter is always the case under light loads. As heavier loads are added, and the maximum resistance of the material is approached, the change continues to progress longer, and, as in the case of the brass above described, it may progress so far as to produce rupture, when the load becomes heavy, if the metal does not belong to the "iron-class." The brass broke under a stress 25 per cent. less than it had actually sustained previously.

There is no evidence that iron or steel ever exhibits this treacherous and exceedingly dangerous behaviour; but, on the contrary, it seems always to carry a load, once borne, however near the maximum it may be. This difference is here, quite as marked as in the experiments previously reported, upon the elevation and the depression of the elastic limit by strain; and no one can fail to note the value in construction of this quality of that metal which is the chief reliance of the engineer in nearly every branch of his art. These principles will find numberless applications in the practice of every member of the profession.

The records are herewith presented, and the curves representing them, shown in Plate I:

RECORDS OF EXPERIMENTS ON RATE OF SET OR DECREASE OF RESISTANCE AND INCREASE OF SET OF METALS WITH TIME.

Bars 1 inch square, 22 inches between supports.

Time.	Load.	Loss of Load.	Deflection.	Set.
Minutes	Pounds.	Pounds.	Inches.	Inches.
No. 648. WROUGHT IRON.				
First Trial.				
..	1,003	..	0.0995	..
..	3	..	..	0.0049
..	1,003	..	0.1001	..
25	999	4	0.1001	..
100	991	12	0.1001	..
275	987	16	0.1001	..
320	987	16	0.1001	..
320	3	..	..	0.007
322	987	..	0.991	..
322	1,003	..	0.1003	..
..	2,720	..	2.64	..
Second Trial.				
..	1,003	..	2.2548	..
..	3	..	..	0.1091
..	1,603	..	0.287	..
1	1,521	82	0.287	..
2	1,493	110	0.287	..
3	1,483	120	0.287	..
23	1,463	140	0.287	..
53	1,461	142	0.287	..
133	1,459	144	0.287	..
193	1,457	146	0.287	..
363	1,457	146	0.287	..
363	3	..	..	0.1481
..	1,457	..	0.2863	..
..	1,603	..	0.3016	..
..	2,720	..	2.64	..
..	..	..	..	..
No. 561. 27.5 PARTS COPPER, 72.5 PARTS TIN.				
..	160	..	0.0696	..
..	5	..	..	0.0145
..	160	..	0.072	..
1	154	6	0.072	..
3	150	10	0.072	..
2,640	104	56	0.072	..
4,140	100	60	0.072	..
..	5	..	..	0.04
..	100	..	0.0763	..
..	160	..	0.097	..
..	320	..	0.22	Broke.
No. 599. 10 PARTS COPPER, 90 PARTS ZINC.				
..	1,233	..	0.5209	..
15	1,137	..	0.5209	..
..	3	..	..	0.2736
..	1,137	..	0.5131	..
..	1,233	..	0.5456	..
15	1,133	100	0.5456	..
28	1,093	140	0.5456	..
40	1,070	163	0.5456	..
46	1,063	170	0.5456	..
63	1,043	190	0.5456	..
77.5	1,023	210	0.5456	..
91.5	1,003	230	0.5456	..
96.5	993	240	0.5456	..
118	911	322	..	..
121	911	326	..	Broke.
No. 612. 47.5 PARTS COPPER, 52.5 PARTS ZINC.				
..	800	..	0.3332	..
..	3	..	..	0.1478
..	800	..	0.3366	..
5	790	10	0.3366	..
25	778	22	0.3366	..
120	766	34	0.3366	..
480	756	44	0.3366	..
1,320	751	49	0.3366	..
..	3	..	..	0.1688
..	751	..	0.3364	..
..	800	..	0.349	..
..	1,100	..	..	Broke.
No. 655. QUEENSLAND TIN.				
..	100	..	0.2109	..
..	3	..	..	0.1753
..	100	..	0.2415	..
1	70	30	0.2415	..
3	62	38	0.2415	..
8	56	44	0.2415	..
..	100	..	0.3033	..
..	150	..	Bent rapidly.	

SECTION II.—THE OBSERVED INCREASE OF DEFLECTION UNDER STATIC LOAD.—In the preceding section, the writer presented results of an investigation made to determine the time required to produce "set" in metals belonging to the two typical classes, which exhibit—the one an exaltation and the other a depression of the elastic limit under strain.

The experiments there described, were made by means of a testing machine in which the test piece could be securely held at a given degree of distortion, and its effort to recover its form measured at intervals, until the progressive loss of effort could no longer be detected, and until it was thus indicated that set had become complete.

The deductions were :

That in metals of all classes, under light loads, this decrease of effort and rate of set become less and less noticeable until, after some time, no further change can be observed, and the set is permanent:

That in metals of the "tin class," or those which had been found to exhibit a depression of the elastic limit with strain, a heavy load, *i. e.*, a load considerably exceeding the proof-strain, the loss of effort continued until, before the set had become complete, the test piece yielded entirely:

And that in the metals of the "iron-class," or those exhibiting an elevation of elastic limit by strain, the set became a maximum and permanent and the test-piece remained unbroken, no matter how near the maximum load the strain may have been.

The experiments here described were conducted with the same object as those above referred to. In these experiments, however, the load, instead of the distortion, was made constant, and deflection was allowed to progress, its rate being observed, until the test-piece either broke under the load or rapidly yielded, or until a permanent set was produced. It will be seen that the results of these experiments are in striking accordance with those conducted in the manner previously described; they exhibit the fact of a gradually changing rate of set for the several cases of light or heavy loads, and illustrate the striking and important distinctions between the two classes of metals even more plainly than the preceding. The accompanying record and the strain-diagrams, (Plate II), which are its graphical representation, will assist the reader in comprehending the method of research and its results. All test-pieces were of one-inch square section, and loaded at the middle. The bearings were 22 inches apart.

No. 651 was of wrought iron from the same bar with No. 648, already described. This specimen subsequently gave way under a load of 2,587 pounds. Its rate of set was determined at about 60 per cent. of its ultimate resistance, or at 1,600 pounds. Its deflection, starting at 0.489 inch, increased in the first minute 0.1047, in the second minute 0.026, in the third minute 0.0125, in the fourth minute 0.0088, in the fifth minute 0.0063, and in the sixth minute 0.0031 inch; the total deflection being 0.5937, 0.6197, 0.6322, 0.641, 0.6473, and 0.6504 inch. In the succeeding 10 minutes the deflection only increased 0.0094 inch, or to 0.6598 inch, and remained at that point without increasing so much as 0.0001 inch, although the load was allowed to remain 344 minutes untouched. The bar had evidently taken a permanent set, and it seems to the writer probable, that it would have remained at that deflection indefinitely, and have been perfectly free from liability to fracture for any length of time.

This bar finally yielded completely, under a load of 2,589 pounds, deflecting 4.67 inches.

No. 479 was a copper bar containing 3¾ per cent. of tin. Its behaviour may be taken as typical of that of the whole "tin-class" of metals, as the preceding illustrates the behavior of the "iron-class" under heavy loads. It was subjected to two trials, the one under a load of 700 and the other of 1,000 pounds, and broke under the latter load, after having sustained it 1¼ hours. The behavior of this bar will be considered especially interesting, if its record and strain-diagram are compared with those of No. 599, previously given, which latter specimen broke after 121 minutes when held at a constant deflection of 0.5456 inch; its resistance gradually falling from an initial amount of 1,233 pounds, to 911 pounds at the instant before breaking.

This bar, No. 479, was loaded with 700 pounds "dead weight," and at once deflected 0.441 inch. The deflection increased 0.118 inch in the first 5 minutes, 0.024 in the second 5 minutes, 0.018 in the second 10 minutes, 0.17 in the fourth, 0.012 in the fifth, and 0.008 inch in the sixth 10 minute-period, the total set increasing from 0.441 to 0.65 inch. The record and the strain-diagram, (Plate II), shows that, at the termination of this trial, the deflection was regularly increasing. The load was then removed and the set was found to be 0.524 inch, the bar springing back 0.126 inch on removal of the weight.

The bar was again loaded with 1,000 pounds. The first deflection which could be caught and measured, was 3.118 inches and the increase at first followed the parabolic law noted in the preceding cases, but quickly became accelerated; this sudden change of law is best seen on the strain-diagram. The new rate of increase continued until fracture actually occurred, at the end of 1¼ hours, and at a deflection of 4.506 inches.

This bar was of very different composition from No. 599; it is a member of the "tin-class," however, and it is seen, by examining their records and strain-diagrams, that these specimens, tested under radically different conditions, both illustrate the peculiar characteristics of the class, by similarly exhibiting its treacherous nature.

RECORD OF EXPERIMENTS WITH "DEAD LOADS" TO DETERMINE THE INCREASE OF DEFLECTION WITH TIME, OR RATE OF SET.

Bars, 1 inch square, 22 inches between supports.

Load applied at the middle.

Time.	Deflection.	Increase. Inches.	
Minutes.	Inches.	Difference.	Total.
No. 651. WROUGHT IRON.			
Load, 1,600 pounds.			
0	0.489	....	....
1	0.5937	0.1047	0.1047
2	0.6197	0.026	0.1307
3	0.6322	0.0125	0.1432
4	0.641	0.0088	0.152
5	0.6473	0.0063	0.1583
6	0.6504	0.0031	0.1614
16	0.6598	0.0094	0.1708
344	0.6598	0.0000	0.1708
Maximum load, 2,589 pounds; maximum deflection, 4.67 inches.			
No. 504. 0.557 PARTS COPPER, 99.443 PARTS TIN.			
Load, 110 pounds.			
0	0.323	....	....
5	0.406	0.083	0.083
845	1.945	1.539	1.622
865	2.005	0.059	1.681
895	2.138	0.134	1.815
1,025	2.248	0.11	1.925
1,110	2.378	0.13	2.055
1,270	2.626	0.248	2.303
Maximum load, 130 pounds; maximum deflection, 8.11 inches.			
No. 479. 96.27 PARTS COPPER, 3.73 PARTS TIN.			
Load, 700 pounds.			
0	0.441	....	....
5	0.559	0.118	0.118
10	0.583	0.024	0.142
20	0.601	0.018	0.16
30	0.618	0.017	0.177
40	0.63	0.012	0.189
50	0.642	0.012	0.201
60	0.65	0.008	0.209
Set.	0.524	....	....
Second Trial.—Load, 1,000 pounds.			
0	3.118	....	....
5	3.54	0.422	0.422
15	3.66	0.12	0.542
45	4.102	0.442	0.984
75	7.634	3.522	4.506
Bar broke under 1,000 pounds.			
No. 501. 9.7 PARTS COPPER, 90.3 PARTS TIN.			
Load, 160 pounds.			
0	1.294	....	....
10	1.319	0.025	0.025
70	1.463	0.144	0.169
130	1.53	0.067	0.236
310	1.691	0.161	0.397
400	1.766	0.075	0.472
460	1.811	0.045	0.517
1,360	2.534	0.723	1.24
1,475	2.697	0.163	1.403
1,565	2.782	0.085	1.488
1,730	2.938	0.156	1.644
1,880	3.136	0.198	1.842
2,780	3.798	0.662	2.504
2,940	4.274	0.476	2.98
3,000	4.349	0.075	3.055
3,295	5.097	0.748	3.803
Bar left under strain at night and found broken in the morning.			

No. 504 was a bar of tin containing about 0.6 per cent. of copper—the opposite end of the scale—and exhibited precisely similar behavior, taking a set of

0.323 inch under 110 pounds and steadily giving way and deflecting uninterruptedly until the trial ended at the end of 1,270 minutes, over twenty-one hours. This bar, subsequently, was, by a maximum stress of 130 pounds, rapidly broken down to a deflection of 8.11 inches.

No. 501 presents the finest illustration yet entered in the record book of the Mechanical Laboratory of the Stevens Institute of Technology. The test extended over nearly 2½ days under observation, and then left for the night, was found next morning broken. The time of fracture is therefore unknown, as is the ultimate deflection. The record is, however, sufficient to determine the law, and the strain-diagram (Plate II) is seen to be similar to that of the second test of No. 479, exhibiting the same tendency to the parabolic shape and the same change of law and reversal of curvature preceding final rupture, and illustrates even more strikingly the fact that this class of metals is not safe against final rupture, even though the load may have been borne a considerable time, and have apparently been shown, *by actual test*, to be capable of sustaining it. A strain-diagram of each of the latter two bars is exhibited on a reduced scale, to present to the eye, more strikingly, this important characteristic. (Plate II).

A comparison of the records and the strain-diagrams (Plate II), with those of Section I, in illustration of the behavior of the two classes of metals under constant deflection, is most instructive. The light thus thrown upon the phenomena of distortion and fracture may be of great service to all who are engaged in construction. It will be necessary to make many experiments to determine under what fraction of their ultimate resistance to rapidly applied and removed loads the members of the "tin-class"—the viscous metals—will be safe under static permanent loads. Their behavior under shocks of various intensities remains also to be determined. The most probable and most satisfactory conclusion which seems likely to be finally reached is, perhaps, that the "iron-class" of metals are capable of carrying indefinitely any load which they have once borne, and that, in some manner—by the relief of internal strain as suggested by the writer* or by some other process—their rest under a load renders them, as time goes on, more and more safe under that load.

The law of deflection and of rate of set, as illustrated graphically by the strain-diagrams given in this and in the preceding paper, is expressed for the lighter loads by equation of the form

$$Y=AT-BT^2$$

in which Y is the deflection or the set, both quantities varying together in this case, and T is the time; A and B being constant co-efficients to be determined for special cases.

For heavy loads, after the first sudden deflection and set, the equation is seen to be

$$Y=AT$$

in which for iron, $A=\frac{1}{Y}$ and for the tin-class A is a constant multiplier up to a limit x, Nos. 501, 479, at which it varies as some new function of the time.

The values of constants for the various metals remain to be determined. The question whether this change in the value of the Modulus of Rupture, as exhibited in the preceding section, and of the value of the quantity representing in the usual formulas the amount of deflection is due to a change in the Modulus of Elasticity, to simple flow, or to a variation of cohesive force, remains to be considered.

SECTION III.—ELEVATION OF ELASTIC LIMIT IN GUN BRONZE.—The writer would refer to the recent criticisms of Prof. Kick, of Prague, on "Autographically produced Strain-Diagrams, and the Elevation of the elastic Limit by Strain." In a late issue of *Dingler's Polytechnisches Journal*, the Professor, in rejoinder to my reply to his criticism of my paper on the subject of the strength of materials and the elevation of the elastic limit by strain, asserts :

1°. That I use his formula, for determining the errors of apparatus due to velocity of motion, incorrectly.

2°. That I claim to be able to deduce the amount of work done in deformation of the test-piece from automatically produced diagrams in which the abscissas

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* Wire makers have learned that newly made wire is considerably weaker than similar wire which has been so long made as to afford time for relief, by flow of the internal straining introduced by the process of drawing.

are proportional to the angular motion of the handle.

3°. That the error introduced by a blow will be greater as velocities are greater.

4°. That experimental proof of the elevation of the elastic limit in gun-bronze by strain was presented to Prof. Kick and others in August, 1873, by Gen. Uchatius, and that his discovery antedates that announced by me to this Society in November of that year.

I have been prevented by illness from noticing that statement before. I would now say:

1°. That I used his formula purposely as its author applied it, in criticising my paper, in order to make more striking the refutation. My reply is just as complete as if I had applied it to the more intricate case.

2°. That the abscissas of the strain-diagrams produced automatically are not proportional to the "motion of the handle," but to the distortion of the test-piece, and that this singular misapprehension of the subject of the criticism may be the result of that which prompted the original criticism.

3°. That I have distinctly disclaimed all intention of ascribing to the Autographic Recording Testing Machine the power of giving *quantitative* results when affected by shocks, and that my paper stands perfectly good, notwithstanding this fact, which was there implicitly stated, and would have been explicitly stated had it not seemed so perfectly obvious.

4°. That I shall endeavor, health and time permitting, to present proof that the experiments of Gen. Uchatius, in 1873 and before the date of my paper, do not prove an elevation of the elastic limit by strain; and, finally,

5°. That *gun-bronze does not possess this property.*

The phenomenon there shown was due, I think, simply to that condensation of metal by pressure, such as occurs in Whitworth steel and metals in which compression had similarly closed up the pores, and, by thus increasing their density, increased the resisting power of the metal.

The phenomenon which I have described in earlier papers as exhibited by autographic strain-diagrams and otherwise, the increase of the resistance of the material to distortion, is an effect, apparently, of internal molecular changes which do not affect density, and which, in tin and some other metals, result in a *depression of the elastic limit.* If my critic, or any other experimentalist, will study this phenomenon as I have done, he will find, I think, that gun-bronze belongs to what I have called the "tin-class," in which strain produces a *depression* of the elastic limit whenever any effect can be observed at all. I therefore think, the claim of my critic in behalf of the distinguished officer mentioned, can not be sustained.

THE DIFFERENT METHODS OF STEEL MANUFACTURE.

From "Iron."

The manufacture of steel by a process as simple as possible, at the lowest cost and of the best quality, has called forth, especially of late years, the exercise of much inventive ability on the part of both chemists and engineers, both at home and abroad. There has resulted such a variety of differing methods that some systematic classification of the processes has become very necessary. In the *Mittheilungen des Hannoverschen Gewerbe-Vereines,* Professor Heeren publishes the complete classification, a translation of which is given below, and which will be found both instructive and of value for purposes of reference. Steel occupies nearly the middle place between cast and wrought iron in its proportion of carbon; it may be prepared either by decarburising pig-iron, or, on the contrary, by causing wrought iron to absorb carbon. The processes to accomplish these ends may be arranged under five principal heads: A, fabrication of steel by decarburization of crude or pig-iron; B, by carburization of wrought iron; C, by mixing a wrought iron poor in carbon with pig-iron rich in the same; D, by by mixing pig-iron with ore (the pig yields carbon which reduces the ore and transforms the reduced iron into steel); E, directly by means of ore; F, cast steel. Subdividing these systems, we have the following methods under each heading:

A.—METHODS BY DECARBURISING THE CRUDE IRON.

(1) Steel obtained by a long heating of the crude iron in an oxidising atmosphere, the metal not being brought to fusion. (*a*) Turner's method in sand, where the deoxidation is produced by means of the oxygen in the air. (*b*) Jullien's method, in forged scales or spathic ore. This produces malleable iron. (*c*) Herzeele's method in steam. (*d*) Thomas' method in carbonic acid. The last two processes have not been employed to any great extent.

(2) Natural steel: In this method, employed since the earliest times, the crude iron is melted in a refining furnace with wood charcoal, and decarburised by the ferrous oxide of the scoria. The product is purified by a repeated refining.

(3) Puddling: This process is the same as the preceding, from a chemical point of view, but is practised in a reverberatory furnace heated with coal. It is necessary to purify the product by repeated refining, or by transforming it into cast steel.

The construction of puddling furnaces has undergone many changes. We distinguish (*a*) the ordinary puddling furnace with fixed hearth and heated by coal, (*b*) the same heated by lignite or peat, (*c*) the puddling furnaces of Schafthautl and others, with mechanical rabbles, designed to diminish the labor so fatiguing to the workman. These, however, have been entirely suspended by the new systems. (*d*) The Danks furnace, the hearth of which is formed of a hollow cylinder placed horizontally, and turning about its axis. It gives a product of exellent quality and is economical. The interior lining, however, is difficult to maintain. (*e*) The Ehrenworth furnace has a horizontal circular hearth, turning about a vertical axis. (*f*) The Pernot furnace also has a circular sole, which, however, is not horizontal, but slightly inclined, so that during its rotation the iron and scoriæ run to the lowest point, and are thus in a state of continual motion; while the elevated part of the hearth, together with the iron and scoriæ thereto adherent, are submitted to the oxidising action of the air. Professor Heeren thinks this furnace to be best, because it realises the advantages of mechanical puddling without needing any special lining.

(4) The Bessemer process: A current of air finely divided is passed through the liquid crude iron. The carbon, silicon and a part of the iron burn, and the temperature is so highly elevated that the iron, decarburised in part or transformed into steel, remains molten. It is then run into moulds.

(5) Berard's modification of the above: Air and gases are alternately introduced into the retort with different advantages.

(6) Peter's process: The liquefied crude iron in a reverberatory furnace falls in the form of rain, in a vertical chamber in which the furnace gases also pass, and in which air is blown so as to decarbonise the metal to the highest degree.

B.—METHODS BY CARBURISATION OF WROUGHT IRON.

(1) Indian or Wootz steel: Wrought iron of extraordinary purity, obtained by treating a very pure ore in small chamber furnaces by the direct method, is hammered, made into bars, cut into short pieces and placed in small crucibles with a few green leaves. The crucibles are hermetically sealed and heated for a long time at a high temperature. The iron is transformed into steel by uniting with it the carbon contained in the leaves and the steel even partially melts. These half melted masses furnish the famous sword blades and plates of Persia and Damascus.

(2) There are several other processes resembling the Indian, which, however, are not carried on on a large scale. There are (*a*) The Mushet process, in which wrought iron, obtained by the ordinary refining method, is melted with powdered wood charcoal. (*b*) The Vickers' process, analogous to the preceding, with the addition of oxide of manganese. (*c*) The Stourbridge, Brooman, Thomas and

Binks processes, based on identical principles.

(3) English cemented steel: Wrought iron of the best possible quality is, in the shape of bars, packed together in clay boxes, together with wood charcoal coarsely pulverised. The heating continues for two or three weeks. Without melting the iron is changed into steel, which by remelting is transformed into cast steel.

(4) Parry's cupola steel: Fragments of wrought iron, melted in the cupola with a large consumption of coke or wood charcoal, may be transformed into steel or even into cast iron, according to the length of the operation. This system offers an advantageous method of utilizing the scrap, and requires no special apparatus.

(5) Chenot's process: In this the ore is reduced by heating it progressively with coal. A non-melted iron sponge is obtained, which is ground and separated as far as possible from the gangues by the aid of a magnet. Lastly, it is mixed by carboniferous substances, and melted under pressure. The principal disadvantage of this process is the difficulty of separating the gangues without losing the steel.

6. Case-hardening has for its object the transformation of the surface of wrought-iron objects into steel. It is done in two ways. (*a*) The pieces are placed in small sheet iron boxes and surrounded with chips of wood. The boxes are hermetically closed and heated in a forge fire for fifteen or thirty minutes, to an intense red heat. They are then removed quickly, opened, and their contents thrown into cold water, thereby the exterior steel shell is rendered as hard as glass. (*b*) The pieces are heated to a whitish red and moistened with ferrocyanide of potassium, which acts by its cyanogen on the iron, and transforms the surface into steel.

C.—METHODS BY FUSION OF A MIXTURE OF CAST AND WROUGHT IRON.

The two materials may be both, or only one of them, used in a melted state.

(1) Bessemer steel, prepared by the ordinary method: The crude and wrought iron here are both liquid, while, as we have previously said, cast iron may be directly transformed into steel. The method most followed, and which leads most surely to the end in view, consists in completely decarburising the crude iron in the converter, and in adding to the melted metallic iron a rigorously-determined quality of liquid crude iron. The carbon of the latter affects the previously decarburised iron, and makes a steel containing a given proportion of carbon.

(2) Crucible steel is obtained by melting in crucibles a mixture of crude and wrought iron. The former liquefies first and slowly melts the latter.

(3) Martin's steel is similarly made by replacing the crucible with a reverberatory furnace. The crude iron is liquefied under a thin layer of scoria on the concave hearth of a reverberatory furnace, heated to an intense red-white heat by a Siemens regenerator. Scraps of steel and wrought iron of all kinds in desired quantity are added, and the steel is run into moulds of cast iron.

D.—METHODS BY A MIXTURE OF CAST IRON AND ORE.

Uchatius steel: The cast iron is granulated by running it into water while molten, and the grains are melted with spathic ore, peroxide of manganese and wrought iron in crucibles. The ferrous oxide of the spathic ore is reduced by the carbon of the cast iron, and the surplus of carbon unites with the wrought iron to make steel.

E.—METHODS BY PREPARATION DIRECT FROM THE ORE.

The Siemens direct process: The ore is melted alone, without addition of reducing material, at a very elevated temperature; then the iron is reduced and transformed into wrought iron or into steel by adding coal.

F.—CAST STEEL.

For the purification of steel by fusion, cemented, forged and puddled steel are employed. To improve the qualities of the steel, and notably to augment its hardness, diverse substances are added. Thus we have: (1) silver steel; (2) nickel steel, and (3) wolfram or Mushet special steel.

MOMENTUM AND VIS VIVA.

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Contributed to Van Nostrand's Magazine.

III.

IV. THE PRESSURE PRODUCED BY IMPACT.

The best writers on the subject of impact recognize clearly that it is impossible to calculate, even approximately, the pressure which the accumulated energy of a moving body will enable it to produce by impact on another body, without we know beforehand just how much the two bodies will be compressed or forced out of shape by the expenditure of that energy, and also what is the law according to which the pressure varies as the distortion proceeds. This last requisite also implies, what must be evident by a little thought, that the total pressure between one body and another is not constant, but variable during even the short time of the impact. In "The Mechanics of Engineering," published by W. Whewell in 1841, there is a chapter on impact, in which he discusses the amount of compression and the pressure produced by the fall of an iron hammer from a certain height on an iron anvil; he also gives formulas for finding how far a given hammer will drive a nail or a pile, taking care to consider the effect of compression, as well as weight and friction. Many later writers have given rules for determining the pressure produced by impact, neglecting altogether the consideration of the amount of yielding or distortion of the substances, and thus making their rules of absolutely no value for general application. And we find, as we might expect, that these rules conflict with one another most remarkably.

The right view of the question is of practical importance, as the supposed results are sometimes made the basis for rules of safety in driving piles for foundations. We will, therefore, examine some of the principles concerning the pressure of impact which are laid down in text-books.

One author, who is in general very clear, states correctly the meaning of *momentum*, as already explained, but unfortunately goes on to say: "The momentum is also frequently called the *moving force* of the body, because it not only represents the intensity of the force required to overcome its motion, but also because the body would itself exert a force of this intensity against any obstacle tending to resist its motion." This statement would, therefore, make the pressure produced by a moving body when it strikes "any obstacle" equal simply to MV, or if we denote the pressure by P, and the weight of a body by W, previously denoted by G, we should have $P=\frac{WV}{32\frac{1}{6}}$.

Another common text-book states that "Beaufoy determined that a body of one lb. weight, with a velocity of one foot in a second strikes with a pressure equal to 0.5003 lbs. To find the pressure produced by the impact of any projectile, we have the general formula, Pressure$=0.5003\ MV^2$." The context shows that M in this equation stands simply for the *weight* of the moving body, and the formula will then be merely, $P=0.5003\ WV^2$.

Haswell, in his "Engineers and Mechanics' Pocket Book," p. 419, admits that in the case of a pile-driver the yielding of the pile will materially affect the "impact," but he then makes the following statement: "By my experiments in 1852, to determine the *dynamical* effect of a falling body, it appeared that while the effect was directly as the velocity, it was far greater than that estimated by the usual formula $\sqrt{s2g}$, which, for a weight of one lb. falling two feet, would be 11.34, giving a momentum of 11.34 ft.-lbs. (*sic*); whereas, by the effect shown by the record of actual observations, it would be $VW\ 4.426=50$ lbs." This result being given in *lbs.*, and not *foot-lbs.*, I suppose it is intended to represent the maximum pressure of the blow. Of course it would be absurd to suppose that a one lb. weight by falling two feet could acquire energy enough to perform fifty foot lbs. of work. Haswell's formula I should then understand to be, $P=4.426\ WV$[*].

It is hardly worth while to add to the foregoing formulas one given by some writers, who are so careless as never to make a distinction between the *weight* and the *mass* of a body, and who also make the "moving force" equal simply to WV.

If we calculate by the preceding formulas the striking pressure of a 1,000 lb. cannon-ball having a velocity of 1,100 feet per second we have the following results:

$$P=\frac{WV}{32\frac{1}{6}} = 17.1 \text{ Tons.}$$

$$P=0.5003\ WV^2=302{,}681.5 \text{ Tons.}$$

$$P=4.426\ WV = 2{,}434.3 \text{ Tons.}$$

$$P= WV = 550.0 \text{ Tons.}$$

Where different formulas give results so astonishingly different as these it is not strange that a writer like Trautwine should say: "The force in lbs. with which a pile hammer makes its blow upon the head of a pile, cannot be calculated. All rules for that purpose are founded in error. We must here depend upon experience alone." Civ. Eng. Pocket Book, p. 321. We agree with this writer that in practical pile-driving it is impossible to calculate the pressure actually produced by the hammer on the pile. But the impossibility does not come from any real difficulty in the mathematics or the theory of impact, but simply from the fact that in practical cases we cannot get accurate data to work with. The intensity of the friction which resists the motion of the pile, changes with every blow of the ram, and so, probably, does the amount of compression and motion of the pile. Not knowing the circumstances of these, any calculations involving them will be liable to great error.

But it will be a help towards a true knowledge of the subject if we can have a formula which under certain supposable conditions will give us the maximum pressure due to impact, even though the conditions may not often be exactly fulfilled in practical cases. If we examine the foregoing formulas, we notice that that of Beaufoy makes the pressure vary as the *square* of the velocity, while all the others make it simply as the *velocity*. Haswell, *loc. cit.*, states that "the *effect* of the blow of a ram, or monkey, of a pile driver, is as the *square* of its velocity," but afterwards goes on, as before quoted, to give a formula from the record of actual observations, in which he makes the result in *pounds* vary as the *velocity* simply. This essential difference between Beaufoy and Haswell is of more importance than the difference of the coefficients in their formulas. But neither of those formulas involves the amount of yielding of the bodies between which impact occurs, and this defect alone would make any formula useless for general application.

To derive a formula for the pressure due to impact, involving the amount of yielding of the bodies, we must assume some law according to which the pressure shall be supposed to vary as the distortion proceeds. If an ordinary coiled spring is either stretched or compressed, its tension varies, within certain limits, directly in proportion to the amount of distortion from the position of rest, the tension being zero in this position. Suppose a spring following this law to be itself without weight, and let an incompressible body whose weight is W be projected against the spring with the velocity V, producing a movement of the free end of the spring through a distance d in stopping the projected body, what will be the number of pounds pressure on the spring at the instant of its greatest compression?

We may easily compute this pressure in terms of the quantity of *work* required to compress the spring through the given distance. For the *work* required is equal to the *mean* pressure multiplied by the distance, and if P represent the pressure at the instant of greatest compression, the mean pressure will, by the law of the spring, be $\frac{P}{2}$. If we denote the necessary quantity of work by Q, we shall then have $Q=\frac{P}{2}d$,

whence,
$$P=\frac{2\,Q}{d} \quad . \quad . \quad . \quad (1)$$

Now we have seen that the work

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[*] Since completing this article I have seen a curious discussion in the July and August Nos. of *The Journal of the Franklin Institute*, between Nystrom and Haswell, but without access to the details of Haswell's experiments, I do not see as it requires any change in my presentation of the subject.

which a moving body can perform in virtue of its velocity is $\frac{1}{2}MV^2$, or $\frac{WV^2}{64\frac{1}{3}}$. Putting this in the place of Q, in equation (1), we have for the pressure at the instant of greatest compresssion,

$$P=\frac{WV^2}{32\frac{1}{6}d} \quad . \quad . \quad . \quad . \quad (2)$$

The constant, $32\frac{1}{6}$, in this formula, supposes the value of V to be expressed in feet per second. In applying the formula, therefore, d must be reduced to feet or fractions of a foot. W may be expressed in any unit of weight, and P will then be in the same.

We have supposed the moving body to be incompressible, and the spring to yield with a resistance increasing uniformly with the amount of its compression. In actual cases of impact between two solids, their molecular springs would take the place of the coiled spring, and both bodies would yield more or less; but if we suppose the resistance to increase according to the same law, a similar formula would apply, assuming d as the distance passed through by the center of gravity of the moving body during impact. Now it is a fact shown by experiment, that within certain small limits any displacement of the molecules of an elastic solid, develops a resistance increasing directly as the amount of the displacement. Hence, within these limits, (which the form of the impinging bodies would modify) in a case where d could be determined, and the modulus of elasticity were known, we could calculate with some accuracy the maximum pressure produced in the impact of a moving solid upon a fixed obstacle. If the law of the increase of the resistance is not known, the pressure cannot be calculated.

From the formula, $P=\frac{WV^2}{32\frac{1}{6}d}$, it appears that, other things being equal, the pressure in case of impact is inversely proportional to the distance in which the moving body is brought to rest. The harder and more incompressible the substances, then, other things being equal, the greater will be the pressure produced by impact. If both the substances between which impact occurs were perfectly incompressible, the pressure from impact would be infinitely great, however small the mass or velocity of the moving body. It would therefore evidently be impossible, with any resistance, however great, to stop in *no* distance an incompressible body, however small, or moving however slowly.

It also appears from the formula, that the pressure from impact increases, *other things being equal*, with the *square* of the velocity of the moving body, instead of simply with the first power of it. How experimenters might be led to the opposite conclusion will be suggested further on.

In the Spring of 1870 I performed a series of experiments for comparison with the formula now given,

$$P=\frac{WV^2}{32\frac{1}{6}d}.$$

I found that I could measure the pressure of impact by the expansion of a spring, with less liability to error than by its compression. After a few preliminary trials I adopted the following plan:

A coiled steel wire spring about a foot in length and weighing three ounces was firmly suspended by one end. Screwed into the bottom of the spring was a small brass thimble, pierced in its center to allow the free passage of a steel wire up through the spring. To this wire near the middle was fitted tightly a small brass collar which could not pass below the thimble, and to the lower end of the wire a cylindrical iron weight was attached. An arrangement for supporting and dropping the weight accurately was made by filing a small notch in the upper part of the wire, which projected up through a hole in the support of the spring. After the weight had fallen freely a certain distance, the collar on the wire would catch on the thimble at the bottom of the spring, and extend the spring till its resistance stopped the weight and caused it to fly up again.

Behind the spring and weight thus arranged, a scale of equal parts was placed perpendicularly, so that the height at which the weight was supported could be accurately measured by placing a small square against the scale, under the weight. To measure to what distance the weight fell when dropped so as to stretch the spring, I first determined the lowest point approximately, and then arranged just beneath it a small wooden needle stuck into a dish of light meal; then raising this needle so that when the weight fell it would drive the needle down a very little and leave it at the lowest point, the extent of the fall could be very accurately determined.

Knowing then, the entire fall of the weight, and the extent to which it stretched the spring, and assuming that the resistance of the spring increased in proportion to the extension, (which is assumed in the ordinary spring balance, and which I found by frequent weighings to be quite closely true), and neglecting a slight loss from friction and heat, I had the data for calculating the maximum pressure which the impact of the weight should produce. (I have also neglected the weight of the spring, assuming that whatever work is spent in giving it vis viva will be given out again by it, by the time the greatest compression is reached). By suspending to the weight an extra load sufficient to draw it down till the extension of the spring was the same as that caused by impact, and weighing the whole load, the actual maximum pressure produced by impact was found, and compared with that calculated by the formula.

In calculating the pressure from the formula, the value of V is taken at what it would be if the weight fell freely for the whole distance, including the extension of the spring. This value is, of course, somewhat greater than the velocity actually attained by the weight; but there is a compensation in the action of gravity on the weight during the extension of the spring. This method reduces the calculation to what it would be if the weight were moving horizontally with the assumed velocity, and extended the spring by its vis viva only, without the assistance of gravity during the extension.

In the first of these experiments the weight of the drop was 14.79 ounces. A mean of ten trials, with an extreme variation from the mean of $\frac{1}{40}$th of an inch, gave for the whole fall of the weight 21.26 inches, and for the extension of the spring 10.82 inches. From these data the formula $P=\frac{WV^2}{32\frac{1}{6}d}$, would give a pressure equal to 58.1 ounces. The total observed steady weight required to extend the spring to the same degree was found to be 55.6 ounces, falling short of the calculated pressure by 2.5 ounces, or about $\frac{1}{23}$rd of the calculated pressure. Two other sets of observations under similar conditions, one of which is the first in the table below, gave results not differing materially from this.

In order to further extend my experiments, I suspended the same spring by a support near its middle, so as to have the effective length of the spring, when at rest, about five inches; making virtually a new and stiffer spring. I also procured an additional spring, about seventeen inches in length, of heavier wire and wound on a smaller core, so as to make a still stiffer spring. Its weight was about five ounces. It was fitted up like the former one, a little more care being taken in details. I experimented with this spring and a drop of about three pounds weight, and also with the drop used before, using first the full length of the spring, and afterwards cutting it in two, thus once more increasing the stiffness of the dynamometer.

I give a summary of the means of nine sets of experiments, separating them into divisions according as they were made with springs of different stiffness, increasing from I to IV:

(*See Table on following page.*)

It was to be expected that the tension calculated by the formula $P=\frac{WV^2}{32\frac{1}{6}d}$ would be a little in excess of the observed tension; for some of the *work* done would be wasted on friction, atmospheric resistance, &c. And in fact, all of the observed tensions are found to fall a little short of those calculated by this formula, yet approximating so closely to them, under a variety of conditions, that the differences may fairly be ascribed to these unconsidered losses. I found in general that with an increase of velocity, other things being the same, the error slightly increased, the proportional effect of these resistances being greater. Also the proportional error was greater when the suddenness of the shock was increased, as in experiments 6 and 7, which were made with the small drop and the stiffest spring.

The co-efficients in the formulas $P = 4.426\,WV$ and $P=0.5003\ WV^2$ are such that with values of V below 8.85

Stiffness of spring.	Reference No.	Weight of drop in ounces.	Height of fall in inches.	Velocity due to height in feet per second.	Extension of spring in inches.	Tension in oz. by $P=4.426\,WV$.	Tension in oz. by $P=0.5003\,WV^2$	Tension in oz. by $P=\frac{WV^2}{32\frac{1}{6}d}$.	Tension in oz. by observation	Ratio of excess by formula $P=\frac{WV^2}{32\frac{1}{6}d}$.
I	1	14.79	21.28	10.67	10.84	698.4	842.6	58.07	55.67	$\frac{1}{24}$
II	2	14.79	9.13	6.99	4.57	457.6	361.5	59.07	57.88	$\frac{1}{49}$
III	3	49.414	10.02	7.33	7.12	1,603.1	1,328.2	139.08	136.03	$\frac{1}{46}$
	4	49.414	14.35	8.77	8.55	1,918.0	1,901.2	165.87	162.96	$\frac{1}{57}$
	5	49.414	20.43	10.46	10.17	2,287.6	2,704.6	198.53	193.91	$\frac{1}{43}$
IV	6	14.79	4.51	4.92	1.89	322 0	179.1	70.58	66.66	$\frac{1}{18}$
	7	14.79	10.62	7.54	2.90	493.6	420.7	108.32	102.28	$\frac{1}{18}$
	8	49.414	8.51	6.75	4.85	1,476.2	1,126.2	173.41	170.91	$\frac{1}{69}$
	9	49.414	13 96	8.65	6.17	1,891.8	1,849.6	223.61	217.43	$\frac{1}{36}$

the pressure calculated by the former will be greater than that by the latter, but with values of V above 8.85 the reverse will be the case. It happens that in case of my experiments both of those formulas give results very much in excess of the observed pressures. This would not always be so. If I had used a very stiff dynamometer some of the tensions calculated by those formulas might have fallen below the observed, instead of above. I purposely employed springs that should yield considerably, so that the distance the drop passed while coming to rest might be more readily and accurately measured.

A discussion of the results of the table will show how experiments with regard to impact might be conducted, which should apparently lead to the conclusion that the resulting pressure is proportional simply to the first power of the velocity. A comparison of experiments 3, 4 and 5, which were performed with a single spring and a single drop, but at different velocities of this, shows that the observed pressures actually produced in these experiments, were very closely proportional to the velocities. If there were only those three experiments it might be assumed from them that the maximum pressure in case of impact would be equal to the product of the weight of a moving body and its velocity, multiplied by a certain constant. Making this assumption, I should find from the *observed* pressure in No. 3, the value of the constant to be 0.3755; from No. 4 the constant would be 0.3761; from No. 5 it would be 0.3751. The mean of these three nearly accordant values would be 0.3756. Now the formula $P=0.3756\,WV$ would apply very closely to any number of experiments, made with the same spring and the same moving weight at various velocities, and would apparently show that the pressure in case of impact varies as the first power of the velocity of the moving mass.

But we shall find that this formula will not apply, with any approach to accuracy, to the impact of the same weight on a spring of different stiffness. Suppose we calculate the pressure which this formula would give when applied to experiments 8 and 9, which were made with the same moving weight, but with a different spring. From the given weight and velocity in these experiments, this formula, $P = 0.3756\,WV$ would make the pressure in No. 8 equal to 125.28, against 170.91 observed, and in No. 9 equal to 160.54, against 217.43 observed, results which show the formula to be useless for the spring used in 8 and 9. But the two observed pressures in 8 and 9 are very closely proportional to the velocities given in them; hence we can easily make a formula of the same sort, which would apply for this spring and weight. Thus if we suppose the pressure to be equal to the product of weight and velocity by a constant, the observations of No. 8 would make the value of the constant 0.512, and those of No. 9 would make it 0.509, the mean being, say, 0.511. Hence the formula, $P = 0.511\,WV$ would apply pretty closely to an unlimited number of experiments, made at various velocities, with the same spring and falling weight as those used in experiments 8 and 9.

But if a similar formula were made from the observations of experiment 1, we should get still another constant for that spring and weight, viz: 0.353; and from the observations of No. 2 yet another constant for that spring and weight, viz: 0.560; and lastly, for the spring and weight of Nos. 6 and 7 we should need a different constant from all these, viz: 0.917. Thus it appears that the supposed constant for such a formula is no constant, but varies with every change in the dynamometer used to stop the weight.

However, we have now seen that *with a given dynamometer* and *a given moving weight*, the maximum pressure of impact varies simply as the first power of the velocity. The reason for this is easily shown, if we consider the *work* required to stretch a given spring to a given tension. We have seen that this work is represented by $Q=\frac{Pd}{2}$. But, by the law of the spring, P is proportional to d, and if C represent a constant, we may write $P=Cd$, or $d=\frac{P}{C}$, which value for d, when substituted in the value of Q just given, produces $Q=\frac{P^2}{2C}$; that is, the quantity of work required to stretch the spring to a given tension P, varies as the *square* of that tension. In order, therefore, to produce a *double* pressure on *a given dynamometer* by a given moving body, we must *quadruple* its power of performing work; which we know, from the principle of vis viva, will be done by simply *doubling* its velocity. Or in general we must make the velocity proportional to the pressure to be produced.

But if we were to double the velocity of the moving body, and let it be stopped by a different dynamometer, stiff enough to stop it in the *same distance* as before, we should of course find a quadruple pressure. If the distance d remains the same, the pressure must vary as the *square* of the velocity. If this distance is not taken account of, it would, however, be no more correct to say that the pressure varies as the square of the velocity, than as the velocity simply; for the pressures calculated by the formula $P=0.5003\,WV^2$ are not only widely at variance with the results of my experiments, but no two of the pressures calculated by that formula have a common ratio to the observed pressures. But the formula $P=\frac{WV^2}{32\frac{1}{6}d}$ applies with reasonable accuracy to all the experiments of the foregoing table.

We have next to inquire in what ratio the pressure changes if the dynamometer remains the same and the moving weight is changed. We are usually taught that the pressure of impact varies directly as the weight of the moving body. And the formula $P=\frac{WV^2}{32\frac{1}{6}d}$ shows that if V and d remain unchanged, P will vary in the same ratio as W varies. But if we do not change the dynamometer or the velocity of the moving body, an increase of W will necessarily increase d, hence P will not in this case vary in the same ratio as W. And we have seen that experiments 8 and 9 may be represented by the formula $P=0.511\,WV$; whereas Nos. 6 and 7, which were made with the same dynamometer but with a different moving weight, would need a different constant, requiring the formula to be $P=0.917\,WV$. If we were to apply the formula $P=0.511\,WV$ to No. 6, we should get $P=37.11$, against 66.66 observed; and the same formula applied to No. 7 would give $P=56.87$, against 102.28 observed; results very wide of the mark. What then is the true ratio of change of pressure, keeping a single dynamometer, but changing the weight of the moving body?

To answer this, suppose that a certain weight with a certain velocity produces a certain pressure. To produce a *double* pressure on the same spring by another moving weight having the same velocity as the first, we know from what precedes that we must perform *four* times the work; hence this moving weight must be *four* times as heavy as the first. And in general, the velocity at impact being constant, the maximum pressure on *a given dynamometer* will only vary as the *square root* of the moving weight, instead of as the weight.

It is wrong then to suppose that even with a single dynamometer, the pressures produced by different moving bodies may be found by multiplying the product of the weight and velocity by a constant. We may, however, as is now evident, properly assume that *with a given dynamometer* the pressure of impact will equal some constant multiplied by the product of the velocity and the *square root* of the weight. Making this assumption the observed pressure in the 8th experiment would give 3.602 as the value of the constant for that dynamometer, and No. 9 would give 3.576, the mean of the two being 3.589. We may now write a formula, $P=3.589\,V\sqrt{W}$, which will apply to various weights and various velocities, with the given dynamometer. Applying this new formula to experiments 6 and 7, made with the other weight, we find for No. 6,

$P=3.589\,V\sqrt{W}=67.9$, and for No. 7, $P=104.05$;

both results being very nearly the same as the observed, the slight excess being due to the fact of greater proportional unconsidered resistances in these two experiments than in 8 and 9.

We have thus shown that for a *given dynamometer*, yielding by the assumed law, we may write an equation of the form $P=C\,V\sqrt{W}$, which shall leave out of view the amount of yielding, and yet give the maximum pressure due to the impact of various weights at various velocities. But the value of C will change with every change in the stiffness of the dynamometer, and hence the formula is not of general application.

This discussion leads me to infer that the experiments from which Haswell deduced his formula must all have been made with a single dynamometer and a single weight; otherwise it would hardly seem possible that he could have obtained such a formula as he gives, with a single and invariable coefficient. A striking illustration of the increase of pressure by increasing the stiffness of the dynamometer is seen by comparing experiments 1 and 6 of my table. In 1, a fall of 21.28 inches gave an observed pressure of 55 ounces, while in 6, with a stiffer spring, the same weight, falling only 4.51 inches produced an observed pressure of 66 ounces.

I will add a brief account of some experiments made in connection with the foregoing, but under a different condition of the dynamometer. In the preceding experiments, the resistance met by the moving body at the instant of first striking the dynamometer was zero, and increased uniformly to the maximum. I wished to try the experiment of having the dynamometer already under a tension on being struck by the weight. I accordingly arranged some pins so as to hold the spring stretched to a certain tension, and then allowed the weight to fall and continue the extension of the spring until the blow was spent. For calculating the maximum pressure which should be produced by the fall of the weight under these conditions, a modification of the formula becomes necessary.

If P' represent the initial tension at which the spring is held, and P the tension after being extended through d additional units of distance, and Q the *work* necessary to produce that extension, the work will be equal to the *mean* tension multiplied by the distance, giving

$$Q=\frac{P+P'}{2}d\,;$$

whence $$P=\frac{2\,Q}{d}-P'$$

Substituting for Q its value, $\frac{WV^2}{64\frac{1}{3}}$ we get

$$P=\frac{WV^2}{32\frac{1}{6}d}-P' \quad . \quad . \quad (3)$$

The spring used in experiment 1 was taken, and was drawn down a certain distance and held by the pins as explained, giving a tension of $P'=14.9$ ounces. A total fall of the weight, (mean of ten trials), was 11.71 inches. The extension of the spring produced by the fall was $d=5.69$ inches. The value of W was 14.79 ounces, as before. These values in the above equation give for the maximum pressure of the impact under the condition described, $P=45.4$ ounces. By trial with steady weights, extending the spring to the same distance, the observed tension was 43.8 ounces, falling short 1.6 ounces, or $\frac{1}{28}$th of the calculated tension. A repetition of the experiment under similar conditions gave me a calculated value of $P=45.3$ ounces, and an observed value $P=44$ ounces; the error being 1.3 ounces, or $\frac{1}{34}$th of the calculated value.

These experiments show that if we know the laws of change governing the yielding of substances during impact, we can calculate closely the maximum pressure produced; otherwise we cannot be sure of the least approach to accuracy. In the experiment just described, if the maximum pressure were calculated under the supposition that it would be the same as if the weight were brought to rest in the observed distance by a spring stiff enough to do it without any initial tension, the value of P would be 60.3 ounces, against 43.8 ounces observed, the excess being more than a third of the latter. But by the proper modification of the formula, a fairly close approximation to the observed result was calculated.

THE FUTURE OF SANITARY SCIENCE—POLITICAL, MEDICAL AND SOCIAL.*

From "Nature."

I COULD have wished it had been in my power on the present occasion to produce one of those essays which appeal to the imagination while they prepare the mind for the reception of sanitary principles and practice. Such essays are tempting and, in their place, instructive. To-day I am bound on a voyage less pleasant, yet I hope not less useful.

There has recently been called into existence a new society under whose summons we now meet. The society has assumed to itself the expressive name of the Sanitary Institute of Great Britain. It starts as a voluntary effort by men and women who are willing and anxious to give effect to those teachings of sanitary science which the past half-century has revealed. It invites all who are concerned to utilize the knowledge that has been acquired in that time. It wishes to encourage new research. But it has for its most anxious care to render useful to mankind at large the accumulated store of knowledge which at this moment lies ready for so many grand purposes relating to health. It accepts as its object, work for health, health of all the human family.

Shall some one say the object is ambitious? Yea, we reply, it is confessedly ambitious. Shall some one say the means at command for the work to be attempted are weak? Even so. Life is short, art long. Yet the short yields the long, and but for the short the long could not be. It is out of these littlenesses of human effort that the greatnesses follow. Or, as Benjamin Rush very forcibly puts it, and simply as forcibly: "There are mites in science as well as in charity, and the ultimate results of each are often alike important and beneficial."

It is my fortune, good or bad, to have to preside over the council of this new society. Of the ability of those who form the council, and of their experience, I need not speak in detail, for their mames are familiar to the world. They represent, I may say, sanitary science in all its branches, and from them, working harmoniously together, good results must be expected.

It seems fitting therefore as we enter on our work to look forward to the future. It is a part at least of our duty to look towards the future with the view of seeing in what directions we may best proceed; what assistances we may have to call upon; and chiefly what great powers we may have to consult and propitiate.

The three great powers with which our society will have to treat are the political, the medical, the social. From each of these we shall expect constant assistance. To one or other of these, whatever we do, our work will be transmitted or transferred. They will bring it into practical form and effect, or they will reduce it to nothingness. We can suggest and set forth initiatives, and with that our functions are complete in each particular branch to which we address ourselves.

It is our special duty to keep this

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* An address delivered before the Sanitary Institute of Great Britain at the Royal Institution, on July 5, 1877, by Benjamin W. Richardson, M.D., LL.D., F.R.S.

special fact steadily in view and to limit our labors by it. It too often happens that young societies like young men are apt to believe that they can conduct national processes as easily as they can conceive them, and under this belief fail most signally with the best of attempts. I remember in my early career getting a lesson from one of our late well-known statesmen on this very point. I was explaining to him the efforts I had made in 1855 and the succeeding three years to establish a registration of the diseases of this kingdom, and I bewailed the hard experience which proved that the greater the scientific success of the effort the more impossible it became to carry it out. In fact, said I, in a pitiful strain, the success almost ruined me in mind, body, and estate. "Served you right," was the immediate reply, "Served you right. If individual men could carry out national projects where would be the nation?" The reply was hard as it was unanswerable, and from that time to this I have given up all thoughts of doing more than sowing seed in the field of literature and leaving it to the chance of fructification on that extensive soil; or in showing some mere model of experiment which, perchance, may grow into working form. And this, I think, is the whole natural scope of our Institute,—to sow the seed of sanitation; to think our plans of projects for working methods; to lend its many minds, as if they made up the mind of one man, for devising from the past the best for the present, and respectfully to declare our conclusions.

The directions in which we shall have to move, the lines on which we shall have to move, are, I repeat, chiefly three—the political, the medical, the social. The powers on these lines must be approached in every work of ours, however simple, however complicated it may be. I shall try, as the title of my discourse explains, to indicate certain points in which we are most likely to come in contact with these powers and the changes we may expect to work in and through them.

THE POLITICAL PART.

In this country political action has been varied in relation to sanitary improvements. Sometimes political necessity has crossed sanitary progress, as, for example, in the imposition of a tax on sunlight, on foods that are essential to life, and in the granting of licences for the sale of pernicious drinks. At other times, and by fits and starts, political action has been in aid of sanitary work. So far back as the reign of Edward the Third, 1361, a royal proclamation was made through Parliament for preventing the slaughter of cattle in the streets of London because of the pollution of the streets and the drains which arose from that cause. From that time under great emergencies other similar acts came into force. They rarely lasted very long. As the urgent necessity for their existence passed away, they were allowed to fall into abeyance, and no permanent machinery was kept in order for insuring their continued and effective action.

Let me not, however, in saying this, be understood as conveying any special charge of neglect against English legislation. It is just to state, as an historical fact most creditable to our national history, that our legislators have by a long precedence taken the lead in sanitary affairs over those of other nations. In 1802 the great sanitary act for regulating the labor of children in factories set the example from which much useful legislation has followed at home and abroad. In 1838 that great original sanitary scheme for the registration of the births and deaths of the kingdom was inaugurated, to become a collection of facts relating to life, and disease, and death, of which there is elsewhere no parallel. And, since the era of the Crimean campaign, so much legislation has been attempted bearing on health, I dare not attempt even to enumerate the titles of the different measures that have been introduced. At this moment there can be no doubt as to the sincerity of our governments, of whatever party they may be composed, for dealing with every subject relating to the public health in an efficient manner, and in as rapid a progression as the slow and sure mode of parliamentary procedure will permit. The subject indeed presses at this moment with so much force on the governing mind, that if there be any danger ahead it is the danger of too miraculous a draught of small enactments, to the exclusion of comprehensive measures which all who run may read.

In saying this it is necessary to guard myself against error of expression. By comparison with all the nations of the world beside, we have obtained legislative measures which are splendidly comprehensive. No other country in the world can present an approach to the Public Health Act of 1875. That Act, as far as it goes, is admirably constructed. Its constitution of sanitary authorities throughout the kingdom; the power it vests in those authorities to appoint learned medical officers of health; the provisions it makes for securing to each locality better sewerage, freedom from nuisances, improved water supply, regulation of cellar dwellings, governance over offensive trades, and removal of unsound foods; the provisions for prevention of spread of infection and for the erection of hospitals and mortuaries; and the provisions for the regulation of streets and highways, lighting of streets, establishment of pleasure grounds, and regulation of slaughter houses; these, as well as the general provisions for the carrying out of the Act, are most commendable as practical plans by the working of which the nation may be tempered into sanitary mould of thought and character.

In a word the Act of 1875 is an improvement of the first degree on all that has preceded it, and although much of it, by the necessities of the constitution of our country,—which recognises the domination of free will even in its age of ignorance,—of a permissive nature, the working of the Act must in a few years remove a great amount of disease from the land and prevent the invasion of diseases of an epidemic and spreading type.

Sanitation however admits of being studied from two distinct points of view, the legislative and the scientific. The legislator may say, and perhaps with justice, that the production of such a measure as the Act of which I now speak is as much as can be done. The man of science may say that this is childish talk, that much more requires to be done, and that after all that which has been done, though it be comparatively great, is practically imperfect and very little. Science in this respect is always in advance of legislation, and that is her true place,—the pioneer's place. I remember the time perfectly when every fragment of the Public Health Act of 1875 was in the hands of men of science solely, and was called a chimera, over which great lawgivers shook their wise heads and passed by.

At this moment the positions of science and legislation are relatively the same as they have ever been, and it is fair for us men of science now as in the past time to declare the way ahead for the law-maker. I shall proceed again, therefore, as I have often before, to indicate one or two new starts in sanitary legislation, not from the legislative but from the purely scientific point of view, uninfluenced by the many and vehement individual grievances and troubles which beset the path of the minister of state. In so doing I shall indicate also, by inference, what I think our society ought to support in the sanitary policy of the future.

In the first place, then, we ought to expect in the political progress of sanitation that there will be established in connection with the Government one central department in which every subject, directly and even indirectly, connected with the health of the people, will be considered. This department, it is to be hoped, will be under the control of a Cabinet Minister, and will supervise the sanitary work performed at present by the Local Government Board, the Registrar-General's department, the sanitary regulations of jails and reformatories, and all the duties now pertaining to the supervision of factories, in so far as the health of the employed is concerned: in fine, every sanitary work that can be weeded out of every other department of the state.

To such a central board or department a specific name is necessary. The name should be as distinct as that of the department for war, for the navy, for the exchequer, or for the post-office. The name, it is to be hoped, will be emphatically the Health Department, and the chief of it the Minister of Health.

It may be urged that substantially we are drifting into some such order as is here suggested. It may be urged that the Local Government Board is step by step assuming the duties assigned, as above, for the State Department of Health. To some extent this is true, and it might be advisable, for the sake of the connection which must always exist between such a central board and the various local boards in the kingdom, to add to the name of Health Department that of Local Government Board. But for the sanitary object the leading name must be Health, and Local Government must come in merely as indicative of the connections that exist between the State and the local centres—as the machinery.

In this question of progress there is involved an immense deal in a name. It is essential to the scientific sanitary teacher that every reasoning mind in the kingdom should become familiar with the two significant words, public health, or national health. It is equally necessary to let the people know fully that the Government has the health of the country under its general and wise supervision. But it is utterly impossible to make either of these facts understood by the masses so long as any sanitary authority, central or local, has a title which fails to convey the meaning of its functions. To speak to the masses who are listening to a lecture or discourse on health about a local government board is only to confuse them. They ask you afterwards what it all means, and they go away imbued with the impression that it means anything except what relates to the health of the people.

I am speaking very practically in suggesting, that in the course of political sanitary progress it is an absolute necessity for success to give its proper and only name to the department of state which presides over the national health. I do not state too much in declaring that every public measure would carry more weight if it went forth as being under the supervision of the health department. It may appear a refinement of illustration, and yet it is a sound argument that vaccination wonld have met and would meet with far less opposition if it were enforced under the general supervision of a State department of health. As it is the people connect the carrying out of vaccination with something other than health, and even as distinct from the idea of conservation of health. It is looked upon as a legal tyranny, having no scientific setting forth of its intention, and as springing from no scientific authority. If you attempt to reason with its active opponents on the subject, and refer to the authority that exists, they dispute the competency of the authority in name and form; and, foolish as the objection may be, it is potent for obstruction.

In making this suggestion there is no necessity to offer a word against the continued action of local self-government. The work of the local centers in all parts of the kingdom instead of being in any degree curtailed and restrained, should be encouraged and maintained. In the sanitary local work the word health should, however, again come forward as the one prominent designating term to which all others should be subject.

Our Sanitary Institute could not turn its attention to any more suitable labor than that of inculcating the necessity for the institution of one state department exclusively devoted to the health of the people. In the success attending such an effort a double result would be achieved. The country would have secured for it the best and most direct guidance on its most vital interest, and scope would be given to the industry of men of science in a new direction. Men, whose lives have been devoted to the study of life and health, would be prepared by their devotion for the accepted service of their country in public form, and the Houses of Parliament would become, at last, congenial spheres for their labors. The Houses would be strengthened by such adhesions; the men would be more useful and honored.

Another work in the political line which will be demanded in the future for the benefit of the sanitary cause is the preparation of such a digest of all our practical sanitary laws that every person of intelligence can read and understand what may be legally enforced for the maintenance of health. What may be done in this direction ought to be so simple and so plain as to be brought into a school-book. Not a line should be left for the subtlety of the legal brain to twist into contortioned illegibility. The laws by which the health of a man, and thereby of a nation, can be preserved to the utmost, are so simple in nature that nothing but the utmost simplicity can truly express them, and the whole labor of the future, if it is to be of any service whatever, must be directed to the discovery and establishment of such simplicity of exposition and direction. Up to the present time much that has been done has been provoked by that most untrustworthy of all human provocatives to action,—fear. Some great epidemic has occurred that has caused universal dismay; some great catastrophe has occurred, like that of the Crimean campaign, which has excited universal criticism on the failure of sanitary provisions by the authorities of the nation. Some such slip has been permitted in sanitary rule as that which recently let scurvy undermine the workers during a great enterprise of discovery. Straightway on the heels of such events there have been commissions of inquiry, and as a direct or indirect result there has often come forth some particular enactment. Or—and this is by no means rare—some individual of the House of Commons, impressed with the danger of a great national evil, has pressed for a national remedy, and, by steady persistance session after session, and by showing that he never knows when he is beaten, has forced the Government to take up his measure and to carry it through.

From these modes of legislating for health we have obtained many minor acts which fill and refill the national statute books. And still this process promises to go on, a process of labor in a circle with much loss of time and expenditure of force without ultimate progression.

It would be vain to find fault with the past for its doings. As vain to find fault with the State for meeting State disorders by empirical remedies as it would be to find fault with the physicians of a former day for the same mode of procedure. If the people demand a recipe they must have it, be it from the State or the family physician. The question that now comes forward is whether the time has not arrived for ceasing to treat the health of the nation by specific or supposed specific remedies for particular errors, and whether we may not find in the future a few very simple and natural guiding principles on which all acts of Parliament relating to the health of the people may be based ?

Before this effort can be attempted the existing acts that touch on health,—public health acts, metropolitan health acts, contagious diseases acts, vaccination acts, factory acts, acts relating to the importation of cattle, adulteration acts, and others relating to prisons, work-houses, and the like, and which, if they even lie latent are not repealed,—these, one and all require to be considered together, with the view of determining whether an English or even a British act of settlement for the vital regeneration of the realm is not practicable on a simple natural basis of natural requirement.

I am fully aware that this suggestion carries with it the idea of a gigantic labor; but it will have to be done, and once fairly tackled I dare say the apparent difficulties will readily dissolve away. It is a mere question between doubting and attempting : and we all know and feel that—

> "Our doubts are traitors,
> And make us lose the good we oft might win
> By fearing to attempt."

Supposing the existence of an efficient central department of health acting under the direction of a minister of health, a grand new duty, as it seems to me, would be to determine what is the evil or what are the evils that have to be removed in order that the cleanest bills of health may be regularly presented to the nation. Without such preliminary knowledge all sanitary work is unsound to the last degree. It were as wise for me to write a prescription for a man without inquiring into his disease, his antecedents, and modes of life, as for the State physician to prescribe for national sickness without inquiry into the nature of the sickness, its antecedents, and the cause or causes that led up to it. The great work, therefore, and indeed the first sanitary work of the future, standing before all other sanitary legislations except the formation of the central authority, is, the systematic enumeration, week by week, of the diseases of the kingdom, through the length and breadth of the kingdom. It is utterly hopeless to attempt any decisive measure for lessening the mortality, which is certainly more than double what it ought to be, until this State labor is faithfully carried out. It is vain, comparatively speaking, to know what totality disease hands over to death, unless we know also what health under one or other cause of disturbance yields over to disease. Physicians and statists strain their eyes to try to get at the extent of disease. Laborious geographers like Mr. Haviland spend years in constructing maps from the tables of mortality, in order to get a mere approximation of the distribution of disease in England: and meanwhile disease itself, constantly cheating the observers, is making its way without being under any systematized recorded observation.

For the omission of a registration of disease there is no conceivable excuse. The thing has only to be done. The organization of the Registrar General's department has fully opened the way to the collection and the utilization of the facts relating to birth and death. These elements swing in the statist's balance readily, and are weighed by our consummate state weigher of life and death, Dr. Farr, as accurately as the Chancellor of the Exchequer balances the national ledger. With equal readiness Dr. Farr, if the data were collected for him, could tell from week to week the health as well as the mortality of the kingdom. In a short time, under such regular record, the whole nation would know the reigning health, the reigning disease, of every center of life. And if, as might easily be done, the diseases of the lower animals and the diseases of the vegetable kingdom were included in the returns, all the facts of disease would be completely rendered.

I think I have already referred to an effort I made many years ago to carry out this design of registering the diseases of the kingdom. I refer to that effort again for a simple reason,—for the purpose of indicating that there is really no greater difficulty in getting the facts than there is in utilizing them. I attempted no more than the registration of the epidemic diseases, and I could afford no more than the publication of a quarterly abstract of the data that were forwarded. But in a short time fifty medical observers were sending in returns from as many stations, extending from St. Mary's, Scilly, to Lerwick, in the Shetland Islands. These stations could easily have been increased to any extent, and the amount of information regularly communicated was indeed most valuable.

Two facts connected with this attempt are perhaps worthy of note, one as showing something determined, and the other as showing something suggested. In the returns sent from the district of Canterbury in the spring quarter of the year 1857 was included the first account of the invasion of this country, at least in any known time, by the disease since then so prevailing and fatal, diphtheria. This disease first appeared in the little village of Ash, and was called the Ash fever. The outbreak was observed and recognized by Mr. Reid, of Canterbury, and was reported to my register by Mr. Haffenden, who collected for me the facts of prevailing diseases from eight medical observers living near to him, of whom Mr. Reid was one. The first facts of a new disease in this country were thus recorded on the spot, which is something even as a matter of history. How such a fact, reported at once to a central government authority, might be dealt with; how promptly a central authority so advised might act in arresting a fatal epidemic at its origin, and what national service might be rendered thereby, you, quite as well as I, can judge!

The fact of a suggestive nature springing from the working of the returns is not less interesting. The labor led me to refer to the returns of sickness sent every week by the medical officers of the Poor-Law districts to their boards of guardians. I found that these returns, over 3,000 in number, which, when they have served their local purpose, are practically worthless, could by the slightest modification be utilized as returns of the sickness of all the sick parochial population under official medical care, and I submitted a plan for such introduction to public approval and to the Government, but without effect. Yet if the plan had been adopted from those three thousand weekly returns, cast away and still cast away, I calculate that 156,000 tables of disease per year would have been submitted to scientific analysis which, since the time when the suggestion was first made, would have multiplied into 3,276,000 tables, including in each table a record of at least ten times as many particular examples of disease. To what important national uses such an array of facts systematically arranged and examined could have been applied you, as well as I, can judge! And still neither of us can judge effectively, because in dealing with data taken from nature there is always something important to be elicited which never was looked for, and often, too, that something unlooked for is better than that which was specially looked for.

Our Sanitary Institute will do well in continuing to press this scheme for the registration of disease on the Government, and it may greatly assist the work by lending its mind to the best means of collecting the facts on which the weekly reports of disease will have to be based. I might enlarge on this part of my subject, but I should prefer to remain silent until the views of the medical officers of health, now a large and influential class, have been correctly ascertained. My present purpose is served if I have sufficiently directed public attention to the principles of the design.

In the future of sanitary science the politician must come forward more determinately than he has yet done, in order to secure for those over whom he governs three requisites—pure water, pure food, pure air.

The Public Health Act of 1875 deals with the water question, and makes provisions for the local authorities to supply their respective districts, by means of a company, or by independent action. For my part I see no hope of any effective change for the better by these propositions. It is utterly hopeless to trust to companies in a matter of such vital moment. It is equally hopeless to trust to the undirected action of local authorities. If we trusted to such agencies for the collection and delivery of letters by post, does any one suppose that the results of our present postage system would be attained? Yet important as intercommunication by letter is, it is less important than the supply in due quantity and pure quality of that vital fluid which makes up three parts out of four of every human organism, and which is wanted as much by the millions who never receive a letter, as by the millions who do. In this political part of sanitation, the Government must do one of two things. It must either produce a process or processes for pure water supply, and insist on every local authority carrying out the proper method; or it must,—and this would be far better,—take the whole matter into its own hands, so that under its supreme direction every living center should, without fail, receive the first necessity of healthy life in the condition fitted for the necessities of all who live.

By recent legislation we have some security for obtaining fresh animal food, and foods freed of foreign substances or adulterations. The penalties that may be inflicted on those who sell decomposing, diseased, or adulterated foods are beginning to have effect, and much good is resulting. Nevertheless, even here the legal rule falls short of completeness. The inspection of animal food is as yet most unsystematic and imperfect. With all our richness of means ready at command, we have not approached that admirable system for the inspection of animal food which our Jewish brethren, through ages of ignorance and oppression, have managed so efficiently to carry out, and which has entirely saved them from many of the great calamities of disease that have fallen on less careful people. The complete inspection of animal foods, including milk, is a clear piece of sanitary law which, from day to day and hour to hour, must ultimately be enforced.

Imperfect as legislation may be in respect to supply of pure water and food, it is advanced in these directions when the steps it has taken for supplying pure air are brought under observation. There is no practical legislation of any kind on this requisite. The air of our large towns is charged with smoke and impurity. The air of our great factories is charged with dusts which destroy life with the precision of a deadly aim. Dr. Purdon, one of the certifying surgeons under the Factory Acts, reports that in the flax-working factories under his care the carders, who are all females, if they get a carding-machine at eighteen years, generally die at thirty years. Can any fact be more terrible than such a fact, that a girl of eighteen should have to live by an occupation that will bring her existence to an end in fourteen years, and to that end with all the prolonged wasting, sleeplessness, suffering, incident to the disease—consumption of the lungs. If it were the fate of these doomed workers that at the close of fourteen years' work the majority of them were taken forth and shot dead in an instant, their fate were infinitely better than it is. The heart of the nation would thus be roused, and the law in all its majesty would be put in operation to arrest the progress of the crime and to punish the offenders. Yet, year after year as effective an offence goes on, and because the results of it is hidden in the sick-room there is no arrest of its progress, no punishment for its commission.

In the application of political science to preservation of health not one subject presses more earnestly than the question of the supply of a pure atmosphere to the millions of industrials of these islands. In an inquiry I recently undertook on this matter for the Society of Arts, Manufactures, and Commerce, the facts that came before me were as of a new world. You will find a compact mass of these facts in the lectures I had the honor to deliver before that learned society. Those lectures contain a tithe only of the things seen. I am quite sure that our leading politicians can have no adequate conception of the mental and physical condition of the great industrial classes, or of the need that exists for reconciling those classes to their fate. These truths are plain.

The catechism has failed to satisfy them. Bad air keeps up in them a depraved mental as well as physical state. Their poverty and not their will consents to their condition. In short, as a physician dealing with the physiological and psychological phenomena belonging to a class instead of an individual,—and this is all the difference there is between a politician and a physician,—my diagnosis is that a serious organic state, febrile, fitful, fatal, exists in this part of the nation; that it demands the watchful consideration of all physicians, State and ordinary; and that the sooner the natural cure for it, pure air, and plenty of it, is let in the better for every class everywhere.

All political troubles have a physiological cause. To the Statesman not less than to the physician, physiology is the only true source of knowledge. A society such as ours, therefore, possessing as it does professed physiological skill, may render most important service by tracing out for the legislator the simplest scientific means for removing with atmospheric impurities and by preparing for that sanitary future when men universally shall breath purity even with their freedom.

If any other incentive to action in this direction were required it would be the further fact that all diseases, mental and physical, national and individual, begotten of an impure atmosphere, are transmitted on. The consumption of body, the restlessness of mind are reproduced and gain intensity of development with each generation until practically they inaugurate a distinct racial type of human imperfectedness.

With this topic of legislating for pure air would come in naturally the question of homes for the people and the development of those recent acts which have been passed to meet the necessity. These efforts of the world political can scarcely be over-estimated; but there is one movement which stands before them and which has been singularly overlooked. It is essential that the home of the working man should in every case be cleared of the details of daily work. So long as he is compelled to work in the room in which he sleeps and takes his food, so long his home must be an unhealthy centre, and too often it will be the centre from which infected work will pass out, bearing infection into the homes of the wealthy. A modification of factory legislation by which a free and properly regulated work-room should be within the easy reach of every working man in every crowded centre is a necessity which all sanitary laborers should strive to get supplied. Our Institute has another urgent task before it in the effort to enforce this necessity on public attention.

In the future of sanitary science one more amongst many other reforms of a political character must needs claim important consideration. I refer to the political assistance that must be given to all of us who are engaged in the labor of quenching the drunkenness of our land. Our best sanitary efforts will fall far short of their deserts until this object shall be achieved. Over the future of sanitary science will be suspended a pall of sorrow until this object shall be achieved. Does any one desire to know how the mortality of the kingdom is modified by strong drink, let him read the knowledge in the State record book which tells that those who sell the destroyer die by it at the rate of one hundred and thirty-eight to the hundred of the whole population. Then, starting from this signal fact, let him trace the influence of the destroyer through all the courses of diseases which, under learnedly obscure names, spring from it and kill from it in all classes of society. Finally, let him reckon up the hereditary evils which are engendered by the same destroyer and the influence of that on the course of disease, and his lesson will be in some measure complete.

I do not think this the occasion to discuss the value of the different political sanitary measures that have been, or are at this time, in the public mind for the repression of the national evil now touched upon. Be it sufficient for me to state two impressions only. Firstly, that every day's experience of the question in various communities where as a teacher of abstaining temperance I am wont to labor, indicates to me that unless the State does come to the aid of the teacher the battle against intemperance must be indefinitely prolonged. Secondly, that if the State itself—doing nothing active in the way of repression, would but determine to cease to legalise the cause of the evil and to make revenue out of the transaction, the labor of the temperance reformer would have the most prosperous season of success presented to his view. Hitherto this has not been considered as a sanitary question. In the future no sanitary student will venture to exclude it from his studies.

The contemplation of the political sanitary future of this kingdom offers many other topics, all of which I must leave in order to devote a few minutes to our subject in its relation to medical science.

THE MEDICAL PART.

The influence which sanitation will exert in the future over the science and art of medicine promises to be momentous. It promises nothing less than the development of a new era; nor is it at all wide of the mark to say that such new era has fairly commenced. The greatest of the world's philosophers, the philosopher whose thoughts cover the world of science as with a garment, I mean Lord Bacon, said of the medicine of his day, that it stood for judgment on quite different merits than did other learned pursuits. "Other arts and sciences," he argued, "are judged of by the power and ability exhibited in the conduct of them by their professors, and not by success or by events. The lawyer is judged by the skill of his pleading, not by the issue of the trial; the pilot by his skill in directing the course of the ship, not by the fortune of the voyage. But the physician can perform no particular act by which his ability can be directly demonstrated, and therefore he is principally judged by the event, which is very unjust. For who shall decide, if a patient die or recover, whether the good or the evil is brought about by art or by accident? Whence," says he, "imposture is frequently extolled, and virtue decried. Nay the weakness and credulity of men is such, that they often prefer a mounte bank or a cunning woman to a learned physician. So the ancients made Esculapius and Circe brother and sister, and both children of Apollo. Hence," he adds, "physicians say to themselves in the words of Solomon, 'If it befall to me as befalleth fools, why should I labor to become more wise?' And therefore one cannot wonder that they commonly study some other art or science more than their profession, because they find that mediocrity and excellency in their own art makes no difference in profit or reputation; for man's impatience of diseases, the solicitude of friends, the sweetness of life, and the inducements of hope, make them depend upon physicians with all their defects."

Had Bacon spoken these sayings in the present day, he had spoken, with one or two exceptional errors, as truthfully as he spoke in his own time. Had he been a physician, he might indeed have gone further than he did. He might have urged his too frequent inadequacy himself to decide whether his own success rested, in particular instances, on skill or on accident. He might further have added how oftentimes the cheek of the right-minded physician pales or burns with doubt as he hears his own praises declared for skill which he himself cannot for a moment take credit to his own heart. This has been the fate of medicine until our day. On such fate all the quackeries have flourished; on it all the "pathies" and dogmatic systems of medicine have flourished; on it the idea of cure has found too willing acceptance and belief.

At last a change has come over the science of medicine. With true nobleness of purpose, true medicine has been the first to strip herself of all mere pretences to cure, and has stood boldly forward to declare as a higher philosophy the prevention of disease. The doctrine of absolute faith in the principle of prevention indicates the existence of a high order of thought, of broad views on life and health, of diseases and their external origins, of death and its correct place in nature. The doctrine of absolute faith in curative medicine, of power vested in the hands of a distinct sect or class, and exercised by them as by regal right and without the assistance or interference of those upon whom it is exercised, indicates a low standard of knowledge; a too confiding spirit is the wisdom of a minority; a departure too wide from the safe law of self-preservation; and an ignorance of the avoidable causes of diseases; a blindness and therefore an unnecessary exposure to danger; an overweening and sudden fear of dangers of all kinds little and great, and a hasty and thoughtless pursuit after that mode of rescue from dangers of disease which claims for itself the greatest pretensions and boasts the greatest successes.

It shall remain as one of the glories of medicine that she herself has first seen these truths, and, willing to sacrifice her own interests to truth and light, has put them forward without fear, without reward. In the science of prevention medicine takes in fact all the world with her. The science becomes a political, a social, as well as a medical study. It appeals to every mind. When it once is so set forth it fills all men with its teachings. It models itself into household truths and commingles with the moral and even religious elements of life. Admitted for a season into the household, it steps forth again to find its way into the legislature. It becomes eventually a governing science—a law.

This scientific course commenced, must needs go on. But in its going it must needs also change greatly the old face of medicine, and remove in the change the Baconian reproach. I do not think there is much difficulty in foreseeing what in the main the change will be like.

I need not say that the "pathies" will go. The pathies of all kinds are as dead as door-nails, and wait only to be decently interred in a common grave. In time the word cure will go altogether. It is clear already that there is indeed no such thing. A man born to live through a given cycle lives through it free of disease, unless he be stricken from without. If he be stricken, and by the stroke the natural functions, by the exercise of which he lives, are not so disturbed but that they can swing back again in due order, he may recover; if he be stricken beyond this, he will die. Nature will pursue her course undisturbed by either event. She will make no special effort to kill, and assuredly she will put out no special hand to save. A man may intervene, and may, by knowledge, put the stricken body into such a condition that it may swing back into natural course whereby he will have put it into a condition in which it will not die. This is the very highest development of medical art resting on science. But it is not cure, in the common meaning of that term.

By the progress of sanitary science and by its influence on practical medicine we shall attain these perfect rules of management after the infliction of the stroke of disease; and I do not doubt that the art of placing the stricken under such conditions that they may not die will for ever afford scope for the inventive genius of man. The more immediate triumphs will, however, come in that part of the work which is purely preventive. Down from the skies comes the forked lightning and lays a man prostrate. It is a question for the ages who shall place that man in a condition under which he shall certainly swing back again into life. But the preventive art that puts up a metallic rod to divert the lightning from other men, that is the present triumph of human skill; skill which, carried to perfection, shall prevent the stroke and put out the second art by removing the necessity for its application.

With the progress of sanitary science we must expect to see preventive medicine taking the ascendancy. Cure will cease, prevention will grow. Humanly-made epidemics, like the great plague of London, which was planted and reared in the rush-covered floors of domiciles saturated with the organic refuse of years, or like the modern typhoid, which is fed by streams of drinking water uncleansed from human excreta, such self-made epidemics will be prevented by simple mechanical skill. Diseases imposed by indulgence in harmful pleasures and appetites, or by physical overwork and shock, will be removed by the effect of moral influences and knowledge of cause; and gradually, I believe, those persistent evils, which, like the lightning-stroke, come without human ordinance or fault, will be placed also under some protecting care, and, if not removed, reduced to a short calendar.

It is felt by some that the medical Sanitarian of the future will have his best efforts thwarted by the forcible excess of life beyond the means that can be found for the support of life, as if life were a mere secondary principle in the universal order. I see no such cause for fear. That in the progress of life on the earth the day will ever come when the earth will not supply food for its people is to my mind pessimism carried into an insane vulgarity. It is clear that man can always reduce to his wants the lives of all animals except man. The question rests therefore on the abnormal increase of man alone. Nature knows that and rules accordingly. Let man remain savage, and, however sensual he may be, he will die fast enough by war, plague, famine, or luxury. In that state he will never overstock the earth, but either grope in solitary places a neglected family, unprotected from all the killing vicissitudes, or will sink into luxurious barbaric decadence. Let man become exalted in life; exalted by communion with noble pursuits; with pursuits of science, art, letters, and cultivation of greatest happiness for the greatest number, and his sensual life will become too subject to the virtue to leave a chance for the danger which a low sensuality sets up as a terror and at the same time a temptation for the vulgar.

I think it my duty to deal plainly with a question which affects so closely the future of sanitation, and to express, from an experience which is confirmed, as I know, by some of the brightest ornaments of my learned profession, that nothing is wanted to correct the danger of over-population but improvement of mental process; nearer communion with the eternal mind in His works; purer artistic education, healthier homes, more rational amusements, and the ennobling influence of a holier life amongst those who assume to be the cynosures of the nation.

On the whole the prospects of medical learning and action will be greatly improved by sanitary advancement. It is possible that fortunes or reputations resting on faith in famous curers will dwindle slowly away, and that not for long will the skill of the physician be valued by the fallacious reckoning of mere results. But in exchange there will be opened to the physician a career in which skill of labor will be exhibited together with results, the results obvious as to their relation to the work, and both, if good, successful beyond praise.

THE SOCIAL PART.

The future of sanitary science in relation to social life generally, its effects that is to say on all classes of the community, promises steady progress. No one who has been actively engaged for the past quarter of a century in sanitary work can doubt this statement. Throughout all sections of the community there is desire to know; and if the legislator will be content not to legislate until he sees that free-will guided by knowledge is in the same train with him—it doesn't matter in which class,—all will go well. The workers in our Sanitary Institute though they be not legislators can, nevertheless, greatly assist Parliament by bringing free-will into harmony with knowledge, and though the distinction does not at first sight stand out, in separating free-will from ignorance and from those automatic demonstrations of ignorance which are the outward and visible signs of unhealthy habits of life.

The social work that has to be carried out for the future of sanitary science is purely educational. Educational not merely by lectures and books and lessons from books, but by demonstrations of sanitary works, plans, buildings, mechanisms, results of all labors bestowed on the cause. Without venturing on details of this kind which would land me in another address, I may be content to touch on two points, both of vital moment for the future.

The first of these relates to modes of teaching so as to carry the sympathies of the learner and his more refined tastes along with his reason; to attract and charm his senses as well as his intellect. It is said of us sanitarians, and sometimes I fear with some truth, that we would make health hideous. We need not do so; and if the feat has ever been accomplished it is but the work of a "prentice han," that ought to be forgiven. Health truly is beauty in the living evidences of it, and should be so in those inanimate evidences which the builder and the engineer construct for us. I would therefore urge that in all coming sanitary work, theoretical or practical, the sanitarian should call the artist also to his side, and that no design of a sanitary kind should ever be executed in which the hand of the artist does not play its beautifying part.

And if I might suggest so much to the imaginative scholars who live to make life sweeter to the many, I would ask them,—poets, painters, sculptors, players, musicians,—to believe that to render practical even their refined labor is to render that labor more acceptable, more diffusible, more durable.

The second topic relates to those who require first to to be taught the sanitary lessons of the future. I want strongly to enforce that it is the section of the nation which Dr. Farr classes as the domestic, the six million of women of the nation, on whom full sanitary light requires first to fall. Health in the home is health everywhere. Elsewhere it has no abiding place.

I have been brought indeed by experience to the conclusion that the whole future progress of the sanitary movement rests for permanent and executive support on the women of the country. When as a physician I enter a house where there is a contagious disease, I am, of course, primarily impressed by the type of the disease and the age, strength, and condition of the sick person. From the observations made on these points I form a judgment of the possible course and termination of the disease, and at one time I should have thought such observations sufficient. Now I know them to be but partly sufficient. A glance at the appointments, and arrangements, and managements of the house is now necessary to make perfect the judgment. By this is shown what aid the physician may expect in keeping the sick in a condition most favorable for escape from death; and by this is also shown what are the chances that the affection will be confined to one sufferer or distributed to many. As a rule to which there are the rarest exceptions, the character of the judgment is hereupon dependent on the character of the presiding genius of the home,—on the woman who rules over that small domain. The men of the house come and go; know little of the ins and outs of anything domestic; are guided by what they are told, and are practically of no assistance whatever. The women are conversant with every nook of the dwelling, from basement to roof, and on their knowledge, wisdom, patience, and skill, the physician rests his hopes. How important, then, how vital that they shall learn as a part of their earliest duties, the choicest sanitary code. How correct the decision of the founders of the Sanitary Institute, that from the first they should include sanitarians of both sexes as working associates.

To women more than to men this work is new. To women more than to men this work is hard to realize. Naturally more conservative than men they are moved with less haste to tasks of reformation and reconstruction. More sensitive to criticism than men, they are given, at first, to resent, as if it were an insult to past customs and usages to which they are attached, the suggestion of innovation. But these passing difficulties removed, there is in the hearts of women such matchless generosity, such overpowering love for every device tending to promote the happiness of all things of life, that we sanitarians may indeed be content for the future of sanitary science in its social aspects, if we do no more than win them to our own cause and entrust its details to their ministering spell.

THE LONDON OBELISK AND EGYPTIAN WORK.

From "The Builder."

Great London, it is certain, must in time be, if it is not now, as remarkable for the variety of art and architecture it possesses, as it is for its hugeness and extent, and the multitudes who inhabit it. It will in time, too, be as remarkable for the specimens it has every here and there of art foreign to itself, and its ancient remains. These are, indeed, already, of all times and phases of art, and we are, as all know, to have, in no longer time, another not a little remarkable specimen of antique art, and to be placed somewhere,—an Egyptian *obelisk*, Cleopatra's Needle, as it has been somewhat oddly called,—a remarkable monument of the past of things in this world, and indeed a history in itself. It may be, therefore, of some interest to hint at a few things to which this strange monument affords a sort of key; for could all things be told about it, and its origin, and its purposes in the place from which it comes, and all be brought to light, what a strange tale it would tell, and how deep into the old world's history would it lead. We can but hint here at a few things, and but glance at this monument, and read, may be, a little of the inscription on it, and of the things about it. In the first place, it is to be borne in mind that this solitary monolithic shaft is not complete in itself, and as standing alone, as it will do when it forms one of the many wonders of London; but it, like so many others in that strange land, Egypt, from which it comes, is but a part of a great system or whole. It was an item in a temple, or pile of buildings, devoted to the special worship of some pagan deity. Obelisks stood in pairs opposite each other, and must have formed *in situ* objects sufficiently remarkable and significant.

It would be difficult, without a somewhat elaborate plan, to make the position of the Egyptian obelisks quite plain. The temples, including the court-yard in which the obelisks stood, took up a large space of ground. The temples differed, according to their size and situation, in details and arrangement, but the main idea was the same, and in this main idea the most notable, perhaps, was the provision that was made for those long and and imposing religious *processions* in which the men of Egypt took such delight, and which indeed went so far to make up their very religion itself. The successive gateways, the very walls of the temple, the long line of recumbent animal forms, the obelisks, the seated colossi, the pylons or gateways within the temple court or enclosure, the arcades, and the covered or roofed enclosure itself, in which the statue of the god was enshrined,—all seemed to be devised with the one object of giving effect and meaning to those imposing processions and shows. No small impulse, too, to the fine art of Egypt must have come from the sight of those world-notable displays.

All this went to make up the general plan and idea of the Egyptian temple, and all certainly had its material or symbolic use. Ornament, in one sense, and as mere ornament, there was none, for the whole temple and pile of buildings were simply written upon, both within and without, as on its four sides is this very obelisk. Even the capitals of the columns would appear to have been wrought out with a symbolic meaning, and the hieroglyphics to be seen everywhere, and on every otherwise vacant stone or granite surface, were there to be read by all who could read them. In short, an Egyptian temple was an open book, and in passing through it you could but read what was written on its walls and ponderous blocks of stone or granite.

We have said that by going into details some very considerable space might be taken up, and it is difficult to know what best to select, there being so much that is noteworthy. But perhaps the most striking thing to be noted first in Egyptian undertakings was the enormous size and consequent weight of the blocks of stone which they quarried, and moved from place to place, and lifted into such all but impossible places, and all, too, with such truth and accuracy of fitting and fixing. Sir G. Wilkinson notes not only the immense size of the quarried blocks, but the confined and awkward places from which it was necessary at times to move them. The obelisk, to cite but a single example, transported from the quarries to Thebes, measured no less than 90 ft. in length of one single stone. Nothing seems to have broken the heart, or the "lifting" courage and patient strength of the old Egyptian. Sir Gardner calculated the weight of one single block at Karnak,—the "mighty" Karnak, as well called,—at no less than 297 tons; and this had to be moved to no less a distance than,—incredible as it seems,—138 miles! And even this, great as it is, is by no means the largest or the weightiest of the ponderous masses which the old Egyptian mechanics and workmen contrived to move and lift into place by mere force of multiplied manual labor.

But, perhaps, even more surprising than the lifting and moving of these ponderous blocks are the wonderful artistic precision and accuracy with which they have been placed and fitted together. There are no broken joints, edges, or corners, and the lines are as straight and as true as a straight-edge. We are here noting the simple masons' work only, for the practical difficulties to be encountered and mastered here, though the work may seem to be simple, can hardly be overestimated. Doubtless this obelisk, which is to add to the curiosities of London, will evidence a little of this. Not far from the banks of the river, on which it will be floated here, can it be taken, say the authorities; but, wherever it may be, its workmanship, as workmanship, may be then compared either with that of the granite-work of the Embankment or with any other masons' works, near it. Much more in detail might be noted on the subject of this work of the old Egyptian mason; but there is in the Egyptian sculpture and deep-cut carving room perhaps for even greater wonder at the skill displayed, and at the accuracy of the eye and hand. Obelisks are covered on all sides with hieroglyphics, and the cutting of these was among the many other masterly things which the Egyptian workmen of old could do so well. In the obelisk that is coming here we shall find evidence of this skill; but even without it, this may be seen in the British Museum, where are a number of examples of it, admirable alike for executive skill and inventive and imitative power in rendering of animal and other forms. Indeed, there may be found marvels of executive skill. The hardest black marble has been incised with a truth and finish not to be surpassed. No one, says a practical judge of such matters, who has ever tried to perforate or cut into a block of Egyptian granite will scruple to acknowledge that our best steel tools are turned in a very short time, and require to be retempered. This the French engineers found when the Luxor obelisk was moved from Thebes; and even, it is added, with the best of modern tools, we find considerable difficulty in doing what to the Egyptians would have been one of the least of their arduous tasks. The more this work of antique Egypt is looked at and attentively studied, the more will it become evident how much there is to be learnt from the study of Egyptian monuments.

Whereabouts in London this fine work of Egypt's artists is to be placed is at present uncertain, and how and in what manner it is to be treated is equally so; but let us hope that no attempt will be made to tamper with it in any way, or to do otherwise than place it as obelisks are usually placed when *in situ*, so that nothing may be visible to disturb the impression which such an art work is calculated to make on the thoughtful student and observer. We deprecate, as others have done, the plan sometimes adopted of adding to the pyramidal top of an obelisk, as the French have done at times, and as the old Romans did, and thus destroying that harmony of lines and simplicity of form which make of the obelisk what it is, and what the Egyptian meant it to be. Its beauty lies in the very simplicity of its outline.

Among many other points to be noted, and for which we have not space, there is one out of which some good ought to come, and it is most instructive. It is the admirable way in which Egyptian artists contrived to treat the surface of their hard granite. It is admirably polished, yet is the color of it not in any way destroyed or marred; and though so polished and smooth, it does not reflect light, like an imperfect mirror. In proof of this the colossal heads in the British Museum galleries should be looked at and studied, and then compared with reference to the injudicious way,—as we take it to be,—in which the granite bases or pedestals, on which they stand, have been treated. These, all of bright red granite, are polished like looking-glasses, and quite kill the heads above them by the violence of the contrast. The best way to polish granite is a problem. We trust that in any attempt (if there be one) to repolish the obelisk, this useful and patent fact may be taken note of, for the incised hieroglyphic all but disappears if care be not taken. One more thought, out of many, and we commend it to engineers. It will depend a good deal on the position in London town of this huge monolith as to how it looks, and what impression it makes, and the Thames Embankment has been suggested as the most appropriate of spots for it, looking at the immense difficulty, if not impossibility, of moving the granite block far from the river. Surely the moving of it ought not to appal modern engineering! It may be a question, indeed, as to what site in London is the best for such an isolated and foreign fragment of art as this, but it may usefully be borne in mind that a very great deal of the impressiveness of the obelisk, when on its own native ground, depends on its close proximity to a building, which it thus emphasises and helps to make complete. In a court-yard would seem to be its appropriate place, and in front of the main entrance to some building. The court-yard of the British Museum would be an appropriate site for this great obelisk; but we rather incline to the Embankment, if a fitting spot with appropriate surroundings can be obtained.

INVESTIGATION OF THE EFFECTS PRODUCED BY LENGTHENING THE HEATING SURFACE IN STEAM BOILERS.

By JACOB T. WAINWRIGHT, C. E.

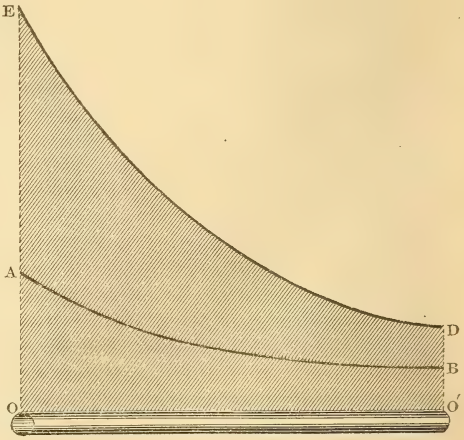
Written for VAN NOSTRAND'S MAGAZINE.

It has been found that: "The rate at which heat is transmitted through any part of a boiler-tube varies directly as the square of the difference of temperature" corresponding to that part.

And, without appreciable error, the velocity of the gases of combustion in their passage through a tube may be considered as constant.

From which it will be seen, that if the ordinates of the curve *AB*, represent the difference of temperature of the inner and outer surface of a tube, the differential equation of this curve may be written thus,

$$\frac{dy}{dx} = -\frac{Cy^2}{V} \quad . \quad . \quad . \quad (1)$$



in which C is a constant for that part of the tube which evaporates the water, and also constant for that part which raises its temperature; and if the feed-water is injected in the same direction as that taken by the gases of combustion, the value for C will be greater than that which it would have, when the feed-water passes in an opposite direction.

V in this equation represents the velocity of the gases of combustion in passing through the tube.

By transposing and integrating in equation (1), the following equation for this curve will be found:

$$y=\frac{V}{Cx} \quad . \quad . \quad . \quad . \quad (2)$$

which is the equation for an equilateral hyperbola.

From this equation it will be seen, that if the ordinates of the curve *ED* represent the rate of conduction through the corresponding parts of the tube, the equation for this curve may be written,

$$y=\frac{C' V^2}{C^2 x^2} \quad . \quad . \quad . \quad (3)$$

in which C' is a constant, and consequently this is the equation of an hyperbola of the second order.

By applying the rule for quadrature, the following expression we obtain for the quantity of heat conducted through the tube in a unit of time:

$$Q=\frac{C' V Y'^2 l}{C y' l+V} \quad . \quad . \quad . \quad (4)$$

in which y' represents the ordinate *OA*, and l, the length of tube.

From which it will be seen, that if in a boiler consuming a given amount of fuel, the length of the tubes be increased proportionally as the smoke area is diminished, the value of V may be represented by $C''l$, (C'' being constant), and consequently equation (4) may be written for this case;

$$Q=\frac{C' C'' y'^2 l}{c y' + C''} \quad . \quad . \quad . \quad (5)$$

From the effect which the value of C has upon this value, it will be seen that it is desirable to inject the feed-water in an opposite direction to that taken by the gases of combustion.

And it will also be seen from the effect which the value of l has upon this equation that it is desirable to lengthen the heating surface at the expense of the smoke area, and that the increase of power and efficiency, so obtained, is proportional to the increased length of heating surface.

It may also be stated, that the efficiency and power so gained, will more than compensate for the mechanical or other contrivances that might be necessary to produce the required draught.

RELATION OF THE RESISTANCE OF MATERIALS TO TRANSVERSE STRESS TO THEIR RESISTANCES TO TENSION AND COMPRESSION.

By WILLIAM KENT, M. E.

Written for VAN NOSTRAND'S MAGAZINE.

In Van Nostrand's Magazine for August, Mr. John D. Crehore gives some formulas showing the relation existing between transverse, tensile, and compressive resistances, and then compares results obtained by the application of his formula to transverse stress with the result of experiments by several authorities. The law upon which he bases his formula he considers "axiomatic and inevitable *a priori*," and the coincidences between his theoretical results, and those obtained by experiment are certainly remarkable.

It is but right that I should call attention to the fact that Mr. Crehore's formula for the relation between transverse resistance and tensile and compressive resistances, is substantially the same as the one discovered by myself about two years ago, and communicated by Prof. R. H. Thurston to the American Society of Civil Engineers shortly afterwards,* in the following words:

"The assumption that resistances vary each way from the neutral surface proportionately with their distance from that surface, is, when coupled with a rejected hypothesis of Navier, nevertheless, not far from the truth in special cases, as may be shown by proper mathematical treatment and comparison with results obtained experimentally." * * * The results of analysis and of experiment give the following values of R in the ordinary formula

$$M=\tfrac{1}{6}RBD^2 \quad . \quad . \quad . \quad (1)$$

for a beam fixed at one end, loaded at the other:

	Cast Iron.	Wro'ght Iron.	Ash.
R—(theoretical)....	32,280	60,000	12,120
R—(experimental)..	35,000	60,000	12,000

13°. This remarkable approximation is thus derived. Suppose a fixed beam, with loaded extremity, and the beam having a depth D, a breadth unity, and a neutral surface situated at a distance Y from the superior surface of the beam. Representing the resistances graphically by the triangles having altitudes $T\,N$, $C\,N$; their measures in tension and compression are respectively $\tfrac{1}{2}\,T\,N$, $\tfrac{1}{2}\,C\,N$, and their moments are

$$\tfrac{1}{2}T\,N\times\tfrac{2}{3}\,Y=\tfrac{1}{3}\,T\,Y^2, \text{ and } \tfrac{1}{2}\,C\,N\times\tfrac{2}{3}(D-Y)=\tfrac{1}{3}\,C(D-Y)^2.$$

An early hypothesis of Navier, which seems to have been entirely abandoned by him subsequently, and which has not been accepted by subsequent writers on the subject, make these moments equal. Assuming this to be correct,

$$T\,Y^2=C\,(D-Y)^2 \quad . \quad . \quad (2)$$

$$\text{and } \frac{C}{T}=\frac{Y^2}{(D-Y)^2} \quad . \quad . \quad (3)$$

and, from this expression, we may find the position of the neutral surface, as determined by the assumed conditions. Then, letting B=the breadth of the beam,

$$W\,L=\tfrac{1}{3}\,B\,[T\,Y^2+C\,(D-Y)^2] \quad . \quad =\tfrac{1}{6}\,R\,B\,D^2 \quad . \quad (4)$$

in which latter expression R is the modulus of rupture, and its value can be found when C and T are known. It will always be of a value intermediate between T and C.

14°. The following are the data and results for the three cases taken: the results are well worthy of examination and record.

	T	C	B	D	L	Y	$D-Y$	WL	R
Cast Iron..........	16,000	96,000	1	1	1	0.71	0.29	5,380	32,280
Wrought Iron......	60,000	60,000	1	1	1	0.5	0.5	10,000	60,000
Ash Timber........	17,200	9,000	1	1	1	0.42	0.58	2,020	12,120."

Mr. Crehore, in his article, refers to the above figures as my *experiments*, but says nothing of the formula given with them. I must, however, disclaim the experiments. The experimental figures I took from the *average* figures given in Wood's *Resistance of Materials* page 80. In fact I proceeded precisely as Mr. Crehore has done, viz: framed the theory first, and then tested it *arithmetically*, by comparing it with the results obtained by various experimenters.

Although the results of experiment agree so closely with those derived from the formula, as shown by the figures above given, and also by the long list given by Mr. Crehore, it is not, therefore, safe to assume the absolute theoretical correctness of the formula. The hypothesis of Navier that the sum of the moments of compression is equal to the sum of the moments of tension, which Mr. Crehore adopts because it seems evident as he says, "in the nature of things, or upon the principle of sufficient reason," and which I also adopted, is disputed by late authorities. I will not, however, now discuss its correctness.

If it be assumed that the sum of the *forces*, and not the moments of compres-

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* Trans. Am. Soc. C.E., Vol. IV p. 287. Dubois' translation of Weyrauch, Prof. Thurston's Appendix, page 202.

sion and tension are equal, the equations (2) and (3) above given will become

$$T\,Y = C\,(D - Y) \text{ and } \frac{C}{T} = \frac{Y}{D - Y}.$$

Equation (4) of the total moment of rupture will remain as it is,

$$WL = \tfrac{1}{3}B\left[TY^2 + C(D - Y^2)\right] = \tfrac{1}{6}RBD^2.$$

in which $Y = \dfrac{CD}{C + T}.$

The *true* theory of rupture by transverse stress has probably never yet been given. The above formula, whether based upon the hypothesis that the moments are equal, or that the forces are equal is only an approximation. A complete theory must necessarily include the effect of the "flow" of the material after it has passed its elastic limit, which will modify the law of proportionality of resistance to distance from the neutral axis, and will vary with every material.

As a statement of conditions upon which a complete and correct theory may some day be based, I believe nothing better can be given than the following remarks of Prof. R. H. Thurston at the Seventh Annual Convention of the American Society of Civil Engineers.

"The ordinary theory, and its resulting equations, in which the resistances of particles to compression and to extension are proportional to their distance from the neutral surface, are apparently sufficiently correct up to that limit of flexure at which the exterior sets of particles on the one side or on the other, are forced beyond the elastic limit.

"With absolutely non-ductile materials, or materials destitute of viscosity, fracture occurs at this point. But, with ordinary materials, and notably with good iron, low steel, and all of the useful metals and alloys in common employ, rupture does not then take place.

"The exterior portions of the mass are compressed on the one side, offering more and more resistance nearly, if not quite, up to the point of actual breaking, which breaking may only occur long after passing the elastic limit. On the other side, the similar sets of particles are drawn apart, passing the elastic limit for tension, and then resisting the stress with approximately constant force, 'flow' occurring until that limit of flow is reached, and rupture takes place.

"Fracture may occur under either of several sets of conditions.

"A. The material may be absolutely brittle. (*a.*) In this case, the elastic limit and the limit of rupture coincide for both simple tension and simple compression. The piece will break with a snap when, under flexure, either limit is reached. (*b.*) Or, it may happen that the limit is reached simultaneously on both sides.

"B. The material may be slightly viscous. (*a.*) The flexure of the piece will produce compression or extension, or both, beyond the elastic limit before rupture, giving three sets of conditions to be expressed by the formula. (*b.*) The increase of resistance, after passing the elastic limit will not be similar for both forms of resistance, and each substance will probably be found characteristically distinguishable from every other. (*c.*) It would appear from experiments already familiar, that the resistance to compression will frequently increase in a very high ratio as compared with that to extension, thus swinging the neutral surface toward the compressed side, and probably sometimes approximately to the limiting surface, with very hard and friable substances, thus bringing about something like a correspondence with 'Galileo's theory.' This, I presume, does not often happen.

"C. The material may be very ductile or viscous.* (*a.*) In this case the phenomena of flexure and rupture will be as last described, but of exaggerated extent and importance. (*b.*) The resistances to extension and to compression as developed in this case, will be approximately, or accurately, those observed in experiments producing rupture by direct tension and by direct compression. The neutral surface will be determined in position by the ratio of these ultimate resistances."

An interesting historical sketch of the various theories which have been advanced upon the relation of transverse to tensile and compressive resistances may be found in Prof. Wood's *Resistance of Materials*, Chap. III. Recent discussions

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* Trans. Am. Soc. C.E., Vol. IV p. 284.

of the subject are published in the Transactions of the American Society of Civil Engineers, Vols. III and IV. The latest theory is given by Mr. D. K. Clark, in his *Manual for Mechanical Engineers*, in which he introduces a new element, called "Diagonal resistance." It is, we think, far from being a correct theory, and has been severely criticised by writers in the London *Engineering*. It is to be hoped that the discussion will be continued, and that some one will through it be led to determine the true theory, and settle forever a question which has vexed engineers since the time of Galileo.

THE LATEST CONTRIBUTIONS TO OUR KNOWLEDGE OF THE SEWAGE QUESTION.

From "The Builder."

It is with some feeling of disappointment, not to say of perplexity, that we endeavour to skim the cream of the recent five days' debate at the Institution of Civil Engineers on the subject of the sewage question. Very much in that debate was of a nature to command respect. New facts were cited. Old facts were illustrated or proved. Men of undoubted eminence in different departments of science addressed the meeting. The dissatisfaction to which we refer arises, not so much from any want of earnestness, skill, or perseverance, but from the fact that, in spite of all, we seem to get no further forward in the matter. The sittings of the 6th, 13th, 20th, and 27th of February, and of the 6th of March last, were occupied in reading and discussing a paper by Mr. C. Norman Bazalgette on the sewage question. But the sittings of 2nd March and 4th and 11th April, 1876, had been occupied by the reading and discussion of papers by Mr. G. R. Redgrave and Mr. W. Shelford on branches of the same subject; and no doubt if we were to trace back the accounts of the Transactions of this body year by year we should find that these were but a few out of many similar debates. But when we ask what is the upshot, we find little more than a record of the experience of contradictory opinions, urged over and over again, among which the bewildered engineer is as much at a loss as ever. We confess that the most distinct conviction which a study of the debate has brought with it is one to the effect that we have too much undervalued certain institutions of the country, on which of late no small degree of discredit has been cast. We mean those rough and imperfect, but yet practical, modes of arriving at a decision on the main gist of a dispute, which we are in the habit of effecting by the verdict of a jury in certain cases, and by a division in either House of Parliament on legislative matters. If some sort of division were called for as to the "conclusions" with which each writer terminates his essay, so that the world should understand that such conclusions did, or did not, meet the acceptance of the majority of the Institution, we might gain much. Not that this would be all that is requisite to make these debates as valuable to the country as they are interesting to those who take part in them. We want the classification so roughly effected in our law courts as to matters of fact and matters of law. We require some sort of seal to be set on the veracity of the statements of fact laid before the Institution; and then should follow a judicial summary of the scientific bearings of these facts. Indeed, we want a series of decisions on specific resolutions, and a competent summary of the upshot and tendency of these conclusions. If this were done, the progress might be slow, but still it would be progress. Now, the movement may be apparently more rapid, but it is, after all, only beating time. No ground is gained. Some of those conclusions as to which we confess we were ourselves disposed to hold that a general acceptance might be predicted, were more ably and more fiercely controverted in the recent debate than probably at any previous time. Mr. Bazalgette referred to the conflicting opinions expressed by the Rivers Pollution Commissioners, by the speakers at the Conference of the Society of Arts, and by the sub-committee of the British Association, and the number of disputants might have been almost indefinitely increased. But in his classification of treatment with chemicals, which if it had been altogether new would have been of permanent value, this gentleman took no notice of the tabulated statement of the comparative outlay on certain chemical processes which was laid before the Institution by Mr. Shelford last year. In his "conclusions," Mr. Shelford then gave exact statements, which must be capable of either verification or disproval. In the former event, a reference to points thus established would have saved much time, both in the composition and the reading of Mr. Bazalgette's paper, and in the discussion which followed. In the latter case, it is no less important that the disproof should have been made clear, so that no unwary person should have been led to rely on the paper last referred to, as embodying conclusions accepted by the main body of the profession. Without offering, at the moment, any opinion as to the actual soundness of any of the views expressed, it is obvious that the form of Mr. Shelford's paper is more exact than is that of Mr. Bazalgette; and that, as a matter of convenience, the substitution of a more general for an exact statement, unless to the effect of proving the latter to be made in error, is not advance, but retrogression.

The "conclusion" arrived at by Mr. Shelford is to the effect that however difficult, and even impossible, it may appear, there can be no doubt that any treatment of sewage which falls short of its profitable application in agriculture, fails to solve the sewage question. The "conclusion" of Mr. Bazalgette is "that no profit must be expected from the cultivation of crops by the sanitary authority, and only a moderate one by the farmer" Mr. Bazalgette further lays down the "conclusion" "that towns situated on the sea coast, or within the tidal range of rivers, should avail themselves of the means of outfall thus presented, as affording the most economical and efficient means of dealing with the sewage." Mr. Crookes, F.R.S., a scientific man of no small note, contends, "were the whole of the sewage to be discharged into the sea, the supply of fixed nitrogen could not stand the drain very long. If, then, the community continued to waste the supply of available nitrogen, the time might come when there would be neither bread nor beef,—and not even gunpowder." It will be seen that if statements of such antagonistic character are published side by side, or even year after year, without any indication of the degree of truth which, in the opinion of the majority of the debating body attaches to either side, the value of the debate, as such, is *nil*, and we merely have an opportunity for the statement of a number of incongruous private opinions, which no one has the power to seize and weld into the compact metal of truth.

It is obvious, in our opinion, and we have before now expressed that opinion, that the first thing to be done in order to arrive at any positive scientific rules with reference to the disposal of the sewage of our towns, is to ascertain whether the product has, in any given case, a practical value. That it has a theoretic value we are ready at once to concede to Mr. Crookes and those who agree with him. As to the maximum money value there is not so much dispute. The practical question is, how many shillings will it cost to extract 5s. worth of value from the corresponding quantity of sewage, while at the same time the offensive and dangerous character of the substance is effectually destroyed? Now, without assuming to speak too positively, it cannot be denied that the general upshot of all experience to that effect is, that no economic treatment, generally applicable, has as yet been discovered. Something to this effect is Mr. Bazalgette's first conclusion: "No chemical process can efficiently deal single-handed with sewage, but must be assisted by subsequent natural or artificial filtration of the treated sewage; and therefore no chemical process *per se* should be adopted for the purification of town sewage." With certain qualifications, we are disposed to accept this conclusion of Mr. Bazalgette. But what the public will ask is,—Is this the conclusion of the

Institution of Civil Engineers, or the mere opinion of an intelligent barrister?

Now, we must confess, with all deference to the many able men who have joined as combatants in this part of the fray, that we are at a loss to understand how it is that this primary question should be at the present time in doubt. We do not make this remark by way of introducing any idea of our own as necessarily the right one, but in the hope of indicating such a mode of inquiry as must lead all impartial investigations into the way of truth. Mr. Crookes tells us that the population of the United Kingdom waste, at the rate of ½ oz. per unit, 445 tons of fixed nitrogen daily, the market value of which is £44,500., or yearly £16,000,000. Dr. Voelcker, who travelled all over Belgium with a view of getting special information from the town authorities, on the other hand, knows of no process, nor believes in the existence of any, for the profitable manufacture of portable manure from sewage. He instances how, in what is called Campbell's process, the addition of £63. worth of chemicals to the million gallons of sewage produced a precipitate of nitrogenous matter of the value of £65. 5s.; but that the cost of the precipitation process for the quantity was £90., which gave a deficit of £1. 2s. 10d. on every ton of manure produced.

Until such extraordinary divergencies in the statements of experts can be explained, it is evident that we have yet to learn the A B C of the sewage question. And it seems to us that the difference between the theoretic and the practical view may be thus investigated. The ingredients of sewage consist of four groups,—namely, water, organic substances, gas, and minerals. Of these water forms nearly 93 per cent.; and the object of the sanitary engineer is to send forth that water chemically pure from his works. Organic matter forms 4.62 per cent. (We are speaking not of dilute sewage, but of pure excreta; for sewage there will be a dilution of from 90 to 100 fold, exclusive of any further dilution from storm water.) It is here that the main source of danger lurks. It used to be considered that the organic matter was a valuable food for plants. It is now, we apprehend, generally known that the reverse is the case. Plants cannot assimilate organic albuminous refuse, and the only thing which can be done with it, to prevent its evil results, is to burn it,—whether that is done by filtering through porous soil, when it becomes gradually oxidised, that is to say, burnt, or in any other manner. No doubt exists as to the fact that this 4.62 per cent. is the main cause of our perplexity and danger, and that its resolution into other forms by combustion is the first object of the engineer. There remain .70 per cent. of mineral, and 1.8 per cent. of gaseous matter. The mineral matter has been derived, by the process of vegetation, from the soil. Its presence is necessary for organic growth. Its return to the soil is a matter highly desirable, or even necessary, if the soil be defective in the elements in question. But here lies the doubt. The mineral elements are lime, magnesia, silica, oxide of iron, soda, potassa, sulphur, and phosphorus, the latter being found in the form of phosphoric acid. It is the last substance alone of which the rarity is such that the return to the soil of the quantity actually abstracted by the animal growth of the crops is an object of extreme importance. It is rather by the mixture of soils, as in Wales, where lime is in many places the only manure employed, than by the return of sewage to the land, that all these minerals, except the phosphorus, and perhaps the sulphur, can be best supplied. So that the real question here is, in what manner can we most economically supply to the growing crops the phosphorus or phosphates which they require?

With regard to the gaseous elements of sewage, a more undetermined, and therefore a more serious, question arises. That vegetable growth requires the presence of nitrogen is well known, as it also requires that of carbon, oxygen, and hydrogen. No doubt, all these gases exist in sewage. But the question which is not as yet absolutely clear is, in what manner, and from what substances, does the plant derive its requisite supplies of these elements? The general view is, that it is from those combinations which are known to exist, and to occur more or less freely mingled with the atmosphere, viz., water, carbonic acid gas and ammonia, that plants derive their food. If this is exclusively the case, it can only be the nitrogen which exists in sewage in the form of ammonia that is of agricultural value. The quantity of this gas found combined in nitrates or nitrites must be left out of sight. Further, it is by no means established that, supposing the other conditions favorable to growth to be present, the plant is unable to supply any deficient amount of nitrogen from the air itself, of which that gas forms so large a portion,—a portion, moreover, not held in chemical combination, as it is in ammonia, but—as is usually considered—in mechanical mixture.

It results that the only element of the food of plants of which we can be tolerably clear that we annually deprive the land by continued cultivation, without return of the manure of the animals, including man, fed on its area, is the phosphorus, in some form, and perhaps the sulphur. Most of the other elements we can readily supply, and at far less cost, than is involved by any process of extracting them from sewage. The phosphates we have also different methods of supplying, of which, it seems very probable, the extraction from sewage is the most costly and unmanageable. We are not at present speaking of the direct application to the soil of effluent sewage. That is an independent question. We are inquiring into the general subject of the extraction from the sewage of great towns of those elements of agricultural value which are not only essential to the growth of plants, but which cannot be more cheaply obtained by any other method, or from any other source, than by extraction from sewage. It is here that so much confusion is allowed to linger. People are too apt to consider the result of a combined process to be favorable, while it might be the case that the result of two independent processes,—one simply a matter of cost, but the other remunerative, would be far more advantageous. It is a consideration of this nature alone, in our opinion, which is adequate, if not to put Dr. Voelcker and his friends in perfect accord with Mr. W. Crookes and his supporters, yet at all events to explain to a third person how it is that two such eminent men take such opposite views, and to show how much truth there is in each of them.

We shall, therefore, be extremely glad if we are able to elicit, from those persons who are able to speak with authority on the subject, a statement of those points on which general, if not unanimous, accord exists as to the main principles which most regulate the satisfactory disposal of sewage. We think we may assume as a primary and cardinal article of accord, the position that all water which has formed a portion of sewage, or come in contact with sewage, must pass through the earth before it is to be regarded as disinfected; and that whatever other methods of disinfection be employed,—filtration, precipitation, or what not,—this earth process is indispensable. We may add, as corollaries to this proposition, that the purifying value of different kinds of earth varies; that experiments have been made on this subject, but that contention still exists as to the value of the results obtained; that further experiment and information are highly desirable; that experience up to this time points to a silicious sand, of sharp grit, as the most valuable sort for the purpose of oxidising sewage; and that the flow of sewage through the stems and roots of plants alone, or through herbage on the surface of the ground, is entirely inadequate to purify the effluent water.

The second proposition, as to which we hold that an almost unanimous assent will be given, is that the organic matter contained in sewage is a positive source of mischief and danger; and that until it be in some way combined with the dose of oxygen which it has lost, it is pernicious, and not useful, to vegetable as well as to animal life. A corollary of this proposition is the statement that the oxidation of this matter at the very earliest moment possible, is the great object which the engineer has to set before him. For example, if cost would allow, the purification of sewage at the very source by the abundant supply of a material containing a large over-dose of oxygen, such as is the case with a well-known disinfecting fluid, would set this matter to rights without more ado. We know, then, what to do, were expense no object, and our efforts should all tend in this direction, while attempting at the same time to exercise practical economy.

Thirdly, it would be of extreme service to all sanitary engineers and agents if chemists and physiologists could arrive at a clear and luminous knowledge of the mode in which plants assimilate their food. The chemical elements of the plants are known. The fact that certain mineral elements, such as silica, are more or less freely present in different plants of the same species, according to the nature of the soil in which they grow, is most important as bearing on this part of the inquiry. But the main point which it is needful here to ascertain is that of the manner in which plants imbibe their gaseous food. Do they at any time absorb oxygen from the atmosphere by their leaves or by their flowers, or only pump it up, as an element of water or of carbonic acid gas, from their roots, and evolve it from their surface? Again, how do they behave as to the other gaseous elements? and in what manner do they assimilate carbon? The general use of vegetable mould as manure points to the view that there is a power in the growing plant to assimilate carbon which is not,—when brought into the proximity with the roots,—in the form of carbonic acid. This point should be made certain. We shall then be more able to ascertain whether nourishment of a nitrogenous kind is derived from the presence of nitrates in the soil, from that of ammonia, or from the nitrogen of the atmosphere. Remembering the curious phenomena of what is called the nascent action of gas (as in the case of the action of hydrogen on spongy platina), and looking at the use of leaf-mould or even of soot, as a manure, there is much reason to suppose that there may be a power in the spongioles of the roots of plants to dissolve solid compounds, or to form new liquid and gaseous compounds, so as to feed their fibres on particles of carbon brought near them in a solid state. The same, it might be inferred, may be the case as to nitrogen. On the other hand, if the opinion that the plant absorbs a main part of its nutriment from the atmosphere directly be correct, it is an argument in favor of the absorption of nitrogen in the same manner. And this is not a mere pedantic bit of trifling, or physiological puzzle; it is a definite, practical inquiry, of a direct economic import. If the plant absorbs all its nitrogen from ammonia, the only value of the nitrogen in sewage will be that due to the quantity of that element which, in the state of ammonia, can be brought into contact with the plant. In that case the nitrates have no agricultural value, except in so far as their mineral elements are concerned, and that is hardly worth discussing. If the plant can derive nitrogen from nitrates present in the soil, the value of that portion of the constituent parts of sewage may be considerable. But if the plant can absorb the comparatively small quantity of nitrogen which it requires in different modes, and can take it from the atmosphere, whether it be present in combination, in the form of ammonia, or in mixture, in the form of atmospheric air, it is plain that we may grossly overvalue the nitrogen present in sewage, and thus be only incurring useless expense by endeavoring to utilise it as manure.

We cannot urge too much the importance of deciding the question of the value of the gaseous elements in question as applied to the growth of plants. No discovery can tend so powerfully to the settlement of the sewage question as this. There is a very strong, and, we believe, a spreading conviction, that the advance of the sanitary question has been more hampered and throttled by the idea that sewage is to be utilised than by anything else. The idea is certainly becoming more and more general that what is wanted is not utilisation, but destruction. Until this fundamental doubt be settled, it is idle to hope for any material progress in the matter. We may question whether the plan of throwing the sewage into rivers or into the sea be a good one or a bad one. But it is one thing if we have only to decide whether we had better, at a given cost, establish a set of outlets of this nature on our shores, or, at another cost, oxidise —that is to say, burn—the offensive matter at home; and another thing if we have to deal with a man who, with all the authority of Mr. Crookes, tells us that throwing into the sea, or burning on the shore, is alike wasteful, and that by adopting either process we are slowly depriving the country of its natural wealth. What we desire is that there should be independent, impartial, and combined scientific effort to remove such a question from the limbo of doubt and of floating opinion altogether, and to place it on the sound basis of scientific truth. And in saying this, we think that it is as much in the interest of the high and honorable position of the truly scientific man, as it is in that of the advance and discovery of truth, to add that the experts whose opinions will be most valued will be those who are entirely disconnected with any process or project of a financial nature. We neither intimate nor believe that any scientific man, worthy of the name, would be consciously biased in giving a chemical or an engineering opinion by personal interest. But those must have a very imperfect knowledge of human nature who are unaware how strong is the influence of unconscious bias. To that influence, in nine cases out of ten, all differences of opinion in matters of scientific research are primarily due. To that cause, to go no further, must be ascribed much, if not all, the reason of the opposite views taken by such men as those to whom we have referred. What is requisite is, that scientific investigation should be purely and exclusively scientific,—that the result which is to come from the test-tube or the receiver should be a matter of indifference to the analyst.

Thus only shall we really advance towards common accord,—by discarding assumptions, by abandoning all erroneous ideas, by laying a firm basis as to what we know, and by thus advancing, step by step, with equal certitude, to that which we desire to know. With the ground quite clear as to the utmost financial advantage which, under given circumstances, can be derived from the chemical elements of which we have spoken, we shall be in a position to discern the cost at which such advantages are to be obtained, and thus to strike the balance of profit and loss, and to understand in which direction to put our shoulders to the wheel.

RESULTS OF EXPERIMENTS ON CONTACT RESISTANCE.*

By Professor W. A. NORTON.

From "The American Journal of Science and Arts."

The experiments here referred to were undertaken with the view of determining the law of the diminution of the minute distance between two surfaces in contact, with the increase of the contact pressure; and its dependence on the extent, condition and nature of the surfaces in contact. Rectangular pieces of various substances $\frac{1}{8}$ inch in thickness, $\frac{1}{4}$ inch in width, and of suitable length for clamping were used in the experiments. The lower piece was clamped to a horizontal iron bar, which was firmly clamped to the vertical pillars of the testing machine used in my former experiments on deflection and set, and was also firmly propped directly beneath the point where the contact occurred. The other piece, $\frac{3}{4}$ inch in length, was keyed to the under surface of the lever used in the same experiments, at the farther end. The weights were placed on a scale pan resting above this on the lever, and vertically over the surfaces in contact. The depressions of this end of the lever were determined by means of a micrometer screw, which gave the equal elevations of the other end to within $\frac{1}{40000}$ of an inch. The firmness of the lower contact piece and its support was frequently tested by causing the weights to press directly upon it, without the intervention of the lever. The small thermal error of the apparatus was carefully determined and allowed for whenever any perceptible change of temperature occurred during any single series of experiments; but the precaution was taken to secure a nearly uniform temperature during the progress of the experiments. The weights employed, in the more precise determinations, ranged from two ounces to twenty-four ounces. The apparent surface of contact varied from $\frac{1}{32}$ of a square inch to a mere point. The touching surfaces were in some instances

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smooth, in others rough; and in the contact of plate glass with plate glass, highly polished. The decrement of contact distance was noted whenever a weight was put on, and the increment when the weight was removed, and in general the average of the two taken. By this means the thermal error, when the rise or fall of temperature was uniform, would be eliminated; as well as any error that might result from a change in the coefficient of the contact resistance, induced by the pressure and not passing off when the weight was removed. That errors from irregular variations of temperature, irregular variations of the coefficient of molecular resistance, and accidental causes, might be in a great degree eliminated, the mean of a considerable number of separate determinations was obtained in each case. A comparison of these means for sets of experiments differing in number, showed that the irregular and accidental errors were generally small. The initial pressure was the same in the different sets of experiments, and was very slight—being barely sufficient to secure a decided contact.

When a weight was applied the resulting diminution of the contact distance was generally greater than the increase that resulted from the removal of the weight. The reverse very rarely occurred; though the increment was sometimes equal to the decrement. It therefore generally happened that there was a slight contact set when the pressure was withdrawn. These facts show that the application of the contact pressure was generally attended with a diminution of the coefficient of molecular resistance at the surface of contact. When the pressures were renewed at short intervals, the contact set at first observed was generally maintained, and often increased.

The following table gives the diminutions of the contact distance obtained with the several weights, 2 oz., 4 oz., 8 oz., 16 oz., and 24 oz. It is to be understood that the numerical determinations given in the table are the means of a number of individual determinations. It thus happens that the decimals are carried beyond the reliable reading of the apparatus. The mean results of different sets of experiments are given in two instances. The apparent surface of contact was about $\frac{1}{32}$ of a square inch, except in the case of the contact of a flat surface with a round surface of sharp curvature, in which the area of contact was too minute to be estimated.

	Iron on Iron.	Same.	Average.	Iron on Iron.	Same.	Average.
	Surf. smooth	Same.		Flat surface on round surf.	Same.	
	In.	In.	In.	In.	In.	In.
2 oz.	0.000170	0.000162	0.000166	0.000165	0.000162	0.000163
4 "	.000250	.000285	.000267	.000240	.000275	.000257
8 "	.000340	.000325	.000332	.000320	.000275	.000297
16 "	.000450	.000425	.000437	.000410	.000425	.000417
24 "		.000500	.000500			

	Iron on Brass.	Brass on Brass.	Brass on Brass.	Plate glass on plate glass.	General average.	Reliable average.
	Surf. smooth	Surf. smooth	Surf. rough.	Surf. polished.		
	In.	In.	In.	In.	In.	In.
2 oz.	0.000167	0.000170	0.000175	0.000170	0.000169	0.00017
4 "	.000256	.000250	.000267	.000212	.000251	0.00025
8 "	.000335	.000256	.000250	.000294	.000294	0.00029
16 "	.000412	.000410	.000400	.000350	.000404	0.00040
24 "	.000500	.000500	.000500	.000475	.000493	0.00049

On examining this table it will be seen,

(1.) That the diminutions of contact distance were very nearly the same, whatever was the nature, or condition of the surfaces in contact.

(2.) That they were nearly independent of the extent of the surface in contact; since they were nearly the same when the surfaces touched in a mere point, as when the surface of contact had an extent of one-fourth of an inch by one-eighth of an inch.

(3.) That the diminution of contact distance for an increase of one ounce in the pressure, was nearly inversely proportional to the pressure. The fractions of an inch that would answer to this law are as follows: For 2 oz. 0.00017 in., for 4 oz. 0.00025 in., for 8 oz. 0.00033 in., for 16 oz. 0.00041 in., for 24 oz. 0.00046 in. These values differ but little from those given in the table as the reliable averages. The only material discrepancies occur in the results for 8 oz. and 24 oz. Now the table of results shows that in a few cases some cause was in operation to reduce the diminution of contact distance for 8 oz. to nearly the value observed for 5 oz. The same tendency was also often manifest in the individual experiments. If we reject the results for 8 oz. in these cases, that occur in the table, the average diminution of contact distance for a pressure of 8 oz., comes out 0.00032 in., and the discrepancy is reduced to 0.00001 in. Again the experimental result for the case of 24 oz. is 0.00003 in. larger than the law above stated calls for; but the individual micrometer readings were liable to this amount of error, and hence if the support had been depressed by this amount, by the 24 oz. weight, it would have escaped detection.

That the law of diminution of the contact distance which has been stated is very nearly, if not the exact law of Nature in the case, may also be inferred from the fact already stated, that the variation of contact distance is nearly if not entirely independent of the extent of the surface of contact. For if the contact area be diminished in any ratio, say 2 to 1, under the pressure of the same weight the pressure at each individual point of contact would be doubled, and the increment of pressure at each point, resulting from an additional weight of one ounce, would also be doubled. Now if we suppose the law, just referred to, to hold good for a given surface of contact, the diminution of contact distance at each point should be inversely proportional to the pressure on it, and therefore be half as great for the same increment of pressure there, as in the case of the larger area of contact; but in fact the additional pressure at a single point, resulting from an additional weight of one ounce, is doubled, and hence the diminution of distance should be the same as in the case of the larger area of contact.

We may conclude, therefore, that in the contact of surfaces, the force of molecular repulsion, in which the force of contact resistance consists, conforms in its variations very nearly, if not exactly, to the law that the decrement of the distance between the molecules, for the same small increment of pressure, is inversely proportional to the effective pressure by which the molecules are urged into closer proximity. If then we suppose the distance between the molecules to be denoted by x, and the effective molecular repulsion by r, and observe that x is a decreasing function of r, we may put $dx = -m\frac{dr}{r}$. This gives, by integration, $x = c - m \log. r$; or $x = m \log. \frac{n}{r}$, in which n is a new constant. It appears then that the curve of the effective molecular repulsion, which resists contact pressure, is the logarithmic curve.

The force of molecular contact repulsion cannot be identical with the effective repulsion in operation in the interior of bodies, when they suffer compression; for the same force of pressure produces a vastly greater diminution of molecular distance at the surface of contact than in the interior of bodies. Thus, in our experiments, a pressure equivalent to 30 lbs. to the square inch, diminished the contact distance by $\frac{1}{2500}$ of an inch. This pressure operating on an iron rod one inch in length would compress it $\frac{1}{800000}$ of an inch. The distance between its individual molecules would be reduced $\frac{1}{800000}$ part. This is immeasurably smaller than the observed diminution of contact distance; and therefore than the diminution of molecular distance at the point of contact, if the decrease of contact distance consisted simply in the closer approximation of the contiguous molecules of the two surfaces. It is not improbable, however, that it consists in part in a compression of a thin layer of molecules at the surface, having a comparatively small coefficient of elasticity. If such a layer have a thickness as great as $\frac{1}{100}$ of an inch, the compression it would receive from a pressure of 30 lbs. to the square inch, would still be 32000 times greater than a layer of the same thickness in the interior of a mass of iron would experience from the same pressure.

We must conclude, therefore, that the force of molecular contact repulsion has, for the same diminution of the distance between the molecules, an exceedingly feeble intensity in comparison with that of the internal molecular repulsion. It must operate then at greater molecular distances; and accordingly the range of its action must lie outside of that of the attraction of cohesion. In confirmation of this conclusion it may be stated that in none of the experiments was any evidence obtained of an attraction between the surfaces, operating outside of the contact distance.

It would seem, then, that the experiments discussed have served to establish the existence of an effective force of molecular repulsion, in operation at the surface of contact of bodies, whose sphere of action is external to the range of the attraction of cohesion for the same molecule, and which has a much feebler coefficient of intensity than the effective molecular repulsion exerted within the sphere of this attraction, They have also made known the law of variation of this force with the change of molecular distance, and shown that its coefficient of intensity is the same, or nearly the same, for the different substances used in the experiments.

THE MECHANICS OF VENTILATION.

By GEO. W. RAFTER, C. E.

Written for VAN NOSTRAND'S MAGAZINE.

I.

FIRST PRINCIPLES.

THE necessity for change of air in inhabited spaces is rendered evident by considering the sources of contamination: they are,

a. The production of carbonic acid by respiration.

b. Increase of moisture from the same cause and exhalation from the body.

c. Organic impurities from the bodily exhalations.

d. Heat thrown off from the occupants and from the lights at night; and

e. The production of carbonic acid from the lights.

The object of ventilation, therefore, is to remove foul air and substitute fresh air in place of the foul air so removed.

Ventilation, then, reduced to its simplest terms, is a matter merely of the movement of bodies of air; and, since, air is a substance possessing weight, the whole question is one of mechanics, and is in the fullest sense susceptible of numerical computation.

This being granted further progress is comparatively rapid, consisting (1) of the development of the laws of air in motion, and (2) in the application of those laws to the removal of foul air, and the consequent supplying of fresh air in buildings and closed structures generally.

Before entering upon the strictly mechanical portion of the subject, it will be well to consider briefly (1) the nature and extent of the contamination rendering change of air in inhabited confined spaces necessary, and (2) the collateral head of ventilation—warming.

SOURCES OF CONTAMINATON.

a. Production of carbonic acid by respiration. When air passes into the lungs it undergoes a chemical change whereby a certain amount of carbonic acid is produced. This when expired adds to the impurity of the air, and were its production in respiration continued a sufficient time in any tight space, the air would ultimately become so impure as to produce death. It has been found by experiment that air in its normal condition contains in all parts of the world an amount of carbonic acid equal to from three to four volumes in 10,000. That some must exist in the air will be readily inferred by considering that respiration and combustion always produce this gas. The amount, however, was only determined by the researches of Regnault, who analysed air from many localities. When carbonic acid is the only impurity from eight to ten volumes in 10,000 may be respired without serious inconvenience, though six volumes in 10,000 is taken as the limit of good ventilation for reasons to appear hereafter. An adult produces 0.6 of a cubic foot of carbonic acid per hour.

b. Increase of moisture from respiration and bodily exhalations. The amount of moisture present in the air at different times varies greatly. Observation shows that there are certain amounts of moisture which when exceeded lead to a rapid deterioration of the air. From 4.5 to 5.0 grains in a cubic foot of air at 60°–62° is the limit of good ventilation.

c. Organic impurities from the bodily exhalations. There is always present in illy-ventilated apartments, especially school-rooms where children from the poorer classes are present, a certain unpleasant smell which a medical friend of large experience in ventilation characterises as "cheesy." In simple justice it must be said that this smell is by no means confined to school-rooms of the kind indicated. Excellent examples of it are frequently noticed in concert halls, theatres, lyceums, and in private houses where even an intimation of uncleanliness would be rank injustice. Organic analysis has thus far been unable to do more than detect the simple presence of this ill-smelling enemy of the human race. It has been found, however, that a rapid increase of the organic impurity takes place when carbonic acid exceeds six volumes in 10,000, or when moisture rises above five grains in a cubic foot of air at 60°–62°. The reason for the limit of carbonic acid and moisture as above given is therefore apparent.

d. Heat thrown off from occupants and from the lights at night. It has been determined by observation that an adult gives out 470–490 units of heat per hour, and that an ordinary sperm or tallow candle gives out in burning one hour substantially the same amount. The specific heat of air is 0.238 when water is taken as unity, consequently the heat from a single person or single candle in one hour would raise 1974 lbs. of air 1°, or, since, a pound of air at 60°–62° equals thirteen cubic feet (exactly 13.09 at 60°) we have heat enough to raise 25662 cubic feet 1°. It will be shown, hereafter, that 2000 cubic feet of air per hour is a fair allowance for an adult. It follows that every person, and every candle, gives out heat enough in an hour, to raise the supply per hour, for each person, from 12° to 13°. If a room were constructed of a capacity equal to say ten persons, and the air supply exactly regulated to give each 2000 cubic feet per hour, the bodily heat alone would increase the temperature of the supply from 12° to 13°.

A large majority of buildings are now lighted by coal gas; we will, therefore, consider the amount of heat produced by the combustion of a cubic foot, having ascertained which it will of course be easy to calculate the elements for rooms of any given capacity and number of lights.

The average of a large number of analyses of coal gas of from twelve to eighteen candle power is as follows :

Hydrogen	=	43.76
Marsh Gas	=	40.47
Carbonic Oxide	=	5.94
Olefiant Gas	=	6.58
Nitrogen	=	1.05
Oxygen	=	0.47
Carbonic Acid	=	0.75
Aqueous Vapor	=	1.00
		100.02

The specific gravity of sixteen candle power gas is approximately 0.450, common air at a temperature of 60° being unity. A pound of air at 60° contains exactly 13.09 cubic feet; consequently, a pound of coal gas of a specific gravity 0.450 and at the same temperature contains :

$$\frac{13.09}{0.450}=29.09 \text{ cubic feet.}$$

A cubic foot of such gas therefore weighs

$$\frac{1.00000}{29.09}=0.0343 \text{ lbs.}$$

Taking the percentage of the constituents of coal gas as given above, we have the following for the weight in pounds of each in a cubic foot of gas :

Hydrogen = 0.01501
Marsh Gas = 0.01388
Carbonic Oxide = 0.00203
Olefiant Gas = 0.00225
Nitrogen = 0.00036
Oxygen = 0.00016
Carbonic Acid = 0.00025
Aqueous Vapor = 0.00034

0.03428

Multiplying each of these weights by the caloric modulus for the corresponding element and we have the number of heat-units evolved in the combustion of that element, and the sum of these units will be the theoretical heat obtained by the combustion of a cubic foot of gas. The calculation referred to the Fahrenheit scale will stand as follows:

Hydrogen =34462×0.01501×1.8 =931.09
Marsh Gas =13063×0.01388×1.8 =326.36
Carbonic Oxide = 3403×0.00203×1.8 = 12.42
Olefiant Gas =11857×0.00225×1.8 = 48.00

1317.87
heat units.

The remaining constituents are non-combustible.

The caloric moduli for the different gases as here used were obtained by combustion in an atmosphere of pure oxygen, while combustion in practice will, of course, take place in common air. Comparing this calculated amount of heat with the observed amount, obtained by calorimetrical tests of illuminating gas of the candle power, and specific gravity above given, shows that a reduction of the calculated heat of forty per cent. is necessary to make calculation agree with experiment. This reduction is undoubtedly owing, largely, to imperfect combustion, in addition to difference of condition. Making such reduction we have

1317.85 × 0.6 = 790.7 = approximate number of heat-units from the combustion of one cubic foot of sixteen candle power illuminating gas.

The mean of several tests was 705 heat-units per cubic foot of gas. We will take, for purposes of calculation, 750 heat units per cubic foot of gas.

Gas burners range from three to six cubic feet consumption per hour, and, where no special arrangements are made for removing the heat and products of combustion from the burners, a simple calculation will show the great influence upon the health of the occupants of an apartment exerted by this apparently insignificant source of contamination.

e. The production of carbonic acid from the lights. According to many careful experiments the carbonic acid from a sperm or paraffine candle equals 0.31 of a cubic foot per hour. Calculation based upon the average composition of coal gas shows that the combustion of a cubic foot of gas produces 0.43 of a cubic foot of carbonic acid. Such a calculation stands substantially as follows : Taking the average analysis of coal gas given above, we have by calculation from chemical equivalency the weight of carbon in a cubic foot of gas, and from this we derive the weight of oxygen necessary to convert that amount of carbon into carbonic acid. The weight of the carbon, plus the weight of the oxygen equals the weight of carbonic acid produced, and is found to be 343 grains. Density of carbonic acid gas is 1.5 at a temperature of 60°. A cubic foot weighs at that temperature then 801 grains, a cubic foot of air weighing 534 grains at that temperature. Dividing the weight of a cubic foot of carbonic acid by the weight of that gas from a cubic foot of coal gas, and we have 0.43 of a cubic foot as the amount of carbonic acid produced by the combustion of a cubic foot of coal gas.

It is evident then that a considerable amount of oxygen is required for the various processes of respiration and combustion going on in confined spaces, and that a further deterioration of the air is continually taking place by reason of the presence of the nitrogen previously mixed with the oxygen so removed; that is to say, oxygen the life-supporting agent in these various processes is continually being removed, while nitrogen the inert, useless element is left behind.

Air in its normal condition contains twenty-three per cent. by weight of oxygen, and seventy-seven per cent. of nitrogen. It will soon result, however, in any confined space where respiration and combustion are going on, that the amount of oxygen constantly decreases while the nitrogen and carbonic acid increase relatively to the amount of oxygen present.

A farther source of contamination is found in insufficient sewer connections, though this part of the subject has been so often discussed that it is unnecessary to consider it at length here.

WARMING.

According to General Morin's paper on warming and ventilation, a translation of which appears in the annual reports of the Smithsonian Institution for 1873–74, heating apparatus should be considered in three different respects:

1. In regard to economy of fuel.
2. In regard to effect on health.
3. In regard to comfort.

The nature of the service to be performed should always be considered in deciding upon the apparatus for fulfilling these considerations. For instance, where occupancy is at intervals, and then only for a short time, the first will decide the choice of method. In buildings occupied more or less continuously, the second consideration should have more weight, while in dwelling houses the second and third together should influence the choice.

The cheapest method of heating is by stoves, more than ninety per cent. of the heat being realized in this way. The first cost is also much less than by the other methods.

Comfortable and healthful heating is obtained by open fire-places, two types of which may be distinguished :

1. Ordinary fire-places.
2. Ventilating fire-places.

The ordinary fire-place takes its supply of air directly from the room and heats solely by radiation. Its heating effect does not exceed fourteen per cent. of the total amount of heat produced. It is healthful and comfortable but not at all economical.

Captain Galton, of the British army, invented a form of the ventilating fire-place which furnishes a very satisfactory solution of the problem of pleasant, healthful heating, combined with excellent ventilation. It consists of a grate with a flue leading therefrom as in the ordinary arrangement. The flue, however, is of a good conducting and radiating material, and passes up through an exterior flue, into which air is admitted from the outside at the back of the grate. This air is warmed by coming in contact with the interior flue, which gives it an upward tendency, and is withdrawn near the ceiling into the room in which the fire-place is located, or it may be conducted into the rooms of the second story. The air to support combustion is taken from the room warmed as in the previous plan. The heating effect realized by this method is thirty-five per cent. of the total heating power of the fuel.

Heating by steam or hot water is pleasant, and when in connection with proper ventilation exceedingly healthful. The heating effect realized is nearly ninety per cent.

Hot air, when properly regulated, is pleasant, and in connection with ventilation, healthful. Heating effect seventy-five per cent.

MECHANICS OF AIR.

At a temperature of 60°, 13.09 cubic feet of air weigh exactly one pound. A cubic foot at the same temperature weighs 534 grains. As already remarked, air in common with all other ponderable bodies obeys the laws of gravitation, and, because of the slight attraction between its particles, it is, like gases generally, extremely sensitive to changes of temperature or pressure.

According to Gay Lussac's law the volume of one and the same gas varies with the temperature.

With notations as follows :

V_0=volume at 32° and barometer at thirty inches.

V=volume at the temperature t.

$a=0.00204$=coefficient of expansion, expresses the increase of volume for an increase in temperature of one degree Fahrenheit.

We have by formula

$$V=[1+a(t-32)]V_0 =[1+0.00204(t-32)]V_0 \quad (1)$$

In case the air is farther heated to the temperature t_1 the corresponding volume becomes

$$V_1=[1+0.00204(t_1-32)]V_0 \qquad (2)$$

the ratio of the volumes V and V_1 will be

$$\frac{V}{V_1}=\frac{1+0.00204\,(t-32)}{1+0.00204(t_1-32)} \qquad (3)$$

According to Mariotte's law we have :

1. The density of one and the same quantity of air is proportional to its pressure.

2. The volumes of one and the same quantity of air are inversely as the pressures.

3. From 1 and 2 it follows that the densities are inversely as the volumes.

Let D=density at volume V and D_1= density at volume V_1 therefore

$$\frac{D}{D_1}=\frac{V_1}{V}=\frac{1+0.00204(t_1-32)}{1+0.00204(t\ -32)}. \qquad (4)$$

Farther let P_0 equal pressure at 32°; P that at temperature t, and P_1 that at t_1 therefore

$$V=1+0.00204(t--32)\frac{P_0}{P}V_0. \qquad (5)$$

and

$$V_1=1+0.00204(t_1-32)\frac{P_0}{P_1}V_0 \qquad (6)$$

hence

$$\frac{V}{V_1}=\frac{1+0.00204(t-32)P_1}{1+0.00204(t_1-32)P}. \qquad (7)$$

and

$$\frac{D}{D_1}=\frac{1+0.00204(t_1-32)P}{1+0.00204(t-32)P_1}. \qquad (8)$$

therefore

$$V_1=V\left(\frac{1+0.00204(t_1-32)P}{1-0.00204(t-32)P_1}\right) \qquad (9)$$

and

$$D_1=D\left(\frac{1+0.00204(t-32)P_1}{1+0.00204(t_1-32)P}\right) \qquad (10)$$

Now suppose t=32° and P_1=P and D =0.081 in the last formula and we have,

$$D_1=\frac{0.081}{1+0.00204(t_1-32)}, \qquad (11)$$

from which the density of the air can be found at any temperature, the pressure P being constant.

In case it is desired to calculate the density D at any pressure whatever, we may write in the above formula where b is any height of barometer and 29.92= height at temperature of 32° and D= 0.081−

$$\frac{P_1}{P}=\frac{b}{29.92}$$

and again taking t= 32° and D=0.081 we have

$$D_1=\frac{0.081}{1+0.00204(t_1-32)}\times\frac{b}{29.92} \qquad (12)$$

According to the law of physics, known as the principle of Archimedes, *a body plunged into a fluid loses a part of its weight equal to the weight of fluid displaced.* This law may apply to three different cases :

1. The weight of the body may exceed the weight of the fluid displaced, or in other words, the mean density of the body may be greater than that of the fluid, in this case the body sinks.

2. The weight of the body may be less than that of the fluid displaced; in this case the body rises partly out of the fluid until the weight of the fluid displaced is equal to its own weight.

3. The weight of the body may be equal to the weight of the fluid displaced; in this case the two opposite forces being equal the body is in equilibrium and remains in any position in which it may be placed. (See Deschanel's Physics, Chapter IX, Principle of Archimedes).

We will consider the application of these laws in the case of common air, considering at first a single cubic foot.

It has been shown, that at a temperature of 32°, and under a pressure of 29.92 inches of mercury, every cubic foot of air weighs 0.081 pounds. While the temperature of the single cubic foot remains at that point we have, by case 3 above, a condition of equilibrium, and no movement or disturbance takes place, since an equivalent volume of the surrounding air has exactly the same weight.

Suppose, however, the temperature of the air is at 70° and that by coming in contact with some cold surface, as, for instance, a window pane, the temperature of the single cubic foot is reduced to 32°. The result of such reduction is by the preceding discussion threefold :

(1) Volume reduced,
(2) Density increased,

(3) Consequently it becomes heavier than surrounding air and tends to fall by a certain definite weight found as follows: Taking formula (11)

$$D=\frac{0.081}{1+0.00204(70-32)},$$

reducing we have

$D=\frac{0.081}{1.077}=0.0752$=the weight of a cubic foot of air at 70°.

The volume under the new conditions may be found by formula (1).

The cubic foot of air in passing from the temperature of 70° to that of 32°, increases its weight by $0.081-0.0752=0.0058$ pounds. Carrying this calculation into grains, we have weight at $70°=7000\times0.0752=526.4$ grains, weight at $32°=7000\times0.081=567.0$ grains, $567.0-526.4=40.6$ grains=gain in weight of a cubic foot of air by reduction of temperature from 70° to 32°.

In cold weather, when the windows and outside walls are much colder than the general temperature inside, it is evident from the preceding that currents of air must be continually passing downward to the floor along such cold surfaces.

Suppose, farther, the air is at a temperature of 40° and by coming in contact with a heated surface is warmed to 98°. Density at $40°=0.0797$ pounds. Density at $98°=0.0714$ pounds. Or, a cubic foot in passing from temperature 40° to that of 98° loses weight by an amount equal to $0.0797-0.0714=0.0083$ pounds$=7000\times0.0083=58.1$ grains. Our particular cubic foot of air is then in the condition of case 2 of the principle of Archimedes. Its tendency is to rise and it continues doing so when unconfined, until a point is reached where the densities are again equal.

The temperature of the human body is 98° while 40° is about an average temperature of the air during the whole year. It is evident, therefore, that a ventilation of the body is continually taking place through the operation of natural causes. We should take advantage of the hint thus obtained from nature, and plan the ventilation of our dwellings and public buildings upon a more rational basis than at present seems to prevail.

It is evident, without special discussion, that movements similar to those just ascribed to the particular cubic foot of air are, in fact, constantly taking place throughout the whole atmosphere, not only out of doors but in as great a degree inside. The atmosphere, considered as a whole, can never be at rest, the slightest changes in temperature produce variations in volume, density and pressure, and these again are the causes of unending motion. In short, we may say the air is perpetually in unstable equilibrium.

The temperature at which inhabited spaces should be kept varies from 60° to 65°, depending to a considerable extent upon the nature of the operations carried on. It is certain, moreover, that the temperature can be kept lower without discomfort in well-ventilated apartments, than in those which are not well-ventilated. Upon this point the writer has recently made some investigations which may with propriety be introduced here.

The revised charter of the city of Rochester, of last year, created a Board of Health with more extended authority than that exercised by previous boards. Among other items the new board has authority to regulate the ventilation of public school-houses throughout the city. During the past winter the writer at the instance of said board made an extended examination of all the school-buildings to the number of twenty-five, and reported thereon at length. In the course of the examination it appeared necessary to ascertain exactly the temperature at which the several rooms were kept. In going through them and using a thermometer in each it very soon became apparent that there were extraordinary variations in the temperature. The school-rooms, numbering more than two-hundred, were then supplied with thermometers, and the teachers carefully instructed as to the manner of taking observations, &c. The observations were taken ten times a day for one week, the times of taking being as follows: At the beginning of school (9 A.M.); at end of first hour, (10 A.M.); before morning intermission; (10.45 A.M.); after morning intermission, (11 A.M.); at end of morning session, (12 M.); at beginning afternoon session (1.30 P.M.); end first hour (2.30 P.M.); before intermission, (2.45 P. M.); after intermission, (3 P.M.); end of afternoon session (4. P.M.).

These observations in connection with a careful consideration of the facilities for ventilation furnished matter of the highest interest. They led, indeed, to the inevitable conclusion that in schoolhouses where the ventilation is thorough, pupils and teachers are perfectly comfortable at a temperature of 60°–62°, and that the temperatures invariably increase as perfection of ventilation decreases. So true is this that the following laws are fairly deducible, which so far as an nquiry into the ventilation of schoolhouses is concerned, may be denominated the laws of temperatures.

1. In rooms having but one outside exposure the temperature is uniformly higher than in those having two or more outside exposures, other conditions being the same.

2. Where the communication is direct by means of roomy halls between lower and upper floors, the temperature ranges higher on upper floors.

3. Of two rooms having equal exposure and equal heating and ventilating facilities, the one containing the greater number of pupils will show the higher average temperature.

4. There is a relation between inside temperature, outside temperature and outside humidity, which relation appears to be expressed by saying that inside temperature varies directly as outside humidity and inversely as outside temperature.

The writer cannot at present vouch for the entire correctness of this last law; the observations were somewhat conflicting, though the preponderance is decidedly in favor of the law. It stands at present, however, as an inference rather than a positive induction.

The temperatures show invariably an increase from beginning of school in morning (9 A.M.) to end of first hour (10 A.M.). They also show a corresponding increase from beginning of school in afternoon (1.30 P.M.) to end of first hour (2.30 P.M.). In buildings of no ventilation, and where, during the winter, flushing with fresh air is resorted to by opening windows at intermissions, the thermometers were naturally lower after intermission than before; while in those where flushing is not resorted to, the temperatures were substantially the same. The maximums were usually obtained before intermission, and at end of session. Observations on humidity were also taken in several of the buildings, and it was found that a great increase of moisture in excess of that outside took place in poorly-ventilated rooms, pointing to the conclusion that excess of temperature is accompanied by excess of moisture, and to the farther consideration that excess of temperature, and excess of moisture are certain indications of defective ventilation.

Judging from the showing of the temperature records a great difference of opinion exists as to the proper temperature at which a room should be kept. The rooms are undoubtedly kept at the point of comfort and that point varies as perfection of ventilation varies. For instance, the lowest temperatures occur in well-ventilated rooms, while the highest occur in those poorly-ventilated. In rooms where the temperature ranges from 62° to 66° the question was asked of the teachers in at least twenty places, "Do you find the room too warm when much above 66°? The reply usually was, "We do." In the case of rooms with an average temperature of 68°–71° the form of question was, "Do you find the room too cold when much below 68°? The reply was in nearly every case, "We do." The general principle, therefore is, in imperfect ventilation the exhalations from the body and breath produce an excess of moisture in the air, and, consequently, an increase of relative humidity. Our sensations of heat and cold depend (within limits) as much upon the amount of moisture present in the air as upon the actual temperature. A high relative humidity always produces chilliness. We instinctively, therefore, increase the heat as the moisture increases. Moreover the air's capacity for moisture increases as the temperature. In illy-ventilated apartments, the two go together, increase of moisture leading to increase of heat, increase of heat leading to increase of moisture. Aside from the mere waste of heat resulting from this condition of affairs, there is matter of more serious import to be considered. Where bad ventilation exists the windows and doors are thrown wide open for flushing at intermissions and noontime. These results are immediate depression of temperature, in some cases sufficient to fall below the dew point, hence moisture is actually deposited as dew about the room, and hence colds and sickness of teachers and pupils generally. The same thing will occur not only in school rooms, but in any illy-ventilated apartment where a large number of people being congregated, windows are suddenly opened for the admission of fresh air.

The question of humidities is somewhat complicated. It will be eminently proper, therefore, to consider briefly the principles involved; and in making such considerations Deschanel's Physics will be drawn upon. With this general acknowledgement such use is made of the excellent work in question as may be necessary to the proper elucidation of the subject in hand.

The condition of the air as regards moisture involves two distinct elements: (1) the amount of vapor present in the air, and (2) the ratio of this to the amount which would saturate the air at the actual temperature. There are two important laws bearing upon the subject:

1. The weight of vapor which will enter a given space is the same whether this space is empty or filled with a gas.

2. When a gas is saturated with a vapor the actual tension of the mixture is the sum of the tensions due to the gas and the vapor separately; that is to say, it is equal to the tension which the gas would exert if it alone occupied the whole space plus the maximum tension of vapor for the temperature of the mixture.

The word *tension* means in this connection the force acting to produce expansion, as opposed to pressure, or the force tending to produce compression, or we may say the tension decreases the density of a gas while pressure increases that element.

Relative humidity is defined as the weight of aqueous vapor in a given volume of air, expressed as a percentage of the weight of vapor at saturation which would occupy the same volume at the actual temperature.

When air containing water in form of vapor is gradually cooled at constant pressure, its capacity for vapor gradually decreases until the point of saturation is reached. Any farther reduction of temperature is accompanied by a deposit of moisture. This point of deposit is called the dew point, the relation of which to the actual temperature is shown by the humidity tables below.

There is a popular idea that air loses its moisture by heating. In fact, since moisture exists in the air, so far as known in the form of vapor and not mechanically suspended, the only way the air can lose moisture is by reduction of temperature to the dew point, and by consequent deposition in the form of dew. The effect of heating air is to increase the capacity for moisture, accompanied by decrease of relative humidity, an apparent dryness may thus be attained, although the absolute humidity remains the same as previous to the heating.

We will now consider the tables given below.

Table No. 1 is a general exhibit of the condition of ventilation at school-house No. 5, with one exception the best ventilated public building in the city of Rochester. This building is warmed by heated air, from three large radiating furnaces placed in the basement. The heating apparatus has, however, one serious defect in not permitting an adequate supply of fresh air, owing to the supply conduit being too small. The result is that the currents of air on reaching the rooms show a temperature much higher than they should. Ventilation is by means of open fire-places in each room in which a fire is kept burning only during shool hours. The flues from the fire-places are of ample capacity to carry away the impure air, and, however crowded the rooms may be, the air is always fresh and pleasant.

Table No. 2 is a similar exhibit for school-house No. 13, one of the worst ventilated buildings in the city. This building is warmed throughout by stoves.

Table No. 3 is a similar exhibit for school No. 9, a complete discussion of which will be found afterwards.

Table No. 4 is a humidity table for No. 5, while table No. 5 is a similar exhibit for school No. 13.

The tables are generally self explanatory, though one or two points may be specially noticed.

No. 1.—Table for No. 5.

No. of grade.	No. boys belonging.	No. girls belonging.	Total No. pupils in room.	No. sittings in room.	Floor area in square feet.	Ratio of sittings to floor area square feet.	Volume in cubic feet.	Ratio of sittings to volume cubic feet.	Deficiency of air space for each pupil in cubic feet.	Amount of air to be supplied per hour in cubic ft.	No. changes of air required per hour.	Estimated capacity of exhaust per hour in cubic ft.	Deficiency of exhaust per hour in cubic feet.	No. outside exposures.	No. windows.	Temperature Fahrenheit.	Maximum.	Minimum.	Range.
9th	40	29	69	55	704	12.80	9559	174	0	46750	4.9	50000	0	2	6	65°.27	70°	60°	10°
8th	25	24	49	55	749	13.62	10150	185	0	57750	5.6	60000	0	2	6	64.72	70	60	10
7th	19	18	37	53	749	14.12	10152	191	4	62010	6.1	63000	0	2	6	62.17	68	54	14
6th	28	28	56	47	704	14.99	8893	189	36	63450	7.1	65000	0	2	6	65.02	68	62	6
5th	25	31	56	49	704	14.28	8891	181	59	70560	7.9	72000	0	2	6	66.56	70	62	8
4th	21	28	49	47	749	15.94	9470	201	62	74260	7.9	75000	0	2	6	67.06	72	60	12
3d	19	31	50	47	749	15.94	9470	201	89	81780	8.6	83000	0	2	6	67.06	71	58	13
2d	16	17	33	36	704	19.55	11012	306	2	66600	6.0	45000	21600	2	6	66.39	72	58	14
1st	15	31	46	42	749	17.83	11703	279	54	84000	7.1	45000	39000	2	6	67.12	72	57	15

No. 2.—Table for No. 13.

No. of grade.	No. boys belonging.	No. girls belonging.	Total No. pupils in room.	No sittings in room.	Floor area in square feet.	Ratio of sittings to floor area square feet.	Volume in cubic feet.	Ratio of sittings to volume cubic feet.	Deficiency of air space for each pupil in cubic feet.	Amount of air to be supplied per hour in cubic ft.	No. changes of air required per hour.	Estimated capacity of exhaust per hour in cubic ft.	Deficiency of exhaust per hour in cubic feet.	No. outside exposures.	No. windows.	Temperature Fahrenheit.	Maximum.	Minimum.	Range.
																°	°	°	°
9th D	24	17	41	48	343	7.35	3386	70	72	40800	12.1	0	40800	1	2				
9th C	18	15	33	44	350	7.70	3456	79	63	37400	10.8	0	37400	1	2				
9th B	18	20	38	40	313	7.82	3240	81	61	34000	10.5	0	34000	1	2				
9th A	23	12	35	42	389	9.26	4292	102	40	35700	8.3	0	35700	1	4				
8th B	24	30	54	56	781	13.95	9476	169	6	58800	6.2	0	58800	2	4	68.16	73	62	11
8th A	20	36	56	64	775	11.95	9394	147	28	67200	7.1	0	67200	1	2	67.91	78	62	16
7th	36	33	69	80	761	9.51	9200	115	80	93600	10.2	0	93600	1	2	68.22	72	61	11
6th B	20	18	38	58	775	13.36	9395	162	63	78300	8.3	21000	57300	1	2	68.64	75	62	13
6th A	23	18	41	56	794	14.18	9612	172	53	75600	7.8	21000	54600	2	5	65.53	72	60	12
5th	29	21	50	50	754	15.08	11818	236	4	72000	6.1	14000	58000	2	5	70.19	78	62	16
4th	16	25	41	46	787	17.11	12202	265	0	72680	5.9	21000	51680	1	2	70.05	76	56	20
3d	29	23	52	53	772	14.56	11975	226	64	92220	7.7	14000	78220	1	2	70.41	75	63	12
2d	18	10	28	42	779	18.55	12110	288	20	77700	6.4	0	77700	1	2	70.35	76	62	14
1st	11	21	32	36	786	21.83	12369	343	0	72000	5.8	0	72000	2	4	70.84	76	64	12

The column of "amount of air to be supplied per hour," is obtained by multiplying the number of sittings in each room by the corresponding factor from the table of "amount of air to be supplied for each pupil in the different grades" (Table No. 7). This factor was obtained as follows: The empirical assumption was made that the pupils in first grades require 2,000 cubic feet of air per hour, and that in the lower grades they require an amount proportional to their ages. The column of "amount of air space for each pupil" of this table was obtained in this way: It is well established that where change of

No. 3.—Table for No. 9.

No. of grade.	No. boys belonging.	No. girls belonging.	Total No. pupils in room.	No. sittings in room.	Floor area in square feet.	Ratio of sittings to floor area square feet.	Volume in cubic feet.	Ratio of sittings to volume cubic feet.	Deficiency of air space for each pupil in cubic feet.	Amount of air to be supplied per hour in cubic ft.	No. changes of air required per hour.	Estimated capacity of exhaust per hour in cubic ft.	Deficiency of exhaust per hour in cubic feet.	No. outside exposures.	No. windows.	Temperature Fahrenheit.	Maximum.	Minimum.	Range.
9th D	32	25	57	56	416	7.43	5369	96	46	47600	8.9	48000	0	2	4	65°.94	75°	56°	19°
9th C	28	26	54	56	435	7.77	5442	97	45	47600	8.7	48000	0	1	2	67.51	75	57	18
9th B	29	17	46	56	427	7.62	5408	96	46	47600	8.9	48000	0	2	4	67.39	72	58	14
9th A	39	13	52	56	435	7.77	5442	97	45	47600	8.7	48000	0	1	2	68.64	79	57	22
8th	24	21	45	56	433	7.73	5417	97	78	58800	10.8	60000	0	1	2	67.74	76	58	18
7th	19	27	46	56	439	7.84	5492	98	97	65520	11.9	67000	0	1	2	67.93	78	56	22
7th&6th	22	24	46	64	437	13.05	9878	154	56	80640	8.2	82000	0	1	2	68.59	75	56	19
6th	19	22	41	41	425	10.36	6208	152	73	55350	8.9	57000	0	2	4	68.17	75	60	15
5th	15	17	32	41	435	10.61	6292	153	87	59000	9.4	60000	0	1	2	68.51	72	60	12
4th B	12	15	27	41	435	10.61	6292	153	110	64780	10.2	66000	0	1	2	66.99	70	58	12
4th A	12	15	27	40	427	10.67	6255	156	107	63200	10.1	65000	0	2	4	69.11	74	60	14
3d	11	14	25	40	433	10.83	6273	156	134	69600	11.1	71000	0	1	2	69.00	75	60	15
2d	13	13	26	36	433	12.03	6273	174	134	66600	10.6	68000	0	1	2	67.68	71	59	12
1st	15	5	20	32	865	27.03	13361	417	0	64000	4.8	65000	0	1	2	68.35	74	59	15

No. 4.—Humidity Table for No. 5.

Date of observation.	Time of observation.	Grade in which observations were taken.	Barometer.	Actual temperature.	Temperature of dew point.	Force of vapor in inches of mercury.	Amount of vapor present in a cubic foot of air in grains.	Amount of vapor required to complete saturation at actual temperature in grains.	Relative Humidity.
Tuesday, March 6, 1877.	From 2 P. M. to 4 P. M.	9th		61°	42°.3	0.286	3.21	2.85	53.0
		8th		64	41.9	0.282	3.15	3.50	47.3
		7th		65	42.6	0.289	3.22	3.65	46.9
		5th		68	48.8	0.358	3.96	3.57	52.6
		4th		68	45.6	0.321	3.54	3.99	47.0
		2d		65	44.2	0.306	3.39	3.48	49.3
		1st		68	45.6	0.321	3.54	3.99	47.0

Outside Humidity for same Date.

	11-58 A.M		30.210	20	5.5	0.060	0.71	0.59	55.6
	2.00 P.M.		30.128	22	3.5	0.069	0.80	0.61	58.4
	4.33 P.M.		30.007	25	9.0	0.067	0.68	0.68	50.0

air, in average apartments, occurs at a greater rate than six times an hour, currents and unpleasant drafts are produced, which are likely to seriously affect the occupants. The limit of space for a single person will, therefore, be the amount of air to be supplied to each person divided by six. In this way the column in question was obtained.

In tables 1, 2 and 3 the column of "no changes of air required per hour" is the number of changes required per hour to

No. 5.—Humidity Table for No. 13.

Date of observation.	Time of observation.	Grade in which observations were taken.	Barometer.	Actual temperature.	Temperature of dew point.	Force of vapor in inches of mercury.	Amount of vapor present in a cubic foot of air in grains.	Amount of vapor required to complete saturation at actual temperature in grains.	Relative Humidity.
Tuesday, March 8, 1877.	From 1.30 P. M. to 4.00 P. M.	8th B		63°	46°.0	0.326	3.63	2.82	56.3
		8th A		65	50.6	0.381	4.23	2.64	61.6
		7th		70	55.0	0.442	4.87	3.13	60.9
		6th B		70	55.0	0.442	4.87	3.13	60.9
		6th A		67	52.6	0.408	4.52	2.78	61.9
		5th		71	54.5	0.435	4.78	3.47	57.9
		4th		71	53.0	0.414	4.54	3.71	55.0
		3d		71	53.0	0.414	4.54	3.71	55.0
		2d		70	52.0	0.400	4.40	3.60	55.0
		1st		70	52.0	0.400	4.40	3.60	55.0

Outside Humidity for same Date.

	2.00 P.M.		29.569	42°	37°.6	0.243	2.83	0.47	85.8
	4.33 P.M.		29.491	43	38.6	0.252	2.93	0.48	85.9

Table No. 6 *is an exhibit of the mean daily temperature in several of the school buildings in the City of Rochester for each of the days on which observations were taken. It shows unmistakably that the same cause has modified the result throughout the whole series. For instance, the range is higher on Tuesday than on Monday, lower on Wednesday, higher than Wednesday on Thursday, and generally still higher on Friday, though there are exceptions to this as will be seen by inspection.*

No. 6.

No. of school.	Monday, Feb. 26, 1877.	Tuesday, Feb. 27, 1877.	Wednesday, Feb. 28, 1877.	Thursday, March 1, 1877.	Friday, March 2, 1877.
2	64°.12	65°.46	64°.48	65°.58	66° 57
3	66.35	66.83	65.62	66.46	67.16
4	65.87	66.09	65.84	66.89	66.01
5	65.01	66.06	65.54	66.51	65.54
6	67.52	68.20	67.82	67.74	67.77
8	61.25	65.05	65.05	66.60	66.17
9	68.13	68.26	67.39	67.73	68.31
11	68.26	69.75	65.56	66.00	66 44
12	66.32	66.80	66.00	66.20	67.73
13	68.21	68.62	66.99	70.16	69.06
16	69.75	71.05	70.44	71.71	71.64
17	68.26	68.40	68.24	68.31	70.38
18	67.02	67.34	67.29	68.61	68.00
19	67.30	66.71	65.69	66.12	65.66
20	65.43	67.33	65.66	67.27	68.22
Means	66.58	67.46	66.50	67.45	67.64

No. 7.

Table of amount of air to be supplied for each pupil in the different grades.

Grade.	Average age in years.	Amount of air for each pupil per hour in cubic feet.	Amount of air space for each pupil in cubic feet.
9th	6.26	850	142
8th	7.76	1050	175
7th	8.66	1170	195
6th	9.52	1350	225
5th	10.69	1440	240
4th	11.72	1580	263
3d	12.90	1740	290
2d	13.69	1850	308
1st	14.88	2000	333

keep within the standard of ventilation here assumed.

The column of "deficiency of air space for each pupil" is a comparison between the column of ratio of sittings to volume and the "amount of air space for each pupil," and shows what addition to the volume for each sitting would be necessary to bring the room within the limits of scientific ventilation.

The temperatures in tables 1–3 are the averages of all observations.

The necessary calculations of humidities are from Glaisher's tables, while the outside humidity record is from the signal service office at Rochester.

According to General Morin, the amount of air to be changed every hour, in order to keep within the limits of good ventilation is as follows :

	Cubic feet.	
Hospitals	2.119	3.709
Prisons		1.776
Workshops	2.119	3.532
Barracks	1.059	1.776
Theatres	1.413	1.776
Lecture and assembly rooms, &c.	1.059	2.119
Schools	.424	1.059

Having decided how much air shall be supplied per unit of time, the next question is how shall the supply be furnished ? This introduces us to our subject proper, *The Mechanics of Ventilation.*

Two methods of producing the result may be distinguished, the *vacuum* and the *plenum.* We confine ourselves entirely to a consideration of the former, as being more in accordance with the natural order of affairs, and as having been demonstrated by experience to give the better results.

The vacuum method requires that every apartment or building shall have, suitably connected with it, a vertical shaft, in which an exhaust draft is constantly maintained. The problem in hand, then, is the determination of the dimensions of such a shaft and its contingent connections, together with such special considerations as shall make the method applicable to any case whatever.

The point of withdrawal will be as near the source of contamination as convenient, that is, at or near the floor. They should never be in the floor itself, as dust, etc., will collect ultimately causing unpleasant stoppages. The points of ingress should, on the contrary, be as far away from the contamination as possible, or at the ceiling. It may be taken as a general law that whatever the method of heating resorted to, foul air should be drawn out of a room by creating a vacuum at some point in the side walls near the floor, while fresh air should be introduced at or near the ceiling.

We have seen that heating a limited amount of air to a temperature higher than the surrounding atmosphere causes the heated portion to ascend. This then offers in many cases a cheap and efficient means of ventilation, by simply utilizing the large amount of heat now wasted through the chimneys. It should be noticed in passing that although there is a great waste of heat at present through the chimneys, it seems certain that improved ideas of chimney building will soon in a great degree correct that evil. In the meantime, the ventilation of structures already erected may be improved by taking advantage of the method here indicated.

The formula expressing the conditions of movement of heated air in a vertical flue is

$$V=E\sqrt{2g\frac{ha\,(t'-t)}{1+at}} \quad . \quad (13)$$

where the notation is

V=velocity of ascent of air current per second.

E=a numerical coefficient, constant for each shaft, and expressing the reduction of the theoretical velocity due to friction, eddies, &c.

g=the acceleration of gravity numerically equal to 32.2 lineal feet.

h=the height of the chimney or flue, and the consequent height of the confined column of air.

a=coefficient of expansion of air =0.00204 as previously given.

t'=temperature within the chimney, and

t=temperature of outside air.

This formula is merely an expression in extended form of Torricelli's theorem, which asserts that the velocity of any freely falling body, is equal to the square root of twice the acceleration of gravity, into the height through which the body falls. Expressing this more elegantly as an equation, and we have

$$V=\sqrt{2g\,h'} \quad . \quad . \quad . \quad (14)$$

where g has the value just given and h' is the height through which the body falls.

In formula (13)

$$\frac{ha\,(t'-t)}{1+at}=h\frac{1+at'}{1+at}-h=h' \quad . \quad (15)$$

We have seen by previous discussion that when air is heated at constant pressure the volume increases. Now in case the heating takes place in a flue open only at the ends, it is evident the expansion must cause an increase in the length of the confined column. Such increase is expressed by h'. Moreover the difference between the original length of the column at temperature t, and the new length at temperature t', is equal to the height through which a volume of outside air equal to the original volume within the flue has fallen, to produce the upward movement within the flue; h' is, therefore, the head of fluid producing pressure, and may be calculated by the general formula for head of fluids flowing through pipes, &c., following:

$$h'=1+\xi_1+\xi_0+\left(\xi\frac{l(a+b)}{2ab}\right)\frac{V^2}{2g} \quad (16)$$

This formula means, that having given the cross-section of a flue, pipe or conduit, and the amount of air to pass through per unit of time, which of course determines the velocity v, per unit of time, the height of column of air necessary to produce the velocity v through the given cross-section, is equal to h' as expressed by the above formula, the notation of which is as follows:

h' has the value previously given.

ξ_1, ξ_0 and ξ are coefficients of resistance of elbows, efflux and influx and friction respectively.

l=length of pipe.

a and b=dimensions of cross-sections.

v=velocity of current per unit of time, and

g=acceleration of gravity as previously given.

In case the flue is cylindrical the expression

$$\left(\xi\frac{l(a+b)}{2ab}\right) \text{ will become } \xi\frac{l}{d}.$$

General formula for velocity is

$$V=\frac{Q}{F} \quad . \quad . \quad . \quad . \quad (17)$$

where Q=quantity discharged per unit of time, and F=sectional area of duct.

Formula (13) enables us to say what the conditions of discharge are in a vertical flue. It depends upon:

1. The height h; that is the power of the flue, or its velocity of discharge increases as the square root of the height increases, the difference of exterior and interior temperatures remaining constant.

2. The square root of the difference of t' and t; that is with a given height of flue the velocity of discharge will increase as the difference between interior and exterior temperatures increases.

3. The resistance to ascent of currents of air by friction and eddies; that is the velocity of discharge, will increase as the smoothness, straightness and adaptation of the cross-section of the flue to the amount of air to be disposed of increases.

It follows from these conditions that having the height, sectional area and general arrangement of any chimney or flue, the quantity of air or products of combustion discharged per unit of time will always be the same, provided the excess of temperature inside the chimney over that of the outside air remains constant.

We will now make a numerical application of the principles here set forth; the example selected for such purpose being school building No. 9, in the city of Rochester.

The object in taking this particular case is that a system of ventilation by the vacuum method was recently constructed at that building. Owing, however, to neglect of the principles here set forth, hardly moderate success was attained. The discussion will therefore include not only the numerical application to that building, but also critical notes on the ventilating works as they were actually carried out.

By referring to the table No. 9, given above, it will be seen that the building contains fourteen rooms, and that the amount of air to be renewed per hour is 837890 cubic feet. The volume of halls is in round numbers 7000 cubic feet. Adding for change of air about twice each hour in halls, and we may take the total amount to be renewed per hour at 850000 cubic feet. The building is two stories high, having seven rooms to each floor, and is L shaped, fronting upon two streets. In the inside angle of the L a shaft fifty-five feet in height was erected, straight inside, with sectional area top and bottom equal to 26.62 square feet. From the foot of the shaft large ducts were carried to either extremity of the L.

The main ducts were (20″×44″); secondary ones (16″×36″); tertiary ones (16″×22″); and so on, decreasing as distance from main shaft increased. Into these horizontal ducts were let vertical ones which connected with the rooms by openings in floor, over which iron registers were set. The number of opening and corresponding ducts from each room is six. Sectional area of each of these small ducts (6″×8″)×48 square inches. The elbows are in every case square. The number of small ducts leading to halls is 13, and since there are 6 to each room, and 14 rooms the total number of exhaust openings is found to be 97. We will assume for present purposes that the openings have equal capacities, though the variations of resistance because of elbows and difference of length of small ducts leading to main ducts in cellar give them in fact, varying capacities. On this assumption each duct will take away a quantity per hour equal to

$$\frac{850000}{97}=8763 \text{ cubic feet.}$$

The amount removed per second by a single duct will be

$$\frac{8763}{3600}=2.434 \text{ cubic feet.}$$

According to formula (17)

$$V=\frac{2.434\times 1728}{6\times 8+12}=7.3 \text{ lineal feet.}$$

In fact the free area of opening is not more than two-thirds of the sectional area of pipe, owing to the register set over the opening. Or the velocity through register will be

$$\frac{4206}{32\times 12}=10.9 \text{ lineal feet.}$$

THE POSITION AND PROSPECTS OF THE BRIISH IRON TRADE.

From "The Engineer."

Next to the extent and duration, accessibility and cheapness, of our coal supplies, there is no subject of greater importance to the industrial interests of Great Britain than the available resources existing within our shores for the production of cheap and useful iron. One committee after another has sat upon the coal question, which has now been more or less exhausted in every phase it is capable of presenting. But of our mineralogical resources, so far as the manufacture of iron is concerned, we have but the most scanty available information; and that, too, scattered over such a wide area, and presented in such fragmentary and sectional contributions, that it can only be read and known by a few. It is almost superfluous to point out that not only are the progress and prospects of the iron trade of cardinal importance for its own sake, as the most extensive, utilitarian, and valuable of all our metallurgical industries, which is, to a very large extent, the foundation and source of all the collateral avenues of wealth and prosperity that have caused England to be distinguished as "the workshop of the world;" but also, because there is no other influence that so nearly and so directly affects the still more vital consideration of the future of our coal supplies, seeing that not less than forty million tons of coal, or nearly one-third of the total quantity raised in the United Kingdom, are employed, one way and another, in the manufacture of iron.

There never was a time when the iron trade suffered more acutely from adverse influences than it does now. Into the whole *rationale* of the process whereby that suffering has been brought about we shall not attempt to enter; but it cannot be otherwise than a matter of deep and anxious concern to the trading and commercial interests of the country at large, to examine certain phenomena which seem on the first blush to justify the conclusion that, for the first time in its history, the iron trade of England has entered on a retrograde career, and has come very near to if it has not actually gone beyond its legitimate development.

Let it be clearly understood that this is put forward simply as an appearance and not as a fact; but the appearance is sufficiently startling, and is environed with so many attendant circumstances not heretofore examined, that we need make no apology for formulating and analysing the actual facts.

It may be well to clear the way by explaining that the iron trade is almost entirely a product of the last half century. In 1740, only 17,000 tons of pig iron were produced in the United Kingdom, from fifty-nine blast furnaces—a production scarcely equal to that of a single fair-sized blast furnace at the present time. In 1796, the annual production of pig iron was 125,000 tons, the number of blast furnaces having, meanwhile, advanced to 130. Thirty-four years later the production had attained the extent of 678,417 tons, the trade, according to the evidence of Sir John Guest, having remained stationary between 1823 and 1831. But from the latter year it began to make rapid strides, until Mushet found a total make of 1,248,781 tons in 1839, the number of furnaces then in blast being 396. In his well-known work on the "Progress of the Nation" Mr. G. R. Porter, F.R.S., returned the make of pig iron in 1840 at 1,396,400 tons, and he calculated the quantity of coal consumed in the operation of smelting that production at 4,877,000 tons. Adding to this, 200,000 tons of coal used in converting the crude iron into wrought iron, it will be found that the total bulk of coal consumed in our iron manufacture thirty-six years ago was only 6,877,000 tons, as compared with about forty million tons at the present time. Steady progress continued to be made in the pig iron trade until 1855, when the total production was returned at 3,218,154 tons, the make having advanced rather more than two million tons in fifteen years. During that period a strong impulse had been given to the development of trade from two remarkable events—the first being the introduction of the hot blast, which reduced the consumption of coal used for smelting a ton of pig iron by nearly two-thirds; and the second, the commercial discovery of the Cleveland ironstone in 1850, which brought about the development of a much cheaper and more extensive field of iron ore than any formerly known to exist. Up to 1858 the growth of the pig iron trade had proceeded almost without a check. Commercial depression, however, caused the make of that year to fall about two hundred thousand tons below that of the previous year, while the estimated value of the 3,456,064 tons then produced fell to £8,640,160, as compared with £9,148,617, the value of the produce of 1857. During that *annus mirabilis* of commercial disaster, 1866, the accretion of growth was again hindered, the make having fallen from 4,819,254 to 4,523,897, and the estimated value from £12,048,133 to £11,309,742. But the dawn of another season of comparative prosperity speedily restored the old order of things, and during the next five years, the expansion of our metallurgical industry was great beyond all precedent, the increase between 1866 and 1871 having been over two million tons per annum, or an increment more than double the whole make of the country in the year 1830. The main increase was between 1870 and 1871, when the make of pig iron rose from 5,963,515 tons to 6,627,179 tons; the quantity of pig iron exported from 753,339 tons to 1,057,458 tons, and the total quantity of coal used in the manufacture of both crude and finished iron from 35,344,634 tons to 38,540,102 tons.

It is now that we come to face the strange, and somewhat inexplicable fact, that since 1871 there has been a diminution both in our production of pig iron and in the quantity of iron ore raised in the United Kingdom. Such a result is not only completely at variance with the experience of the past, but belies both the calculations and expectations of every authority on the subject of our industrial resources. To the large and rapidly accumulating demands made by the iron trade was attributed the origin of the coal famine, and the Royal Commission appointed to inquire into the operation of that event found that the order of things had been: first, the rise in the price of iron, then the rise in the price of coal, and, finally, the increase in the rate of wages. Every one who took the trouble to inquire into the causation of events concluded that the future development of the coal trade would be largely determined by the demand for iron-making purposes; and yet we find the altogether bewildering truth to be that concurrently with a considerable falling off in the consumption for iron making, there has been a very large increase in the aggregate production of coal. The total quantity of pig iron made in the United Kingdom in 1875 was only 6,365,462 tons, or in round figures, about two hundred and fifty thousand tons less than in 1871. The total quantity of coal used in the manufacture of pig iron in 1875 was 15,645,774 tons, as compared with 19,881,537 tons in 1871, being, in round figures, a decrease of four million tons; but while this decrease took place in the consumption of coal for iron-making purposes, we find, not as might have been expected, a corresponding falling off in the production of coal, but an increase of production to the extent of about fifteen million tons! Hence, therefore, the fact is clearly established that, although the iron trade as a whole—including both its crude and its finished products—absorbs nearly a full third of all the coal raised in the country, the development of our coal supplies is not only not directly contingent upon the demand for iron-making, but follows certain lines with which that demand has little or nothing to do, and may henceforth be expected to proceed without that regard to the requirements of our chief manufacturing industry on which the Royal Commissioners and other authorities have laid so much stress.

The origin and operation of the apparent decadence of the iron trade of Great Britain form a still more important and perplexing source of inquiry. In 1871 we raised in the United Kingdom 16,334,888 tons of iron ore. In 1875, the total quantity of iron ore raised was only 15,821,060 tons. In the former year, however, we imported only 324,043 tons of iron ore, as compared with 458,693 tons in the latter; and the burnt or purple ore imported within the same period advanced from 200,000 to 280,000 tons. The increase in the latter items is too trifling, perhaps, to justify the conclusion that our supplies of native ore are becoming exhausted, and that we will henceforth be more and more dependent upon foreign supplies; but this is an inference fairly warranted by the greatly increased demand for good hematites suitable for the steel manufacture, of which in this country we have such a limited supply that we are compelled year by year to increase our imports from Spain, Algeria, and elsewhere. The following figures exhibit the relative increase or decrease of iron ore, produced in the several districts yielding that mineral in the United Kingdom, between the years 1871 and 1875, the latter year being the most recent for which authentic returns are as yet available:

Name of District.	1871.	1875.
	Tons.	Tons.
Cornwall...............	21,947	11,403
Devonshire.............	14,124	10,594
Somersetshire	32,883	45,165
Nottinghamshire	—	11,750
Gloucestershire.........	207,598	111,825
Wiltshire...............	159,894	87,152
Oxfordshire	28,330	34,568
Northamptonshire	779,314	1,085,898
Lincolnshire............	290,673	573,366
Shropshire	415,972	240,568
Warwickshire	34,075	97,456
Staffordshire, North	1,513,080	939,023
Do. South	705,665	715,451
Cheshire	—	1,500
Derbyshire	492,973	218,132
Lancashire.............	931,048	834,484
Cumberland	1,302,703	1,147,968
Yorkshire, North Riding	4,581,901	6,121,794
Do. West Riding	407,997	353,582
No'humberl'd & Durham	285,297	60,515
North Wales...........	51,887	42,184
So'th Wales & Monmouth	969,714	495,840
Isle of Man.............	75	—
Scotland...............	3,000,000	2,452,235
Ireland.................	107,734	128.602
Totals	16,334,888	15,821,060

The reader will find, if he takes very slight trouble in the analysis of the above statistics, that most of the older districts are going to the wall, having either become nearly exhausted, or furnishing their produce at such a heavy cost as to render it commercially impossible to proceed with their further development; and this fact is abundantly attested by the difference in the value of the ores raised during the years above named. We find that although there has been a decrease of 513,000 tons in the quantity of iron ore produced in 1875 as compared with 1871, the difference in value is not less than £1,695,000. Such a circumstance, moreover, is all the more remarkable when we bear in mind that the average price of pig iron in 1875 was considerably above that of 1871. The mean price of Cleveland pig iron during 1871 was £2 9*s.* 6d. per ton, whilst in 1875 it was £3 per ton; and Scotch pig iron averaged £3 2*s.* 2d. in the former year as compared with £3 4*s.* in the latter. Broadly stated, the above table brings out the fact that it has become necessary for the English ironmaster to discard the expensive ores of such counties as Cornwall, Devonshire, Gloucestershire, Wiltshire, Shropshire, and Derbyshire, in favor of the less expensive and coarser ores of Cleveland, Northampton, and Lincolnshire; and these three new districts are consequently elbowing the older districts out of the race of competition and usurping their places. Between 1871 and 1875 two new iron ore-producing districts—Notts and Cheshire—have come into the field, but neither of them has as yet assumed any real importance; and to all practical intents the ironmasters of England must look in the future towards Cleveland, whose supplies of ore, illimitable in extent, are now being worked out at the rate of about seven million tons per annum; Lincolnshire, where the iron ore is more cheaply worked than any other part of the country, although the absence of an immediately contiguous coal-field places the smelter at a disadvantage; and Northamptonshire, whence large supplies of ore are now sent to older districts, whose resources are more scanty and precarious. It may be accepted as an industrial axiom of the iron trade that when a district no longer depends upon itself alone for raw material it must cease to advance if it does not absolutely tend towards decay. And this is precisely the position in which Wales and Staffordshire are now placed. The production of iron ore in South Wales has been diminished one-half between 1871 and 1875, and in North Staffordshire has gone more than half a million tons to the bad as between the two dates. Scotland, another of our most venerable centres of metallurgical industry, has been reducing her output of ore for some years, consequent upon the rapid exhaustion of that famous deposit of blackband ironstone on which her ferruginous prestige has been reared. These districts may, of course, be able to maintain a kind of parasitical influence for years by importing largely from other districts, as they have actually begun to do; but in the last resort they must yield up the sceptre to such districts as can claim to be more richly endowed with the resources which they lack. The most promising districts in the future are more distinguished for the cheapness and abundance than for the quality of their ores. For steel-making purposes and the production of high-class iron, the resources of this country are exceedingly meagre. Even in Cumberland, where the most extensive deposits of hematite iron known in this country have originated the marvellous fecundity and enterprise of Barrow-in-Furness, there has recently been a declension of supply, and the future of the hematite iron trade in regard to local supplies of ore is becoming a source of no little anxiety. The ores of the West Riding, out of which the renowned bands of Bowling and Low Moor have been evolved, are assuming attenuated proportion; and in the dales of Durham, where the spathose and sparry ore has been worked for a number of years by the Weardale Iron Company, the supply has so far given out that the quantity worked is now diminishing year by year. There are several districts, it is true, where large quantities of superior ore are still known to exist; but it would not pay to work the ores of such localities as Wiltshire, Cornwall, and Devonshire on a large scale, considering their long distance from a suitable supply of coal; and the result of the past few years leaves little scope for belief in an augmentation of supply from these sources. It is increasingly probable that the great bulk of our high-class ores will henceforth be imported from Spain and elsewhere; and this, although comparatively a new trade, has already attained very respectable proportions. As a single proof of the potential direction of this fact we may mention, that in the Cleveland district large works are now being built to smelt Spanish hematite for steel-making purposes, in the immediate locality of mines capable of producing the cheapest iron ore in this or any other country; and if "carrying coals to Newcastle" has heretofore been regarded as typical of the greatest anomaly, it will hereafter be paralleled by the still greater anomaly of carrying ironstone to Cleveland.

It is desirable that we should pursue this subject one stage further, and take cognizance of some recent features of our pig iron trade. To insist upon the importance of this branch of our national industry would be entirely supererogatory after what we have already stated; but in a single sentence we may affirm, that for some years past Great Britain has produced about one-half of all the pig iron made in the world; that in and through the pig iron trade employment is furnished to fully half a million hands, representing a total population of at least two millions; that pig and finished iron rank in the Board of Trade returns as the most valuable of our manufactured productions, and that they jointly absorb more labor, more *matériel*, more ingenuity, and more capital than any other industries, whether at home or abroad.

We have not yet at command the detailed figures relative to the production of pig iron in 1876, but, from the most authentic sources available, we have sought out the actual returns of that year, and here we present them, along with an abstract of the production of the different districts enumerated for the years 1871 and 1875 :

Name of district.	1871.	1875.	1876.
	Tons.	Tons.	Tons.
Northumberland	34,165	22,870	2,070,000
Durham	759,244	786,206	
Yorkshire, North	1,029,885	1,240,243	
Do. West	114,549	267,153	—
Derbyshire	270,485	272,065	—
Lancashire	520,359	558,780	520,000
Cumberland	336,569	486,112	400,000
Shropshire	129,467	120,996	—
Staffordshire, North	268,300	241,398	220,000
Do. South	725,716	470,540	390,000
Northamptonshire	60,512	80,689	—
Lincolnshire	30,122	111,683	—
Gloucestershire and Somersetshire	99,997	59,819	—
North Wales	41,893	55,099	—
South Wales	1,045,916	541,809	420,000
Scotland	1,160,000	1,050,000	1,103,000
Other districts	—	—	1,027,000
Totals	6,627,179	6,365,462	6,150,000

The figures for 1876 are, of course, subject to the modification of more accurate knowledge, but we believe they will be found pretty near the truth, and assuming their approximate correctness, it follows that our pig iron trade has declined to the extent of about half a million tons, while its value has diminished to the extent of nearly two millions sterling since 1871. There is another curious fact germane to this inquiry. The number of furnaces in blast fell from 673 in 1871 to 629 in 1875, while the nnmber of furnaces in blast during 1876 must have been still less.

How far the results of the last two or three years may be affected by the events of the future it is impossible to foresee or anticipate. The prohibitory price of iron of all kinds in 1872–73 stimulated the development of the resources of other countries, which would otherwise in all probability have been content to depend on England for their supplies of iron, and hence we find that over-production has caused a crisis in other countries as well as at home. This is more particularly true of the United States, where the make of pig iron has steadily declined for the last two years; and the iron trade of this country has certainly the satisfaction of knowing, that if production has progressed backwards at home it has not been otherwise abroad. But then England is different from other nations in respect of having resources superior to most, and a prestige and supremacy far above all, which justify the reasonable expectation that we should be the last to feel the pinch and the first to retrieve losses. It is impossible to overlook the fact that important changes have recently influenced the iron trade of Great Britain—changes the full import and significance of which are hardly yet understood, and to which we have not quite become reconciled. Foreign competition will assail us more and more. It is now defensive, but will ultimately become doggedly aggressive, and in some quarters it will more than tax our energies to hold our own. There is hardly a single European nation that is not trying to become independent of our wares. Some countries have wrested orders from our grasp, and there is no more singular phenomenon of trade than that presented by the Belgians—when they import our pig iron and work it up into the finished article at a price that enables them to undersell us in our own markets. Cheap and nasty commodities are falling out of repute. The enormous requirements of the railway world will not henceforth, as heretofore, be satisfied by iron rails made from the most coarse and silicious ores that England can show, but must more and more demand a superior metal, made from the best ores, and of such ores England is certainly not possessed to a greater extent than some other European nations, who have the superior gain of cheaper labor and closer proximity to large and lucrative markets. On the other hand, however, England can always depend upon being able to produce a cheap quality of iron fit for many of the ordinary purposes of commerce, and capable, by chemical and other processes now being evolved from the region of experiment, of turning out a much better product than anything yet realized.

EXPLOSIONS OF STEAM BOILERS.

By JOHN W. HILL, M. E., a Member of the American Society of Civil Engineers.

Contributed to VAN NOSTRAND'S MAGAZINE.

The alarming frequency of explosions, especially in the rural districts, demands that the attention of State Legislatures be directed to a speedy solution of the important problem of safety in the use of steam boilers. The interest of the public in a proper system of inspection of steam boilers is rapidly developing, and the necessity of such a surveillance of the manufacture and operation of this eminently useful and dangerous adjunct of civilization, as will reduce explosions to a minimum, is probably felt by all, however remotely interested in steam machinery.

What is required is the appointment of a Board of Inspectors in every State, to investigate and report upon every explosion, as well as to pursue a rigid system of inspection of the construction and use of steam boilers.

Whilst it is not imagined that such a Board could enter upon their duties sufficiently charged with information to prevent all explosions in the future, their association with the work from year to year, and by frequent exchange of views with other similar Boards, would presently expand and develop their knowledge in a manner not to be attained by other processes.

It appears to the writer that the appointment of an Engineer with a selected corps of assistants, to inspect all boilers now in use and recommend legal measures for the prevention of disastrous explosions in the future, would be quite as desirable a "luxury" as the usual Geological corps, for whilst the labors of the latter may improve our knowledge of the physical structure of our respective increments of the sphere, and open up avenues to unexpected wealth, the labors of the former will save priceless lives and property to the extent of millions.

That there are certain political objections to the inauguration of such a system is admitted, but the combined wisdom of our State law makers should be sufficient to meet the "legal" and "moral" impediments to a rigid law regulating the manufacture and use of steam boilers.

However this may be, no one who is a constant reader of the metropolitan daily papers can doubt the necessity of a careful system of inspection of the materials and workmanship employed in the construction of steam boilers, and in the use of the boiler after it is set to work.

The great majority of accidents are not with boilers in the hands of men who, from the force of circumstances, are supposed to have a certain knowledge of the "regimes" to be established in operating a steam boiler, but with the rural steam users whose knowledge is naturally very "limited," and as naturally very "dangerous." Whether the frequency of explosions in the "country" is the immediate result of the lack of appreciation of the dangers surrounding a seething boiler, or to impositions practiced on the unwary by knavish boiler makers in furnishing poor workmanship and defective materials, is a question to be determined. That poor materials and workmanship are often the "prime" cause of disastrous explosions is well known, and however this may be, a system of rigid inspection, by competent officials in every state, would speedily bring the construction and use of steam boilers to the proper level.

The system of inspection should embrace: The form of boiler as affected by the water of the locality in which it is to be used; The variability of load, and the fuel to be burned in the furnace; The dimensions as affected by maximum capacity required; The thickness of plates, class of riveting and caulking, and quality of iron to be used as affected by maximum pressure under which the boiler is to be worked; The test to be applied to the iron used, and the tests to be applied to the finished boilers; The manner of heating and purifying the feed water and its introduction into the boiler; The style of furnace to be used and general arrangement for facility of inspection; The safety appliances, and standard of tests for "steam gauges," "safety valves," low water alarms and other devices applied to steam boilers.

Every steam boiler now in use, and every steam boiler made in the future should be subject to inspection, and a "seal" put upon it, and a certificate with restrictions under which it may be worked, furnished the owner, tampering with the one or exceeding the other to be visited with a severe punishment.

In France a manufacturer cannot put in use a steam boiler without a permit from the prefect of the department. In making an application for "license" to purchase and put to work a steam boiler, the manufacturer addresses the prefect on a government blank furnishing the following information: Maximum pressure of steam under which the boiler is to work; Horse power and class of connected engine; Form of boiler desired; Location of boiler in relation to buildings and public highway; Fuel to be burned; Nature of business conducted in the establishment, and plan of location (on separate sheet).

The prefect of the department refers the application to the prefect of the *arrondissement*, who in turn refers it to the mayor of the *commune;* this officer then proceeds to an investigation *de commodo et incommodo.* The investigation is continued for ten days; five days after its termination the mayor addresses the *proces-verbal* of the investigation, with his recommendation in the premises, to the prefect of the arrondissement who transmits with his opinion to the prefect of the department. The prefect then lays the *proces* before the nearest government engineer who examines and delivers an opinion upon which the decision of the prefect is based. This decree of 1810 (which as the writer is advised is still in force) in connection with an ordinance passed in 1843 relating to steam boilers, which provides that the boiler shall be tested—first, at the shop where it is built; second, at the establishment where it is to be used—by the nearest government engineer, who after inspection furnishes the owner a certificate of condition and restrictions under which the boiler shall be operated. The tests are *obligatory* (except for mines) and give the manufacturer an *immunity* in the use of a steam boiler nowhere else approximated.

Under our system, or rather lack of system, the manufacturer buys and operates his boiler at his own option; if he desires to drive a forty horse power engine with a twenty horse power boiler, there is no law so far as the writer is aware to prevent his doing so.

The one great impediment to procuring a legal enactment relating to steam boilers is the general indifference of the public to the safety of human life. Take the preceding instance: If in carrying out the intention to drive a forty horse power engine with the twenty horse power boiler, the boiler "lets go," the public sympathy would be as great for the man who lost the boiler as for the men who lost their lives.

As an illustration of this, the writer would relate a circumstance happening several years ago:

A steam boiler, furnishing power to a very large agricultural machine shop, exploded with terrible violence, demolishing one entire section of the building, and killing and injuring several of the workmen; the writer, coming on the ground a few minutes after the explosion, saw the workmen bearing off the corpse of one of the victims. Shocked at the sight, and desirous of ascertaining the extent of damage to life and limb, he suggested to a bystander "that it appeared to be a very rough accident;" the response came in a suppressed tone, "It was rough on Smith, he would be obliged to buy a new boiler." In this instance six men were killed and perhaps twenty seriously injured. It may not be out of place to remark that this was one of those rare cases where the engineer enjoyed the princely income of "six dollars a week."

In nearly every instance of boiler explosion, it appears that the usual legal investigation of the causes of the accident is a mere "farce," that neither determines the real or proximate causes, or locates the blame where it properly belongs; and whilst the facts usually adduced at the inquest may form a foundation upon which the experienced engineer can build a theory of explosion, it is in the great majority of cases simply absurd to base a legal verdict upon the opinion of men whose knowledge of the steam boiler is of the most limited kind.

Several years ago a small cylinder boiler furnishing steam to a "digester" in a large soap and candle works in Cincinnati, suddenly "let go," killing the attendant and one of the factory hands on the spot; while a section of the shell weighing upwards of a thousand pounds passed directly up two or three hundred feet, thence westward nearly a half mile and fell killing three small children.

At the inquest it was ascertained that no one but the attendant was to blame, and as he was already dead, the coroner "generously forebore to prosecute him;" at the same time the facts in this case as related to the writer by the previous attendant of the boiler, were such as to have condemned the proprietors to several years penal servitude, under the "boiler law" of Prussia.

Coupled with the lack of legal inspection, the general location of boilers in many of our large manufacturing establishments is reprehensible in the highest degree. In the city of Cincinnati there is a certain establishment covering a superfice of 300×200 feet, and lifting skywards seven stories.

Each of the floors except the basement, contains a small army of workmen, and thousands of dollars worth of costly materials and manufactured goods in various stages of completion. In the basement about as central as posts and stone pillars would permit, is located the battery of boilers furnishing the power to drive the machinery. Let us suppose an explosion in this case, what would be the probable results? Is it to be imagined that any large portion of the several hundred workmen shut up in this miniature Vesuvius would escape whole? By no means. Let the slender threads now linking safety to disaster loose their hold, and the pent up volcano would burst forth pouring human lava through the vents. Such an occurrence would fall upon the community like a mantle of darkness, and great would be the desire to locate the blame *somewhere.* The coroner would assume an air of marvelous concern, and swear by the party that put him in office, that the affair should be probed to the quick, and the fault brought home to its father, "though angels weep." To this end a jury would be struck, composed of distinguished citizens, with a plentiful *lack* of information upon the questions to be brought before them, who after the usual delays would "on with the quest."

The picture may be highly colored, but the outlines are lifelike, as any one may verify who will read the testimony and verdicts of the inquests following appalling accidents.

The fall of the Dixon bridge, the breaking of the Mill River dam, the Ashtabula horror, and the late total demolition of the Rockford court house, furnish excellent magazines of information upon the customary "legal" proceedings following these wholesale murders.

If we could have the inquest before the accident instead of after, how much better it would be; although this might seem "paradoxical," it is the spirit of the French law regulating the use of "steam boilers," and an explosion in that country *is a rare event.*

ON THE BOTTOM-VELOCITY AND THE VELOCITY-SCALE OF RIVERS

By J. SCHLICHTING.

Translated from "Zeitschrift für Bauwesen," for "Abstracts" of Institution of Civil Engineers.

In estimating the water masses of rivers, only approximate values of the velocity of water immediately above the bed have yet been obtained, because the instruments in use do not permit of a direct measurement, and the theories relating to it are not universally accepted.

To remove this difficulty, to clear away doubts respecting the bottom velocity, and to find a law applicable to all streams, numerous measures were made in the years 1873 to 1875 inclusive, and it has been found that the bottom velocity at any point can be determined where the velocity has been measured by instruments at two known points vertically above it.

With the exception of Hagen all recent writers on this subject assume the velocity scale to be a parabola with axis horizontal and under the surface, while according to observations of A. Gasse on the Elbe it has been found on the surface.

The Author remarks that the results of the American experiments are founded upon an arbitrary grouping of experiments, and are consequently of no value, and that the only essential differences now remaining are whether the maximum velocity lies in the surface or below it, and whether the axis of the parabola is horizontal or vertical.

The Author used Woltmann's current meter for all measurements of velocity. For surface velocities one edge of the vane just touched the surface of the water while the opposite edge was 7.8 inches (20 centimètres) below it, and the resulting velocity is regarded as that at the depth of 4 inches (10 centimètres). The velocities lower down were taken at intervals of 19.7 inches. Five cross sections of the Memel were taken near the bridge of boats at Tilsit, and were plotted on paper, the most suitable points being chosen for the velocity measurements, and marked by buoys. A pole 20 feet long was driven into the bed of the river, so that the current meter might slide up and down, and be suspended at any required depth by a light chain. Velocities were taken at 550 points in 130 different verticles. Each velocity was taken three times, during 100 seconds, and the arithmetic mean was adopted as the true velocity. The maximum velocity was found 80 times in the surface, or at the highest point of measurement, 4 inches under the surface; 5 times, simultaneously, in the surface, and 19.7 inches below the same; 35 times at 19.7 inches below the surface, and 10 times at 39.4 inches below the surface. The minimum velocity was found at 125 times at the lowest point of measurement, and 5 times at points from 11.8 inches to 19.7 inches higher.

The Author concludes that the velocity continually diminishes from the surface to the bottom, and it would follow a mathematical law if the irregularities in the velocity of running water could be eliminated. These irregularities near the shore produce whirlpools, sometimes even reversing the direction of the revolution of the vane.

Two Woltmann's current meters were used and always showed small and variable differences, but similar small variations were noticed when a single instrument was employed. The Author attributes these irregular variations to the irregular motion of the water, and to the inaccuracy which always attends velocity measurements with the Woltmann meter, and to the imperfection of the instruments themselves.

He observes that the formulæ for determining the value of a revolution of the vane are open to weighty objections, and prefers to use the simple formulæ for the velocity of the water,

$$v = \beta n;$$

where β is the value of one revolution of the vane taken from a number of experiments in still water, and n the number of revolutions in a second of time.

He remarks that if the velocity of the boat used for testing the current meter in still water be the same as the velocity of the stream to be measured, the above formula is the only correct one, and that in all other cases the unavoidable errors of observation render any co-efficient which might be introduced of little value.

If the motion of the water suffered no resistance from the friction of the bed, and from the pressure of the air upon its surface, the velocity scale would be a vertical straight line, because the velocity would be the same, the pressure and the fall being the same.

But on account of these resistances the motion of the lowest layer is retarded by the friction of the bed, and this retardation will affect the motion of the next layer, and so on to the surface. The retardation gradually diminishes from the bottom upwards, and the form of the velocity curve or scale would appear to correspond better with a parabola with its axis vertical and its vertex in the bed, than with a parabola with horizontal axis on or near the surface. The resistance due to the pressure of the air upon the surface of the water may modify the form of the velocity of the scale, supposing the air to be still; but no known instrument is capable of giving such small differences of velocity as would be required to establish this point. Floats will not suffice for this, because, besides their immersement, they have a part of their surface in the air; also they give only the mean velocity in a certain length, and not the velocity at any single point. It is probable that the resistance of the air at the surface of the water acts in the same way as the friction of the bed, but in a much smaller degree.

This would lead to the conclusion that the velocity scale was in its upper part a parabola with its axis horizontal and under the surface, and that this secondary parabola would meet the principle one in an unknown point not far below the surface.

This curve, composed of portions of two parabolas, may be the true velocity scale when water is flowing with steady motion, and the air is still;—the velocity scale being accepted as a parabola with its axis vertical, and its vertex in the bed.

After reviewing various formulæ, the Author sums up as follows:—

1. The mean velocity scale of rivers for each cross section, as long as it does not present any striking irregularites of depth, is a parabola with the axis vertical and the vertex at the bottom. The scale is obtained from average values of direct measurements of velocity, and consists of a rectangle contained by the mean bottom velocity as base, and the mean depth as height, and of a semi-parabola of the same altitude. Two average values suffice to determine the velocity scale, and also the bottom and mean velocities, and the parameter of the parabola.

2. The maximum velocity in a stream, not influenced by wind, storm, ebb, or tide, lies in the surface or very near it, as well in the mean velocity scale as in the separate verticals, excepting those near the shores.

The minimum velocity is directly above the bed, and the mean at $\frac{4}{9}$ of the depth above the bed.

3. The velocity at any depth is equal to the bottom velocity increased by the ordinate of the parabola corresponding to the depth.

4. The ordinate of the parabola at $\frac{4}{9}$ of the depth above the bed is to that in the surface as 2 to 3, if the resistance of the air be disregarded.

5. The bottom velocity at any point depends on the fall, the depth, and the nature of the river bed. The value of the retardation is at least as great as the value of the ordinate of the parabola which lies in the surface.

6. In consequence of the variable bottom velocity, the relation between the bottom and mean surface velocity cannot be constant either in different verticals, or in the same vertical with different heights of water. Also the relation between the parameters of the parabolas belonging to the different velocity scales cannot be constant.

7. Such constant relations cannot be de-

termined even by making use of co-efficients, because the latter vary more or less according to the resistance of the bottom.

8. The frequent change in the form of the velocity scale, based on the variable resistance of the river bed, explains the unequal movements of water in rivers. As long as the river bed is irregular and changeable no universal law can be determined for the motion of the water.

The methods hitherto used for the calculation of the water masses of rivers, from the measured current velocities in various verticals and at different depths, lead to inaccurate results.

Water masses may be more correctly calculated by dividing the cross section of the stream by vertical lines at numerous points, and determining only the mean velocity at each point at $\frac{4}{9}$ of the depth above the river bed, from the arithmetic mean of at least three observations, and then summing up the products of the mean velocities thus found, and the separate areas of the cross section.

The original paper is illustrated by two plates: one shows the five cross sections of the Memel taken near Tilsit, and the other contains diagrams in illustration of the velocity scales obtained from the measured velocities at the above cross sections.

REPORTS OF ENGINEERING SOCIETIES.

American Society of Civil Engineers.—The last number of the Transactions contains:

Discussions on the failure of the Ashtabula Bridge, W. M. Roberts; Relative quantities of Material in bridges of different kinds and of various heights, C. E. Emery; The Flow of Water in open Channels, T. G. Ellis; A Novel Railroad Survey, T. S. Hardee.

The Mechanical Engineers at Bristol.—The summer meeting of the Institution of Mechanical Engineers was held at Bristol, and was mainly remarkable for the grave, not to say alarmist, tone of the address of the president, Mr. Thomas Hawkesley. After giving a brief account of the financial position of the institution, and of the arrangements made for its permanent settlement in Victoria Street, the President went to the war question, and spoke of the necessity for this nation to keep open the paths of the ocean in order that we may obtain our food supplies. He condemned, as others have condemned, the building of floating castles, the loss of one of which would be a costly disaster; and he recommended, as others have recommended, the construction of a fleet of light, swift, and well-engined ships, which by the exercise of a daring and hornet like activity, would succeed in driving every enemy's ship from the face of the sea. Then turning to the question of trade, he astonished his audience by stating that "we are at this time diminishing our wealth, and the means of supporting our rapidly-increasing population, by more than one hundred million pounds per annum," simply because foreign nations will not take a sufficiency of our goods to enable us to liquidate our indebtedness for food supplies. According to Mr. Hawkesley, we do not manufacture cheap enough. Labor is too dear, and workmen are at war with their employers. Wages have been unduly forced up, but not the means of living; and trades which were once monopolized have been driven away. Mr. Hawkesley contends that if the evil day is to be postponed, the cost of all commodities, labor included, must be closely assimilated in this kingdom to the cost of like commodities on the continent of Europe and in America. Mr. Bramwell, in proposing the vote of thanks, said that while he hoped some of his hearers differed from the president, he trusted they would all join in thanking Mr. Hawkesley for setting them thinking upon matters of so much importance.

The reading of the papers was then commenced by Mr. J. C. Wilson, who submitted a paper mainly descriptive of a safety-valve designed by Prof. Klotz, of Prague, which, however, did not meet with much favor from the members present, and which Mr. Lewis Olrick declared was almost identical with a valve introduced by Mr. Bodmer upwards of fifteen years ago. The discussion that followed was well sustained, and was highly interesting. Mr. Webb, of Crewe, then read a paper on a circular slide-valve, designed by him some years ago, but which has only been in use long enough to fairly test its merits. We will merely say here that the valve is made circular, and is free to revolve in its buckle, so that if there is more friction in one part than in another, the valve revolves, and the sliding faces are, in consequence, equally worn, grooving being effectually prevented. The valve is applicable to either steam or hydraulic engines, and samples which have been severely tested were exhibited, so truly plane that if the surfaces were wetted one would support another. Mr. J. G. Geach gave a description of the appliances used in constructing the heading under the Severn for the Severn Tunnel Railway, and in the afternoon the members visited several works in Bristol and the neighborhood, and attended the conversazione given in the evening by the President and Mrs. Hawkesley at the Merchant Venturers' Hall.

On the following day Mr. T. H. Riches read a paper on the Tynewydd colliery inundation, and described the operations undertaken for the release of the imprisoned miners. This paper was probably the most interesting of those read at the meeting, and it was satisfactory to learn from Mr. Riches that, after the experience gained by the Welsh miners, he thought there would in future be no difficulty in getting men out in case any accident of a similar character occurred. With reference to the air lock, for enabling the rescuers to work without danger from the rush of compressed air, Mr. Cowper suggested there ought to be a regulation that one should be kept at all collieries. Mr. Froude, F.R.S., described a new dynamometer, devised for measuring the power delivered to screw-propellers of large ships, the well-known friction brake having been found to involve greater difficulties than were anticipated, consequently necessitating the invention of some other instrument to enable Mr. Froude to carry on the investigations directed by the Admiralty. Mr. J. C. Fell read a paper on "Variable Automatic Expansion for Steam Engines," which was mainly descriptive of the working economy of Rider's gear, the promptness and efficiency of which are, according to Mr. Fell, most marked. He said that an engine at a fulling mill in the neighborhood had been fitted with the valve gear, and a crucial test of its powers was made by throwing off both mills simultaneously, when no change in the speed of the engine was perceptible to those watching it. Several papers were postponed for want of time, notably a description of improved radial axle-boxes and guides, by Mr. H. Widmark, of Bristol, and one on special mechanical appliances for use in certain classes of mine accidents, by Mr. C. Hawkesley and Mr. E. B. Martin.

Excursions were made to the Severn Tunnel works, when the powers of the drilling machine described by Mr. Geach in his paper were ocularly demonstrated to the assembled visitors, several of whom expressed the opinion that it was the best drill yet invented, and that if the present work were carried to a successful issue, it would hasten the construction of the Channel Tunnel. Several other excursions were made, and the summer meeting at Bristol will be long remembered by those members of the Institution who attended.

IRON AND STEEL NOTES.

How Siemens Steel is Made.—At the January meeting of the graduates' section of the Institution of Engineers and Shipbuilders in Scotland, Mr. Jn. Mayer, F.C.S., explained at some length the nature of the materials employed for this purpose, the chemical transformations which they undergo in the Siemens regenerative furnace, in order to obtain the different qualities of steel which are required for rails, sheets, plates, hoops, wire, &c., and he stated that in this country alone works capable of producing by this process 250,000 tons per annum are erected. Dr. Siemens lent some diagrams to illustrate the paper. The discussion was adjourned, so that members who had practical experience in using Siemens steel for engineering purposes might be present to speak.

Danks Iron.—Messrs. Hopkins, Gilkes, and Co., Limited, of the Tees Side Iron Works, Middlesbrough, have succeeded in making, with the Danks rotary puddling apparatus, homogeneous iron exclusively from Cleveland forge pig iron, and this iron will be branded " H. & Co. Danks." The makers state that it is equal in quality to South Yorkshire or best Staffordshire iron. At the Middlesbrough quarterly iron market in January they showed specimens of rails, bars, &c., which had sustained very severe tests. The rails were exceedingly good in fracture, crystalline in structure, and perfectly homogeneous, and the iron was decidedly much better than has previously been obtained from Cleveland forge iron alone. The rails and other iron are made without piling, the whole process of manufacture being followed straight through from the puddling machine to the finished product with nothing more than a wash-heat before rolling off. It will be remembered that Messrs. Hopkins, Gilkes, and Co, were the first in Europe to adopt the Danks furnace, and they exhibited one in operation immediately after the meeting of the Institute in London in 1872, since which time they have been experimenting with the machine, having had to encounter numerous mechanical difficulties. The results are now before the trade. A portion of the works was some time ago remodelled, and about a dozen furnaces erected.

Manufacture of Slag Wool.—Heretofore in the manufacture of this material the hot slag as it leaves the furnace has been subjected to the action of a jet of steam or air for the purpose of dividing it into extremely fine filaments, but the direct action of the steam has not been altogether successful in the production of a material free from impurities known generally in such manufacture as shot. Mr. Chas. Wood, of Middlesborough-on-Tees, has, therefore, devised means whereby he believes a large proportion can be made entirely free from shot, thus leaving the fibers or filaments almost pure. He conducts the slag from the furnace by the usual slag runner and at the discharge end, and on one side underneath the usual slag runner he places an air or steam jet, preferably the latter; on the other side of the runner, and opposite the steam or air jet, he provides a large tube of wrought or cast-iron leading to a chamber or receiver, to be hereafter described. The mouth of the tube—that portion of it near the runner—is open on the lowest side, so that the shot coming from the slag as the wool is divided from the same, or in other words, as it is manufactured, is free to fall to the ground or into any suitable receptacle, while the slag proper goes into an ordinary slag box, or is otherwise disposed of as desired. Into the tube and beyond that part thereof which is not open he leads a second jet for the passage of air or steam, and the object of this second pipe is that the air or steam which is forced through the same toward the chamber draws the wool or silicate cotton which has been produced by the first steam or air jet into the tube, and sends it on into the chamber; this chamber is constructed or formed of a series of frames made of wire netting for the purpose of catching the wool blown into the same, and allowing at the same time the steam or air to escape.

With regard to the arrangement of the wire netting, he finds it most convenient to have them in a V or corrugated form, and to connect to the apexes of (say) the V's what he terms draught plates, which tend to check the current of air or steam, and to allow the fine qualities of the wool to settle behind them in the angles formed by the V-shaped netting. Near the entrance of the chamber, and opposite the tube, he places a board or plate for the purpose of arresting any shot that may possibly be carried into the chamber through the tube, and thus stop the shot from contaminating the wool. A galvanized iron or other roof may be provided for the cage or chamber, but this may also be of wire netting or any other material. The invention then essentially consists in the employment in the manufacture of two air or steam jets, one to make the wool, and the other to draw it into and send it through a tube into a chamber or cage made of wire netting or perforated plates, or equivalent material, in such manner that the wool is caught by the sides, while the air or steam is able freely to escape without forming currents, and also a great side area of netting or perforations upon a comparatively small space of ground. The simple but effective means provided for arresting the shot, and dividing it from the wool, is also a very important feature in the invention. —*London Mining Journal.*

RAILWAY NOTES.

Rarchaert's Total Adherence Locomotive.—The scientific and technical portion of a late number of the *Annales des Mines,* is wholly taken up by a memoir of M. Massieu, and a short note of the inventor, upon Rarchaert's "locomotive of total adherence and converging axles." The locomotive was first tried on a circuit of 58 kilometres, during which it behaved well in all points of view, reaching a mean velocity of 40 kilometres, and sometimes exceeding 50 kilometres per hour, on slopes of .015 and in curves of 250 metres radius. After this trial, it was employed, for about two months, upon trains. M. Rarchaert finally obtained authority from the company to employ his machine on regular trains, under the charge of the company's engineers and stokers, the expenses being borne by him. This condition was so burdensome, that he terminated the trial at the end of a month. During that time the locomotive had traveled 4349 kilometres, and had satisfied all requirements without accidents. It has since been employed on the road from Orleans to Chalons. M. Massieu and his principal subordinates carefully studied the details of construction and operation, and presented two successive reports, which he was invited, by the commission of the *Annales des Mines,* to embody in a single memoir, under the following heads:

1. Brief examination of the processes hitherto employed to facilitate the passage of locomotives on curves, with the more or less complete utilization of the adherence which their total weight can give.
2. Description of the apparatus.
3. Study of the apparatus in a kinematic point of view; examination of the conditions in which it can overcome the curves and irregularities of the track.
4. Study of the apparatus dynamically; examination of the causes which can impair its stability.
5. Comparison with other locomotives employed on secondary lines; results of the trials.
6. Examination of the objections against the use of a single and straight connecting rod.
7. Summary and conclusion.

The memoir covers 200 pages; M. Rarchaert's note, 12 pages. Both terminate with the following conclusion: "For a long time, the constructors of locomotives with a single motor, have tried to separately realize, sometimes total adherence, sometimes flexibility; sacrificing, according to circumstances, one of these conditions to the other. Among those who have tried to realize them both at once, I am led to believe that M. Rarchaert is the one who has best, and even for the first time, practically succeeded."

Continuous Railway Brakes.—A valuable report was recently forwarded by the Belgian Government to the British Board of Trade, on the subject of continuous railway brakes. The Administration of State railways in that country, fully aware of the importance of obtaining the best available means for the control of trains, have since a long time given a fair trial to various systems of brakes which promised to give good results. Experiments were conducted with the Achard, Masin, Heberlein, and others, which however were thrown aside, and careful attention was given to the system which in 1873 appeared the most perfect that had yet been introduced. This was the Westinghouse atmospheric brake, which had previously been tried in this country, and which has long been working with excellent results upon the Metropolitan District Railway. The experiments with this system showed, as might have been expected, a superiority as compared with the hand brakes of about 3½ to 1. The experimental trains passed into regular service, and showed consistently their efficiency and reliability. The defects of the system were however fully appreciated by the Railway Administration, the chief one being the delay that arose in bringing the brake into action. On the other hand, they equally appreciate the advantages it possessed, and which they summarized under the following heads: 1. Comparative promptness of action. 2. The facility of gradual application, and the fact that the driver of train could control the brake. 3. The distribution of elastic pressure on all the wheels throughout the train. 4. Its applicability to every wheel in the train. 5. Its non-liability to derangement. 6. Its action being independent of the motion of the train.

Between 1873 and 1875 financial reasons prevented any further adoption of the system on the part of the Belgium Government, and in the course of that year the new form of brake perfected by Mr. Westinghouse, and known as the Automatic, was submitted to the Administration, and at the same time another system which had also been acting, brought into notice—the Smith vacuum—was submitted to the consideration of the Belgian railway authorities. It was therefore decided to disregard the various other systems which showed more or less promise, and to confine attention in the new series of experiments solely to the automatic and vacuum brakes. Accordingly two similar trains were fitted with these apparatus, and a number of carefully conducted experiments were made with them, and which are recorded in the report. The comparative results obtained corresponded very closely with those deduced from similar trials in this country, but it will be noticed that with neither brake did the stops made approach those attained on the North British Railway. It is to be regretted that the times occupied in making the stops are not recorded in the report. It will be noticed also that all the experiments were made at low speeds, so that the respective capacities of the two systems were not by any means developed as they have been in this country. Still, as will be seen in the report, the results were so decidedly in favor of the automatic brake as regards its promptness and efficiency, that the Committee reported unanimously in favor of its adoption, and acting on this view, the Administration has accepted the Automatic as the standard brake throughout all the State lines, and its application will be commenced immediately. The Committee are also unanimous in their conclusion that the system they have proved to their own satisfaction, to be so superior to the vacuum brake, that they recommend it, in spite of the greater cost, will prove far less expensive in maintenance and working than the latter, and they take pains to point out that, judging from the short experience obtained, and from the nature of the brake, that the apparent advantage in first cost is "more deceptive than real, the cost of maintenance being higher for the Smith brake than for the Westinghouse, either automatic or atmospheric;" while the quantity of steam required to work the first-named is greater than for either of the other two, and therefore the cost of each stop made is greater.

The Belgian Government in taking this prompt action in the brake question, are to be congratulated upon the selection they have made. Whatever may be the drawbacks to having the railway system of a country vested in one central Administration, such an arrangement has, at least, this advantage, that it is free from all the conflicting interests and opinion, which in England render concerted and uniform action almost, if not quite, impossible. This is illustrated by the Government publication just issued containing the correspondence between the Board of Trade and the Railway Companies Association, in which the representatives of the great lines of this country all advance conflicting statements, relative to the efficiency of various brake apparatus.

We believe that a series of trials with continuous brakes is shortly to be carried out in Germany under Government auspices, and we trust that they will be conducted in such a way as to develop to the utmost the full capacity of all the different systems competing, that is to say, that the trials should be exhaustive, and made with similar trains, consisting of at least ten or twelve carriages. So far as this latter feature is concerned, we believe it has been the intention of the railway authorities only to fit up four or five carriages on each system, but it need scarcely be pointed out that so limited an application would fail in giving anything like conclusive or satisfactory results.

ENGINEERING STRUCTURES.

THE bridge of the new Cincinnati Southern R. R., over the Kentucky river, is said to be the highest railroad bridge in the country. It is 275 feet high, having three spans, the middle one 375 feet long, and the others 300 feet each, the total length being 1,125 feet. There is a bridge in Switzerland which is 254 feet high, but with a span only 144 feet long, and one at Verrugas, in the Andes, 252 feet high, with spans 125 feet long. The piers of this Kentucky bridge are the largest in the country, except those at Brooklyn, the stone work being 130x47 feet and the base of the iron work 117x28. The frame is all wrought iron, and was built out from the abutments toward the center of each span.

THE HUDSON RIVER TUNNEL.—It is the intention of the Tunnel Company to begin work early in the coming fall. A shaft twenty-eight feet deep has been dug at the foot of Fifteenth Street, Jersey City, and this depth will be increased twenty feet. From this as a starting point, the tunnel will proceed in a northeasterly direction under the Hudson River and the Christopher Street ferry slip. The entrance on the New York side will be in the neighborhood of Washington square. From Jersey City the grade will descend two feet in every hundred feet, until a point 2,700 feet from the New York side is reached, when it will begin to ascend at the rate of one foot in every hundred feet. The tunnel will be two miles in length, with a road-bed twenty-three feet wide, and two separate tracks. Through its entire length it will be lighted by gas. The wall will be constructed of brick, with a thickness of four feet. At no point will the top of the tunnel be less than thirty-five feet below the surface of the water, and in many places it will be seventy feet below. One hundred and twenty laborers will be engaged in the construction of the tunnel. The work will go on during the whole of the twenty-four hours, the force working in three relays, for eight hours each. Although the tunnel will be used for the conveyance of passengers, its main object will be the transportation of freight to and from the great railroad lines which terminate in Jersey City. The capital of the company is $10,000,000.—*Iron Age.*

THE CHANNEL TUNNEL.—The engineers of the Channel Tunnel—MM. Polier and Lapparent—in a report upon the result of the numerous soundings which they have taken during the last eighteen months for the purpose of ascertaining the thickness and impermeability of the different strata in the Channel, state that "observation shows that the soil under the sea preserves the same characteristic as upon the French mainland—that is to say, a line traced upon the surface of one of the beds of chalk consists of long straight lines connected with one another with very pronounced sinuosities. Observation has also shown—and this is a capital point with regard to the making of the tunnel—that in these sinuosities, of which there are two in the Channel, the strata are evidently continuous, and that the distance between the two successive lines is filled up, not by a fissure, but simply by a curve. There is, therefore, no reason for anticipating that the work will encounter any geological difficulties, properly so called, if we bear in mind the information furnished by a study of the sea's bed and by the work done in the mines which run down into the chalk. The boring at Sangatte has not been carried far enough to admit of any definite opinion being given as to the jurassical development of the soil in that region, but it has confirmed the deductions drawn as to the progressive diminution in thickness of the strata below the chalk which runs from Boulogne towards Calais. These strata are formed of sand and clay, with an admixture of palæozoic pebbles, red calcareous and carboniferous marl, but without any rock of the coal order."

The Wrought Iron Bridge Co., of Canton, Ohio, has the contract for building the bridge over the Connecticut river, at Northampton, Mass. The bridge has a total length of 1219 feet in eight spans with 18 feet roadway, and will cost about $27,000, exclusive of flooring. This company is also building a 930 feet bridge in six spans, with 16 feet roadway, at Columbus Junction, Iowa, being the longest highway bridge in the State, and has the contract for six 120 feet spans, with 18 feet roadway, and 5 feet sidewalk at Paris, Ont. They have just completed a 160 feet bridge, with 30 feet roadway and two 8 feet walks, on iron piers 25 feet high, at San Jose, Cal., and are building a 256 feet span, with 18 feet roadway, at Preston, W. Virginia; all of the above bridges being on the Company's patented truss plans with all wrought iron details. The Company have now over 12,000 feet of bridging in process of construction, and are running their works day and night, giving employment to over 300 men, and are making extensive additions to their shops and machinery to meet their increase of business.

ORDNANCE AND NAVAL.

Mr. Brassey on Gunboats.—The following letter has been addressed by Mr. Brassey to the editor of the *Times*:—

Sir,—It may not be uninteresting to many of your readers to know that the two gun vessels recently constructed by Sir William Armstrong's firm for the Chinese Government were at Aden on the 16th of April, having performed their long voyage from England satisfactorily.

It has been urged most strongly by Mr. Barnaby that every monster ironclad should be supported by a flotilla of gunboats. Has due weight been given to this suggestion of the Chief Constructor, which received the strongest support from Sir Spencer Robinson and other authorities?

As skirmishers, in combination with the monster ironclads of the Inflexible type, gunboats of the type designed by Mr. Rendel would be invaluable. In addition to the attendant flotilla of gunboats, each armored ship should carry torpedo boats of great speed. When it is remembered that the blow from a single torpedo would prove fatal, and how improbable it is that such tiny craft, steaming at the rate of 17 knots, and enveloped in smoke, will be struck by heavy projectiles, it can scarcely be doubted that victory would incline in favour of that fleet which would possess the greatest number and the most effective of these light armed naval skirmishers.

Again, considering how impossible it is to construct an invulnerable ship, and that the costliest ships are almost as liable to destruction as those of a smaller and less costly type, ought it not to be a cardinal maxim with naval constructors and administrators to distribute the strength of the navy into as large a number of ships as may be, taking care, of course, that no ship shall be built which is too small to be thoroughly effective in its own particular class? Let us seek for the best practical application of this principle.

The expediency of adding largely to the dimensions of an armored steam ram, for the purposes of mounting an armament of two or four 80-ton guns is open to question on another ground. A captain may hesitate to open fire, and to obstruct his field of view by the smoke of a heavy cannonade, when he sees a cloud of torpedo boats hovering round him, only deferring their fatal assault until their movements are rendered invisible to the enemy by the smoke from his own guns.

How, then, are we to meet these various and complicated conditions with which the naval constructor has to deal? It can only be done by adopting distinct types of ships for the use of the gun, the ram, and the torpedo respectively.

The artillery of the fleet should be mounted on floating gun carriages of the Gamma type, designed by Mr. Rendel. For the torpedo each large ship should carry two or more of Mr. Thorneycroft's swift launches. For the purposes of ramming, a swift armored ship of handy proportions is required; and, with the view to a limitation of size, and in order to secure that quality of handiness so vitally necessary to an effective ram, these vessels should not be encumbered with armor-protected guns.

Let progressive, unprejudiced naval officers compare the kind of force that could be created for a given sum of money, if constituted, according to the suggestions here offered, with a fleet composed of vessels of the Inflexible type. I assume that armored vessels can be built for £50 a ton. The Inflexible type, in round figures, has a tonnage of 10,000 tons, and costs £500,000. Five millions sterling, therefore, would produce only ten Inflexibles, which, powerful as they are, possess no special defence against the torpedo, are armored with penetrable armor, and together carry only forty guns. A fleet of

Inflexibles, it will be remarked, costs £125,000 a gun.

I venture to believe that a like sum of £5 000,000 might be much more effectively applied in the construction of the following vessels :—

1—Thirty armored steam rams of 2000 tons without guns, costing, at £50 a ton, each £100,000..................	£3,000,000
2—Sixty gun vessels of the Gamma type, armed with one 38-ton gun, two 12-pounder breech-loading guns, and one Gatling gun. Each £25,000.........	1,500,000
3—Launches on Mr. Thorneycroft's plan, and other descriptions of offensive torpedoes and torpedo boats....................	500,000
	£5,000,000

For operations in European waters, in the Mediterranean, the Red Sea, the Straits of Singapore, in short, along the whole line of our communications with the East, such a fleet as I have indicated would, in the hands of dashing commanders, be more effective than ten Inflexibiles. Is the ram, as some think, the most formidable weapon of the navy, and is the quality of handiness the first condition of efficiency? Then thirty rams are matched against ten, and the smaller vessels, being more handy, are the more effective. True it is that the Inflexibles could steam faster than the smaller rams, and could therefore place themselves beyond their reach ; but such powerful vessels were surely not built to run away from an enemy.

Is the gun the weapon on which we can rely ? In the plan suggested eighty guns are carried as against forty ; and they are, for many purposes, mounted, so as to be more available than if carried in ships of the Inflexible type. For bombardments and for coast defence, a shallow draught is often essential in the vessels employed. It may be necessary that they should pass over shoals or navigate shallow channels. St. Petersburg and Cronstadt were inaccessible to our line-of-battle ships. They might have been destroyed by gunboats. Take, again, the question of coast defence. What could be done to protect the Thames, the Mersey, or the Humber, by ships drawing 30ft., in comparison with the services which could be rendered by gunboats capable of navigating the in-shore channels and threading their way through the intricate maze of mud flats and sand banks with which the estuaries of our largest rivers are beset ?

In a naval engagement the utility of gunboats would be scarcely less apparent. Having double the number of guns, the chance of delivering a fatal shot is largely in favor of the fleet supported by a flotilla of gunboats. It may be thought that the gunboats could not sail in company with the larger ships. The plan of towing would meet this difficulty. For an example of what may be done with torpedo boats the recent experiments at Cherbourg are conclusive.

Were half the money now so lavishly, and I fear vainly, spent on unwieldy and no longer indestructible or impenetrable ironclads, employed in procuring for the navy the means of destroying them, the naval administrations of other Powers would abandon the construction of armored vessels. They would acknowledge the hopelessness of the attempt to give protection to their fleets by means of armor against the cloud of gunboats, torpedo boats, and rams which we could bring together against them. Thomas Brassey.

BOOK NOTICES.

Science Series, No. 31.

The Sanitary Condition of City and Country Dwelling Houses. By George Waring Jr. New York: D. Van Nostrand. Price 50 cts.

This subject is only beginning to engage the public attention to the extent to which its importance entitles it. It is fast gaining its true position however. The best authorities tell us that the time is not far distant when every case of typhoid fever or diphtheria, will be a matter for official investigation in all of our large towns. The author of this essay has put in condensed form the leading principles of Sanitary Science, and their practical application to city and country dwellings, and has thereby made the best contribution we have yet seen to the cause of public education, inasmuch as in so small a compass he has presented quite clearly and completely the most advanced views.

Fowne's Manual of Chemistry, Theoretical and Practical. Twelfth Edition revised and corrected. By Henry Watts, B.A., F.R.S. London: J. & A. Churchill. For sale by D. Van Nostrand. Vol. I. Price $4.25.

This work has long been considered one of the best books for reference in general chemistry in the English language. Without the ponderous dimensions of the three or four larger works it contained all that was needful for the student, and in general satisfied the wants of the instructor. It had reached its third edition in 1850, and the tenth in 1868.

The additions necessary to fully present the department of Organic Chemistry were so many, that in the present edition the work appears in two volumes. The first including Chemical Physics and Inorganic Chemistry, and the second Organic Chemistry only.

The usual tables and spectrum chart are found in the first volume.

Traite D'Astronomie et de Meteorologie Appliquees a la Navigation. Par G. Chabirand et L. Brault. Tome I, Astronomie. Paris: Arthur Bertrand. For sale by D. Van Nostrand. Price $4.00.

This first volume, which is the only one yet received, is divided into four books. The first treats of the form of the earth and the different methods of mapping its surface, concluding with a statement of the general problem of determining a ship's place at sea.

Book Second is devoted to Spherical Trigonometry, Spherical Astronomy, Methods of calculation, and the reduction of observations.

Book Third contains Movements of Celestial bodies; Measure of Time, Astronomical Ephemeridæ.

Book Fourth deals with practical observations and the necessary reductions.

The whole forming a very complete practical Astronomy for students.

It is a beautifully printed royal octavo volume of 460 pages.

SCEPTICISM IN GEOLOGY AND THE REASONS FOR IT. London: John Murray. For sale by D. Van Nostrand. Price $3.00.

The author who assumes the name of Verifier, finds difficulty in following the line of argument of modern geologists, so he refuses to accept the modern theories.

From such reading as we could give the book, we concluded that the author, whoever he is, has given less information in regard to the basis of modern geological science, than about his own peculiar mental constitution.

In the preface we find this clause: "If the book should attract any attention, it is sure to be met in certain quarters with rough usage." We feel confident that no "rough usage" will be inflicted.

A TREATISE ON CHEMISTRY. By H. E. ROSCOE, F.R.S., AND C. SCHORLEMMER, F.R.S. Vol. I. London: Macmillan & Co. For sale by D. Van Nostrand. Price $10.50.

When we state that this volume of 770 pages royal octavo size is devoted to non-metallic elements only, the reader will doubtless conclude with us that this is the first supply of an extensive treatise.

The more important chemical processes relating to non-metals are fully described, and the apparatus illustrated with the best of wood cuts.

We are assured in the preface that care has been taken to present the most recent, exact and experimental data. The names of the authors are a sufficient guaranty of scientific exactness. No one can take exception to the style in which the publishers have put the treatise before the public.

PERMANENT WAY ROLLING STOCK AND TECHNICAL WORKING OF RAILWAYS. By CH. COUCHE, Inspector-General of Mines, Professor of Railway and General Construction at the School of Mines, Paris. Translated from the French, by JAMES N. SCHOOLBRED, B.A. London: Dulau & Co. Paris: Dunod. For sale by D. Van Nostrand. Vol. I. Price $20.00.

The following is the translator's preface:

"It is but an act of justice to the distinguished author of this work to say, that some years have elapsed since the translation of the following pages were first undertaken. Various circumstances, over which the author had no control, have led to the delay in the appearance of the English edition; and, in consequence, many of the facts scattered through its pages may now appear somewhat out of date, though when originally written they were fresh and full of interest.

"The author has kindly endeavored to remedy somewhat this fault of the translator's by a few notes in the Supplementary Chapter; and also by some remarks there on 'Steel rails,' which have now almost superseded iron ones, though still in considerable use when the pages were first written.

"In order to render this edition of further interest to English readers, the translator has added, in the body of the work, some details, as to the arrangement of the many lines of rail which communicate with each other at Clapham Junction, and also as to a few of the earlier forms of the interlocking of points and signals; and, in the supplementary remarks on Steel rails, he has ventured to insert a few of the interesting facts and opinions which have been of late elicited on the subject of this metal, now become a paramount importance in connection with railways."

The following is the table of contents:

BOOK I, PERMANENT WAY.—Chapter I.—Breadth of Guage; II.—Form of Rails on Non-Continuous Supports; III.—Establishment of Continuity at the Joints by means of Fish-Plates Strains on the Metal in the Rails, Fish-Plates, and Bolts; IV.—Rails on Longitudinal Bearers; V.—Cross Sleepers; VI.—Ballast, Conditions which it should fulfill, Provisional Ballasting of a Line; VII.—Stone Blocks as Sleepers; VIII.—Systems of Metallic Permanent Way; IX.—Special Points in the Permanent Way; X.—Turntables, and Traversers for Carriage Sheds, and for the Open Line; XI.—Manufacture and Delivery of Rails; XII.—Drainage of the Line, Amelioration of Cuttings and Embankments.

The work is to be completed in four volumes, royal quarto.

The first volume which is the only one yet ready is accompanied by a folio atlas of 38 plates.

THE MAGNETISM OF IRON VESSELS, WITH A SHORT TREATISE ON TERRESTRIAL MAGNETISM. By FAIRMAN ROGERS, Member of the Compass Commission of the National Academy of Sciences. New York: D. Van Nostrand. Price 50 cts.

Several years since much interesting discussion took place in the *Mining Journal* concerning the magnetic action of iron in the vicinity of compasses employed in instruments of precision, the great interest in the subject felt by miners arising from the importance of knowledge of this kind to secure accuracy in the underground surveys. Facts in connection with magnetism, which are of interest to those engaged in navigation, are, therefore, of equal interest to miners and surveyors; and a great mass of these facts have been brought together in Mr. Fairman Rogers' treatise on Terrestrial Magnetism and the Magnetism of Iron Vessels, recently reprinted from VAN NOSTRAND'S ENGINEERING MAGAZINE, as one of his Science Series. Mr. Rogers points out that the class for which he writes are interested in terrestrial magnetism, on account of the magnetic needle and its variations with the magnetic action of the iron in the surrounding machinery. The simple experiment of floating a magnetic needle upon water shows by its assuming the usual direction, without moving towards the northern boundary of the vessel containing it, that the force acting upon the needle is a directive force, not one of attraction for the needle as a whole. Careful experiments have also led to the inference that the compass needle points to the north in obedience to the law which causes it to take a position at right angles to currents which are passing around the earth in a direction nearly parallel to the equator, and from west to east.

To whatever causes we may attribute the action of the magnetic needle, a large number of observations are required to furnish the data upon which the true theory is finally to be founded, and the numerical value which will serve as a basis for calculation. These observations are at present directed to the direction of the needle at the different parts of the earth's surface with reference to the true meridians or geodetic north pole and to the dip of the needle, or the direction with reference to a horizontal plane which a needle accurately balanced before being magnetised assumes after being magnetised. In both these cases the amount of the directive force as well as the position assumed is to be measured. The force acting upon the needle in the direction of the dip at any given place is called the total force of the earth at that place. In other words, the force acting upon the compass needle is not simply one in a horizontal plane directing the needle towards a magnetic pole, but it is a force acting in an inclined direction, which we divide for practical purposes and for convenience of observation and consideration, by a well-known device in mechanics, into a horizontal component and a vertical component. The direction of the horizontal component can be readily observed by comparing the direction of the needle with that of the true meridian obtained by observations upon the north star or the sun, or by any of the means well known to seamen. The deviation from the true north line, or the variation of the compass, differs in different localities and at different periods. The diurnal variation is too small to be of practical importance to the navigator. The variation is of the utmost importance, as in some localities ordinarily visited by the navigator it is as great as 50° to 60°, or four or five points to the west, so that when heading due north magnetic the ship will be sailing N.E., or N.E. by E. Assuming similar variations to occur on land as at sea, miners will appreciate more than ever the importance of all plans of mining and engineering works being laid down to the true north, and as Mr. Rogers' object has been to enable all concerned to allow for the deviation or to correct the compasses, so as to eliminate it, the value of his treatise will readily be appreciated.

Originality of assertions in a treatise of this character would render it worthless, yet the modes of stating facts are so numerous that there is frequently as much newness in technical as in other works, and the value of the former depends much on the clearness with which the facts are stated. Mr. Rogers does not pretend to give any material not previously published, but he supplies such information as will enable an officer, previously unacquainted with the subject, to undertake a series of observations which would be of value in adding to the general knowledge of the matter, or in studying his own ship, so as to avoid mishaps from a too firm reliance upon uncorrected compasses, or from unexpected changes in new magnetic latitudes. With regard to arrangement, there is an entirely new table which conveys as much information in a few minutes as would otherwise require many hours' reading to get together, and then would not be nearly so well impressed upon the memory. To navigators the treatise will prove invaluable (indeed it was originally written for a Manual of Scientific Enquiry, which the Navy Department proposed to publish, though the intention was never carried out), whilst mining officers will find a vast amount of information which it will be to their material advantage to possess; it may be hoped, therefore, that the volume will be widely read.—*London Mining Journal.*

MISCELLANEOUS.

Power of Electric Light.—Late experiments at St. Petersburgh show that the power of the light may be increased by covering the carbon with a thin sheet of copper, and turning the cup towards the object to be illuminated. The most economical machine tried was that of Alteneck, which, with a galvanized carbon of 10 mm. diameter, gave a maximum of 20,275, and a mean of 14,039 candles. The light was sufficient to make objects visible, for military purposes, at a distance of 3080 yards. —*Nature.*

Defences of Victoria.—A Royal Commission appointed to consider the question of the defences of Victoria has recommended that the strength of the Naval Reserve formed by the colony should be increased with a proper complement of officers to 300 men, and that the men should be instructed in garrison drill and in the work of laying torpedoes, in addition to their duties on board ship. It is also suggested by the commissioners that a supply of material for stationary torpedoes should be procured without delay.

Davyum: A New Metal.—Serge Kern announces his discovery in June last, of a new platinoid metal which he calls *davyum*, in honor of Sir Humphrey Davy. It is hard, silvery in lustre, malleable at red heat, readily soluble in aqua-regia and very feebly in boiling sulphuric acid, yieding a yellow precipitate with caustic potash. Sulphureted hydrogen, passed through a dilute solution of the chloride, yields a brown precipitate which becomes black upon drying. Potassic sulphocyanide, with the same solution, is colored red, and if the solution of davyum in KCyS is concentrated, a red precipitate is obtained. Sp. gr. 9.385 at 25° C. Kern thinks that in Mendelejeff's proposed classification of the elements, davyum is the hypothetical element placed between molybdenum and ruthenium, in which case its equivalent should be 100. It would then rank as the second confirmation of Mendelejeff's predictions, gallium having been the first. It is probably rare. The platiniferous sand does not contain more than 0.0045 of davyum.—*Comptes Rendus.*

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NO. CVII.—NOVEMBER, 1877.—VOL. XVII.

GRAPHICAL DETERMINATION OF STRAINS IN TRUSSES WITH PARALLEL CHORDS.

By CHAS. H. TUTTON.

Written for Van Nostrand's Magazine.

Believing the following to be a simpler method of finding the strains in Trusses with Parallel Chords graphically than any which has fallen under the writer's observation, it is here presented to the public, in the hope that it may do its mite of good. It will be presupposed that the reader is familiar with the principle of the lever, and the "triangle and polygon of forces." Sufficient extension of the latter for our purpose, will be found in Prof. DuBois' article in Van Nostrand's Magazine for Feb., 1875.

We will consider two cases.

1st. Pratt truss with vertical ends.

Let CDEA Fig. 1 represent a Pratt truss with vertical ends. For the purpose of allowing our results to be verified by calculation, we will suppose it to consist of 8 panels of 10 feet each, having a moving load of 8 tons, and a dead load of 2 tons per panel per truss. Height of truss 10 feet. Scale of tons and feet, 20 to an inch. Heavy lines indicate compression, light lines tension, and dotted lines those of construction.

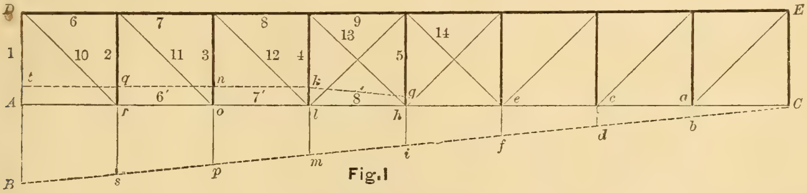


Fig. 1

A. Maximum strains in chords.

We will assume the fact as already proven, that the maximum chord strains occur when the truss is fully loaded. To find these strains, erect an indefinite perpendicular to the horizontal line, OZ, and on it lay off the reaction of the left abutment, OL, which, as the load is symmetrical with respect to the center of the truss, will be one-half of the weight of the truss and its load, or $8 \times 10 \div 2 = 40$ tons. Since one half of one panel weight is supported immediately by the abutment, and therefore causes no strain on the truss, lay down from L to M this half panel weight, to the scale of weights equal to 5 tons, and then consecutively, the equal panel weights of 10 tons=MN, NP, PR to O, for the "force polygon" (See Du Bois' article before referred to). RO then becomes but 5 tons or one half panel weight, and this is as it should be, since the load at *h* (Fig. 1) is equally divided between the two abutments, being at the center of the truss. The reaction of the abutment, less the half panel weight immediately supported by it, is evidently the compressive strain on post 1. This strain is in equilibrium with the strains through brace 10, and top chord 6, the three pieces which meet at the point B, hence from M draw a parallel to brace 10, (Fig. 1), and limit it by the horizontal through O, and we find 6 as the compression in the top chord, and 10 as the tensile strain on the corresponding brace, caused by an uniform load on the truss. Passing to *a* (Fig 1), we find this latter strain in equilibrium with the strains through bottom chord $6'$ and post 2, but we know that one panel weight of the load does not affect post 2, therefore we draw, in Fig. 2 the vertical 2 parallel to post 2, and limit it by a line through N parallel to the bottom chord of the truss, or horizontal. This gives us $6'$ as tension in the lower chord and 2 as compression on post 2. Continue these operations until the diagram is completed. It can be checked by a similar construction from the lower side.

We have then for maximum compression on top chord, on 6, Fig. $1 = 6$ or OX Fig. 2; on 7 Fig. $1 = 7$ or OY, Fig 2; on $8 = 8$ or OZ; on $9 = 9$ or OM$'$. For maximum tension on lower chord, on $Ar = Ca = 0$ since there is no horizontal through M; on $6' = 6'$; on $7' = 7'$; on $8' = 8'$, and on pin connection at the center of the truss $= 9' = $ OM.$'$

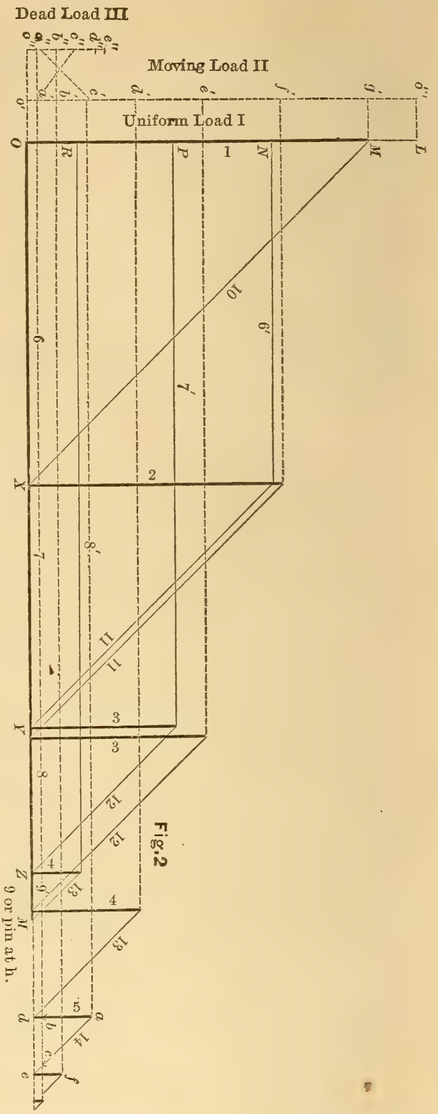


Fig. 2

B. ROLLING LOAD, MAXIMUM POST AND BRACE STRAINS.

Suppose the bridge entirely covered, and then that the load moves off to the right. It is self evident that with the bridge covered, the strains on post 1, brace 10, and top chord 6, will be the same as before, since the reaction of the abutment is the same, hence, as before, on the second vertical marked "running load" in Fig. 2, lay of $0'0'''$ equal to reaction of abutment $= 40$ tons; deduct $0'''g'$ equal to an half panel weight of total load or 5 tons, and draw 10, limiting it as before. (All of these distances should be laid off on the first vertical, hence they are all projected on it. They are laid off on different verticals here to avoid confusion). Now in Fig. 1, lay off AB equal to one panel weight of moving load or 8 tons, and draw CB, then will the ordinates at the panel points show the portion of the panel load sent to the left abutment from that point. Also, lay off $At =$ bridge panel weight or 2 tons, and draw *tk* parallel to AC. At the middle point of this (even paneled) truss, one-half of the dead load goes to each abutment, as is evident from the chord strain diagram, therefore, lay off at the center, $hg=$ one half a panel load of dead weight or 1 ton. Connect k and g, then will the ordinates between this line and the bottom chord, represent the dead load sent to the left abutment from each panel point. As the live load runs off to the right, the reaction of the left abutment will be diminished by qs, np, km, gi, ef, cd and ab successively, hence from g', Fig. 2, lay down successively $g'f'=qs$; $f'e'=np$; $e'd'=km$; $d'c'=gi$; $c'b'=ef$; $b'a'=cd$; $a'o'=ab$, which should close at o'. Through these points draw horizontals, and complete the diagram similarly to that for an uniform load. This gives the maximum strains on braces and verticals.

C. COUNTER BRACES.

Certain braces found by the above method to have strains indicated on them are unnecessary, as the dead weight of the bridge, acting through the main braces and in opposition to these, will more than counterbalance them, since their strain would but tend to relieve the main brace of some of its strain. To determine then, what counters are unnecessary, and, when necessary, what portion of their strain is relieved by the action of the main braces, erect a third perpendicular $o''e''$, equal to the reaction of the abutment due to dead weight only, or $2\times8\div2=8$ tons; then lay off the $\frac{1}{2}$ panel weight $e''d''=1$ ton, immediately supported by the abutment, and thence the consecutive panel weights of two tons, similar to that for uniform loading. By referring to Fig. 1, we see that the strain through brace 10 has no counter strain, no portion of the load held immediately by abutment A going to the right. That through brace 11 is countered by strain through brace corresponding to machine $o'a'$ in moving load diagram. That through brace 12 by $o'b'$; through 13 by $o'c'$; or, referring simply to abutment reactions, $o''c''$ counters $o'a'$, and hence is connected with it on the diagram. But $o''c''$ is greater than $o'a'$, hence the counter strain only relieves the dead weight strain, and no counter trace is necessary. Similarly, $o''b''=o'b'$, or the two strains neutralize each other in the diagram at f. Again $o''a''$ is less than $o'c'$, hence the counter strain is but partially relieved by the main brace strain on 13; so, drawing the horizontal $a''a'bc$, we find the strains ce and bd due to dead weight, acting in opposition to strains ae and ad of post and counter brace caused by moving load, thus leaving the maximum final strains on post 5 and counter 14 indicated by ab and ac. There being no other strains traveling to the right from dead weight, all other strains are correctly indicated by the diagram for moving load.

The diagram may be checked by a corresponding construction from the lower side.

2D. LINVILLE TRUSS WITH VERTICAL ENDS.

Let us take the worst possible combination, a truss having an odd number of panels, with an odd number on either side of the center panel.

This truss can be divided into two simple systems, the system 1 being shown in Fig. 5. The panel load signifies one panel load of the double truss.

We will take this truss 10 feet high, and 55 feet long, divided into 11 panels of 5 feet each (Fig. 4). Loads and scale as before. The simple system shown there, gives 5 panels 10 feet long, with an end panel 5 feet long.

The load is now eccentric and the reactions of the abutments will no longer be alike.

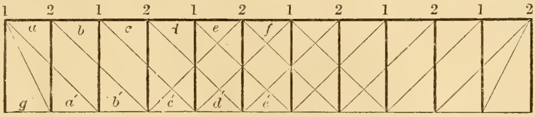


Fig.4

A. TRUSS UNIFORMLY LOADED. MAXIMUM CHORD STRAINS.

We will assume that the half panel load, which is immediately supported by the abutment, is at the end having the short panel, hence, each truss will hold an equal load, symmetrical in opposite directions, or in other words, if we change one system, end for end, we have the other.

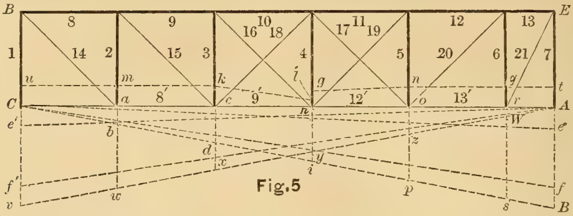


Fig.5

The reactions of the abutments may be found thus: At A, Fig. 5, lay off AB =one panel weight of combined live and dead load, and draw CB. The reaction of the right abutment (exclusive of the half panel load, or 5 tons immediately supported by it) will then be equal to $ab+cd+hi+op+rs$. Similarly for the left abutment, lay down Cv=CB, and its reaction will be $r\mathrm{W}+oz+hy+cx+aw$. Erect the vertical LM (Fig. 6), and lay off on it OM equal to the reaction of the right abutment, and OL equal to the reaction of the left abutment$=(2+4+6+8+10)\frac{10}{11}+5=32.27$ tons. TL will then be the half panel load, or 5 tons immediately supported, and ML should just equal the total weight of the system or 55 tons. One therefore equals $(1+3+5+7+9)\frac{10}{11}=22.73$ tons, which added to 32.27 gives 55 tons. (Since the truss is not symmetrical, the strains will not be so, hence we must construct the diagram for each end of the truss, as explained in the Pratt and shown in the figure. As a check, the strains in the chords should be equal at the panel point which send part of its load each way, or 10=11). From M and T lay off the regular panel weights = 10 tons, then construct the diagram as before explained.

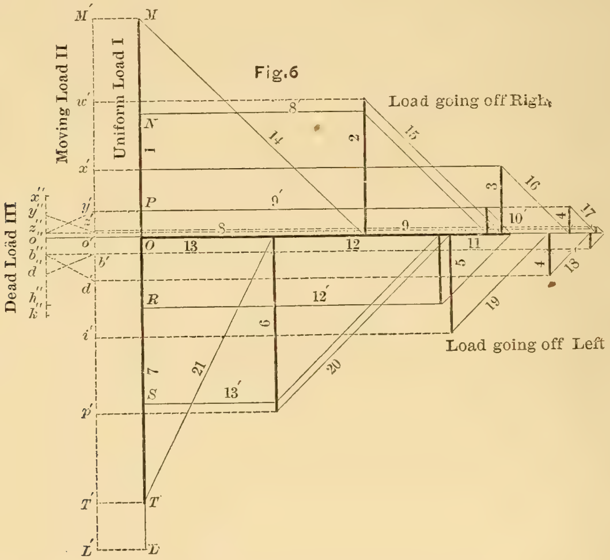


Fig.6

This gives the chord strains in one system. The systems being similar, their strains will be similar but from the opposite end. Thus from Figs. 4 and 6. Strain in $g=o$; in $a'=13'$; in $b'=13'+8'$; in $c'=12'+8'$; in $d'=12'+9'$; in $e'=11+9'$, and similarly for the top chord.

B. ROLLING LOAD, MAXIMUM POST AND BRACE STRAINS.

The strains from a moving load will also be different for loads going off in different directions.

As before, lay off Af and Cf' (Fig. 5,) =one live panel load, or 8 tons, and draw the lines Af' and Cf. Also lay off $At=Cu$=one dead panel load, or 2 tons, and draw wk and tu. Then hg and hl may be calculated or found graphically.

Since the dead load is just one-fifth of the combined load, the reactions will be one-fifth as much, or 6.454 and 4.546 tons, whence $hl=4.546-4=.546$ tons, and $hg=6.454-5=1.454$ tons, the one ton being supported at the abutment. Graphically, since we know the reactions of the abutments for an uniform load, and know that they can be no greater than that, lay off M′O′ and O′T′ exactly as for an uniform load. Then from M′ lay off $M'w'=$ordinate included between Af' and AC at a (Fig. 5)$+am$; then $w'x'=$ordinate between same lines at $c+ck$. Then from O′ lay off ordinate between same lines at r; $z'y'=$ordinate at o; then should $x'y'$ equal ordinate at $h+hl$, if work is correct. Or, draw the lines Ae', Ce, when $Ae=Ce'=$ one panel load of dead weight or 2 tons, and construct reactions of abutments exactly as for an uniform load, and as is done on vertical 3 for dead load in Fig. 6, then will $o''z''=lh$ and $o''b''=hg$, $lh+hg$ should just equal one panel load of dead $\text{weight}=z''b''$. Similarly divide up O′L′ from the other reaction, and complete the diagram exactly as before, and we obtain the maximum post and brace strains with load coming on from either end.

C. COUNTER BRACES.

Lay off the dead load reaction on vertical 3 as before explained, then we find $o''y''$ countering $o'z'$ and no brace is needed, $o''z''$ counters $o'y'$ hence counter 17 is needed with strain as given between arrow heads and point when 17 and 4 meet. Similarly for post 4.

On the lower side, the load moving off in the other direction, we find $o''d''$ counters $o'b'$ or no brace is needed, $o''b''$ counters $o'd'$ or counter 18 is needed. also post 4.

The proper value to give to the strain on post 4 is evidently taken from that side of the diagram on which the strain is heaviest.

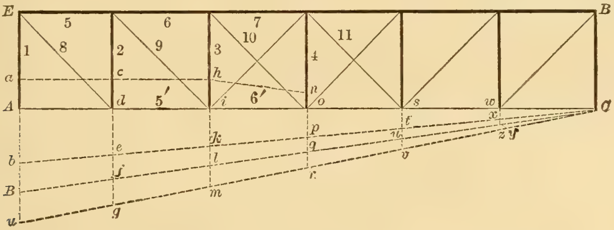
Strains caused by an excess of load in any panel or panels, such as those of a train headed by an engine.

1ST. PRATT TRUSS WITH VERTICAL ENDS.

Let ACBE (Fig. 7), be a Pratt truss with vertical ends, having 6 panels 10×10. Loading per truss per panel as follows: Dead weight of bridge 3 tons; Uniform live load 6 tons; Excess of engine weight on two panels, 3 tons per panel, or total weight of engine 9 tons per panel. Scales as before.

In this case the reaction of the abutments will no longer be alike, and our main problem is to find them.

Fig.7



A. TRUSS COVERED WITH LOAD, MAXIMUM CHORD STRAINS.

Suppose the excess of weight to be that due to an engine or engines leading the train, and covering n panels, the whole number of panels in the truss being N.

In this case, therefore, $N=6$ and $n=2$. Suppose the train is standing on the truss in such a manner that the last brace, 8 in example, receives the full load of engine excess, and yet so that the excess throws no direct weight upon the abutment A, the panel points d and i will then be loaded with engine excess in addition to the weight acting at all the other panel points. Now when the truss is fully loaded, lay off on the vertical I (Fig. 8), $OL=$reaction of abutment A. To find this reaction, lay down AB (Fig. 7)=one panel load of combined uniform and dead load or 9 tons, and $Bu=$excess of engine weight over moving load of uniform density in one panel or 3 tons, and draw BC and Cu. The reaction OL will then be equal to one half the weight of truss and uniform load $+\frac{ne}{2N}(2N-n-1)$ less the one half

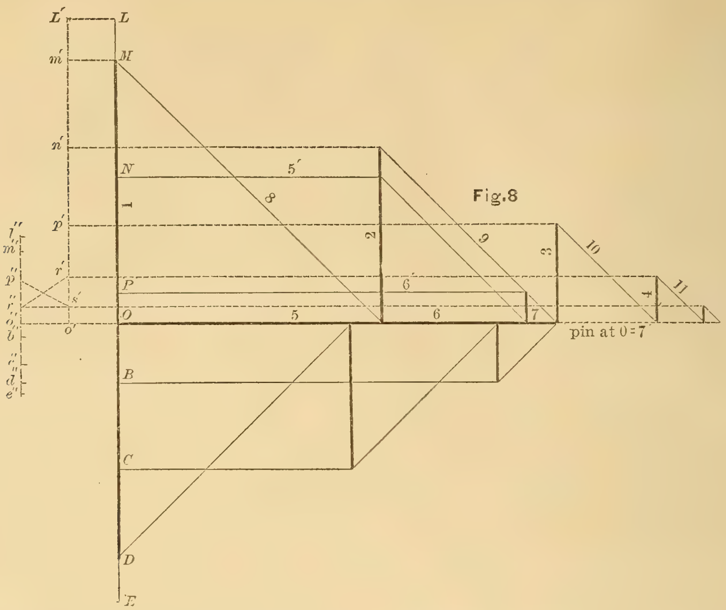


Fig. 8

panel load supported at the abutment of combined live (uniform only) and dead load. This on the supposition before stated that the engine throws no weight directly on A to be then supported. Formulating the above, let N=number of panels in the truss,

W=weight per panel of dead and uniform load, e=excess of engine weight over uniform load per panel, n=number of panels covered by engines; then, if R =the reaction at A and R′ that at C causing strains in the truss,

$$R=OM=\tfrac{1}{2}W(N-1)+\frac{ne}{2N}(2N-n-1)$$

$$=\text{for this case 27 tons,}$$

$$R'=OD=W(NI)+ne-R'=24 \text{ tons.}$$

since $W=9$, $N=6$, $n=2$ and $e=3$.

The total reactions OL and OE are respectively ½W greater.

Graphically, the reaction at A=OL =½NW+gf if only one panel is excessively loaded; if two are so loaded=½NW +$gf+ml$; if then=½NW+$gf+ml+gr$, &c. Also OE=EL−OL=(total load of all kinds=NW+ne)−(OL=reaction of the other abutment). Graphically for this particular case, OL=½W+$df+il$ +$og+su+wy+fg+lm$, and OE = ½W +$df+il+og+su+wy+yz+uv$. Having thus found OL and OE, we lay off LM=DE=½W=½ panel weight of uniform and dead load, then lay off MN =NP=W+e=12 tons, and generally lay off W+e uniformly for n times, that is, as many times as there are panels covered by the excessive load; and then the uniform panel loads = W = PB =BC=CD=9 tons, which should close at D. Then complete the diagram exactly as before explained, and we get the maximum chord strains.

The truss of course will be proportioned symmetrically to allow for the load coming on from either end, hence, the lower half of the diagram is only useful as a check.

B. ROLLING LOAD, MAXIMUM POST AND BRACE STRAINS.

O′L′ and L′M′ are found and laid off exactly as in the case of total loading in the last paragraph. Lay off AB in Fig. 7=1 panel load of uniform rolling load or 6 tons and draw Cb. Now as the engine and train backs off towards the right, as the engine passes each panel point it will lessen the reaction of the left abutment by an amount which equals $\frac{ne}{N}$ for each point passed, until the engine gets to the nth post counting from the other abutment, besides the amount of the uniform and dead load. We see this from the fact that if the excess is on d and i, then $fg+lm$ goes to the left, but if moved to

i and o, $lm+qr$ only goes to the left. Their difference $fg+lm-(lm+qr)=fg-qr=\frac{2e}{\mathrm{N}}$ or, as we have supposed $n=2=\frac{ne}{\mathrm{N}}$. Hence, lay down from M′, $\mathrm{M}'n'=cd+de+uv$ ($uv=\frac{ne}{\mathrm{N}}$ in this case), $n'p'=hi+ik+uv$; $p'r'=no+oq+uv$; $r's'=st+uv$; but here, at the next panel point, a part of the excessive load has passed off the bridge, and the part remaining throws but yz to the left. Hence lay down $s'o'=wx+yz$ which should close at o'. These distances numerized are equal to 9, 8, 5.5, 3 and 1.5 tons, or total $=27$ tons. We thus see that after reaching the nth post from the other abutment, the vertical end post being considered as $n=1$, the value of $\frac{ne}{\mathrm{N}}$ becomes successively $\frac{n-1}{\mathrm{N}}e$, $\frac{n-2}{\mathrm{N}}e$ $\frac{n-n}{\mathrm{N}}e$ $=o$ as the train leaves the bridge.

Complete the diagram exactly as before for the strains.

C. COUNTER BRACES.

The reactions for the dead load of the bridge being equal on the two abutments, lay off on the third vertical the dead loads exactly as described for the first case, and proceed as there explained.

2D. LINVILLE TRUSS WITH VERTICAL ENDS.

We will take the same truss that we had before, and of which one system is shown in Fig. 9. We will also suppose four panels $= n$ panels of the double truss loaded with an excess of 2 tons per truss per panel. The reactions will now be different from the preceding case, and will also differ when the engine is at different ends of the bridge, hence we must construct two diagrams, one for each case.

A. TRUSS COVERED WITH LOAD. MAXIMUM CHORD STRAINS.

In Fig. 10, suppose the engines to be standing on the left end of the bridge. Let (Fig. 9) CK = one panel weight of truss with uniform load, and KL=H1= the engine excess in one panel. That is CK=EH=10 tons; KL=H1= 2 tons.

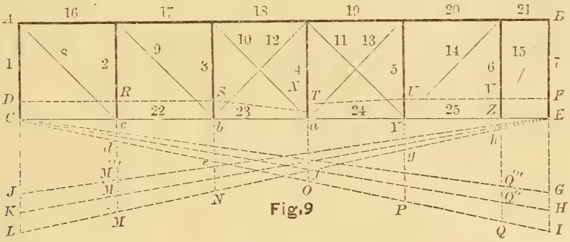


Fig. 9

Now since $n=4$ panels of the double truss are bearing this excess, there will be $\frac{n}{2}$ panels in each single system so covered; in this case$=2$. (With n even, opposite ends of each system will always support $\frac{n}{2}e$, but if n is odd, the end of the single system with the short panel will hold $\frac{n+1}{2}e$, while the other end would hold $\frac{n-1}{2}e$; that is, n even, $\frac{n}{2}$ panels are loaded in each system with the excess; n odd, the system with the short panel end is loaded at one more panel point than the other). Supposing, however, n even, then in Fig. 10 which supposes the engine at the left abutment, $\mathrm{R} = \mathrm{MO}=\mathrm{M}''c+\mathrm{N}''b+j''a+g''\mathrm{Y}+h''\mathrm{Z}+\mathrm{MM}''+\mathrm{NN}''$.

(The M″, N″, f'', &c., are supposed to be on the line KE, immediately above the same letters on the line LE. The line JE will be denoted by thirds, as M‴, N‴, f''', &c., and similarly for CH and CG. Also CJ=EG=live uniform panel load=8 tons, and JK=GH=dead panel load=2 tons).

That is, numerizing, $\mathrm{R}=\mathrm{MO}=(9+7+5+3+1)\frac{10}{11}+(9+7)\frac{2}{11}=25.64$ tons.

$\mathrm{R}'=\mathrm{OL}=\frac{1}{4}\mathrm{NW}-\mathrm{R}=cd''+be''+ao''+\mathrm{YP}''+\mathrm{ZQ}''+dd''+ee''+\frac{1}{2}\mathrm{W}$.

$=(2+4+6+8+10)\frac{10}{11}+(2+4)\frac{2}{11}+5=33.36$ tons.

$=\frac{1}{2}\mathrm{NW}+\frac{1}{2}ne-\mathrm{R}=59+25.64$.

N=number of panels in double truss.

Now lay off OM=K, then MN=NP $=W+e=12$ tons for $\frac{n}{2}=2$ times, then $W=10$ tons successively to close at V. VL then should equal $\frac{1}{2}$W. Complete the diagram as before.

If, as in Fig. 11, the engine stands at the right end of the bridge, then the panel points Y and Z would bear the excess of load, and OL would equal $\frac{1}{2}W+ZQ''+YP''+ao''+be''+cd''+QQ''+PP''$. Conversely $oY=M''c+N''b+f''a+g''Y+h''z+hh''+gg''$ or, numerizing, $OL=5+(2+4+6+8+10)\frac{10}{11}+(8+10)\frac{2}{11}=35.543$ tons, $OY=(9+7+5+3+1)\frac{10}{11}+(3+1)\frac{2}{11}=23.457$ tons.

Lay off ML=$\frac{1}{2}$W, and then $MN=NP=W+e=12$ tons for $\frac{n}{z}$ times, then $PR=RS=SY=W=10$ tons, which should close at Y. Then complete the diagram exactly as before.

We find by this that either method of loading gives the same center strain on the chords, so long as n is even, but that in the end panels, the strains are heavier according to the position of the excessive load; therefore, from the maximum strains, find the strains in the double truss as before explained.

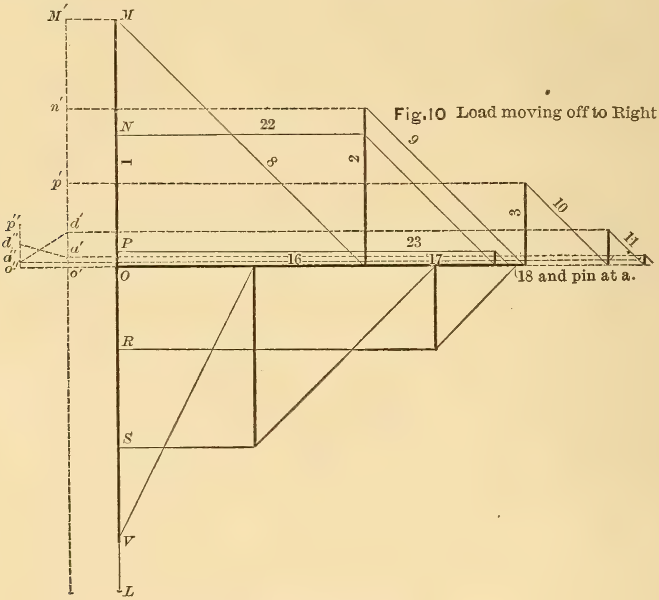


Fig.10 Load moving off to Right

B. MOVING LOAD. MAXIMUM POST AND BRACE STRAINS.

A. LOAD MOVING OFF TO THE RIGHT.

In Fig. 10 O'M'=OM as before. Also, the strains will be lessened by $\frac{ne}{N}$ as explained previously, N and n still referring to the double truss, hence lay off $M'n'=Rc+cM'''+ee''=9.27$ tons; $n'p'=Sb+bW'''+ee''=7.82$ t; $p'd'=ax+af'''+ee''=4.91$ tons; but here, since ZE is put on half panel, $\frac{1}{n}$ of the engine will have left the bridge, or $\frac{ne}{N}-\frac{n1}{N}e$, hence make $d'a'=Yg'''+gg''=2.73$ tons, and $a'o'=zh'''+hh''=.91$ tons, which should close at o'. Complete the diagram as before.

b. LOAD MOVING OFF TO THE LEFT.

In Fig. 11 O'M' still equals OM, and the strain caused by e still diminishes by $\frac{ne}{N}$, therefore lay off $M'N'=ZQ'''+ee''+Vz=10$ tons; $n'p'=UY+Yp'''+ee''=8,545$ tons; $p'd'=Ta+aO'''+ee''=6,544$ tons; $d'a'=be'''+ee''=3.636$ tons; and here again the engine goes off at the next panel, so that $a'o'=cd'''+dd''=1.817$ tons; which should close at O'. Complete this diagram, and assume those strains which give the largest results.

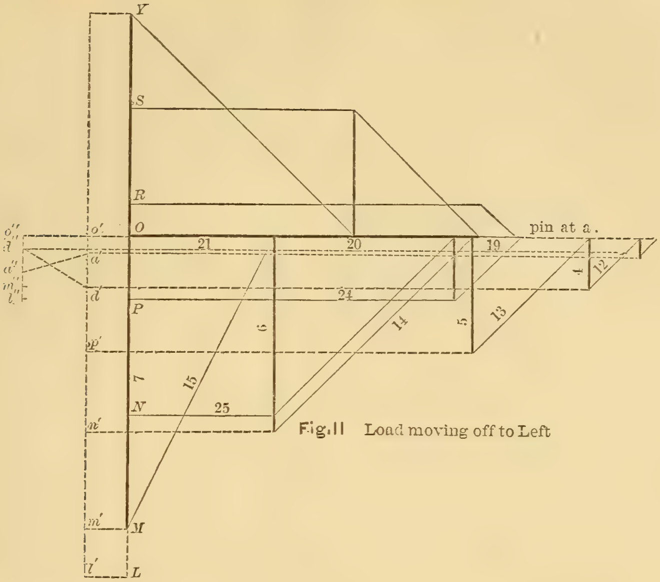


Fig. 11 Load moving off to Left

C. COUNTER BRACES.

These are found precisely the same as when we supposed the bridge covered with an uniform load.

INDUSTRIAL CHEMISTRY.*

By Prof. ABEL, F. R. S.

From "Nature."

The epoch is well within the recollection of chemists of my generation, when the British iron-master first awoke to the benefits which might accrue to him from an application of the labors of the analytical chemist in connection with iron-smelting.

When the last great stride was made in the manufacture of cast-iron by the introduction of the hot blast, the iron-smelter was naturally led to seek profit, to the fullest extent, with respect both to the great increase in the rate of production of pig-iron attainable thereby and to the economy achievable in regard to the proportions and characters of the materials employed in the production of pig-iron. But after a time the great falling-off in the quality of a large proportion of the products of the blast-furnace, and the difficulties experienced in the production of malleable iron of even very moderate quality, aided by the great impetus to competition in respect of quality, given by the first International Exhibition in 1851, directed the attention of our more enlightened iron-masters to the likelihood of their deriving important aid from chemical science, and more especially from the investigations of the analytical chemist.

Among the earliest to realize the importance of trustworthy and detailed information regarding the composition of the iron ores of the country was Mr. S. H. Blackwell, who, in presenting to the Royal School of Mines a very extensive and interesting series of British ores which he had collected with great labor and expense for exhibition in 1851, placed at the disposal of Dr. Percy the requisite funds for engaging the services of competent analysts (Messrs. J. Spiller and A. H. Dick) who, under his direction

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[*] Abstract of an address before the Chemical Section of the British Association.

and with subsequent pecuniary aid from himself and from Government funds, carried out a very careful and complete examination of this series, the results of which have been of great value, for purposes of reference, to those actively interested in the iron industry. It was, however, the first connection of Messrs. Nicholson and D. S. Price and of Mr. E. Riley with two of the most important iron works of this country, about a quarter of a century ago (*i.e.* at the time when the above investigation was commenced), that marked, I believe, the commencement of systematic endeavors to apply the results of analytical research to the improvement and regulation of the quality of the products of our iron works.

It is, perhaps, but natural that the primary object sought by applications of the knowledge of the analytical chemist should have been to eliminate or reduce the existing elements of uncertainty in obtaining the most abundant yield of pig-iron capable of conversion into railway-bar sufficiently good to meet the minimum standard of quality, and to reduce still further the cost of production of such bar-iron by utilising materials concerning the composition of which (richness in iron, &c.) the iron-smelter was completely in the dark. The information accumulated by the analyst respecting the composition of the ores, fuel and fluxes available at the works, and the composition of the pig-iron and slags or cinders, produced under varied conditions, in regard to materials employed, and to the proportion of ore, fuel, and flux used in the blast furnace, could not, however, exist long without exerting a marked beneficial influence upon the quality of iron produced, and generally upon the iron industry of the country.

Percy's invaluable work of reference on Metallurgy furnishes abundant evidence of the scientifically interesting, as well as practically useful, nature of the results obtained at that time by the chemists above named, and others, working under Dr. Percy, with respect both to the elaboration of important analytical processes (in which direction Mr. Riley has continued to the present day to do valuable work) and to the elucidation of the reactions occurring in the processes of reduction and refining of the metal. It is needless to dwell upon the fact that the aid of the analyst has now long since become absolutely indispensable to the iron and steel manufacturer; but I may, perhaps, be allowed briefly to refer to one or two recent illustrations of the indispensable part which analytical research has played, and continues to play, in the extension of our knowledge of the chemical reactions involved in the production of cast and wrought iron and of steel, and of the influences which the chief associates of iron in its mercantile forms exert upon its physical characters.

Among the many valuable communications made to that most important body, the Iron and Steel Institute of Great Britain, by men who combine great practical knowledge and experience in iron and steel manufacture with high attainments in mechanical science and such knowledge of chemical science as insures a full appreciation of its value at their hands, one of the most interesting and suggestive to the chemist is that on the separation of carbon, sulphur, silicon, and phosphorus in the refining and puddling furnace and in the Bessemer Converter, contributed to the *Transactions* of the Institute's recent meeting, by Mr. Lowthian Bell, whose valuable investigations in connection with the iron industry are as interesting to the chemist as they are useful to the manufacturer. Mr. Bell has brought together the results of an extensive series of practical experiments on the treatment of different kinds of pig-iron of known composition, in the finery, the puddling-furnace, and the Bessemer Converter, and, by comparing the results of analytical investigation of the products of those experimental operations with each other and with those of the materials operated upon, he has obtained valuable confirmation of the views already held by metallurgic chemists regarding the succession in which carbon, silicon, sulphur, and phosphorus are attacked when pig-metal is submitted to the above purifying processes, and the extent to which those foreign associates of iron are abstracted or resist removal, by the more or less thorough application of those several modes of treatment. He has also thrown new light on the reasons why the most difficultly-available impurity, phosphorus, obstinately resists all attempts to effect even a slight diminution in its amount by application of the Bessemer treatment. The earnestness with which Mr. Bell wages war against this enemy of the iron-master in one of its most favorite haunts, the Cleveland District, not simply with the old British pluck, which acknowledges not defeat, but systematically, on scientific principles, calling to his aid all the resources which the continual advances in applied mechanical and chemical research place within his reach, cannot fail to contribute importantly, if it does not of itself directly lead, to the complete subjection of this most untractable of the associates to which iron becomes linked in the blast-furnace. Indications have lately not been wanting that the existence of phosphorus in very notable proportion in iron may not of necessity be inimical to its conversion into steel of good quality, and it may be that this element, which is now turned to useful account to impart particular characteristics to the alloys of copper and tin, is even destined to play a distinctly useful part in connection with the production of steel possessed of particular characters valuable for some special purpose.

In the great development which steel manufacture has received within the last few years, one most prominent feature has been the production, with precision, upon a large scale, of steel of desired characteristics, in regard to hardness, &c., by first adding to fluid cast-iron of known composition the requisite proportion of a rich iron ore (with or without the addition of scrap iron) to effect a reduction of the carbon to the desired amount, concurrent with a refining of the metal by the oxidising action of the ore, and then giving to the resulting steel the desired special qualities by the addition of suitable proportions of iron compound of known composition, rich in manganese and carbon (Spiegeleisen and the similar product called ferro-manganese). The germ of this system of producing steel varieties of predetermined characteristics exists in crucible processes like that of Uchatius, which have been in more or less extensive use for many years past, but it is to such invaluable arrangements as are most prominently represented by the Siemens-Martin Furnace—wherein several tons of metal may be fused and maintained at a very high temperature with as little liability to change from causes not under control, as if the operation were conducted in a crucible—that we are indebted for the very great expansion which the direct application of the analytical chemist's labors to the development of the steel industry is now receiving.

The production of steel upon the open hearth, to the elaboration of which Dr. C. K. Siemens has so largely contributed since he first established the process at Llandore in 1868, has in fact, become assimilated in simplicity of character and precision of results to a laboratory operation, and may be justly regarded as a triumph of the successful application of chemical principles and of the power of guidance and control afforded by utilising analytical research, to the attainment of prescribed results upon a stupendous scale, with an accuracy approaching that which the experienced chemical operator secures in the laboratory upon a small scale, under conditions which he can completely control. The production of steel by a large number of small separate operations in pots has now become supplanted with great advantage by the Siemens-Martin system of working at some of our largest establishments at Sheffield; this system has also secured a footing at highly renowned Continental works, which are formidable competitors with us in the manufacture of steel, such as those of Essen, Creusot, and Terrenoire. It is specially interesting to notice that, in the hands of those who, on the Continent at least equally with ourselves, have learned to combine the results of practical experience with the teachings of chemical science, the facilities now existing for dealing in a single receptacle with large masses of fluid steel have greatly facilitated the application of chemical means to the production of *solid* masses of considerable size, thereby reducing, if not altogether dispensing with the necessity for submitting large steel castings to costly mechanical operations with the object of closing up cavities caused by the escape of occluded gas as the liquid metal cools. The success in this direction which appears to have attended the addition of silicon, in combination with iron and manganese, to the steel before casting in preventing the formation of so-called *blow-holes*, and in contributing at the same time to the production of the particular character of steel required, bids fair to be of special importance in connection with the application of steel to the production of projectiles for use against armor-plates, as affording ready and comparatively very economical means of ensuring the production of perfectly sound castings, or which in compactness of structure will, it is asserted, compete successfully with carefully forged castings, and even with the magnificent material which Whitworth produces by submitting the fluid metal to powerful pressure.

The part which silicon plays by its comparatively high susceptibility to oxidation, in promoting the production of sound steel castings is readily inteligible, but the functions of the manganese compounds which are an indispensable adjunct to the *Bessemer* process, and the application of which has become an integral part of steel manufacture, are still far from being thoroughly understood, and there is ample scope for chemical research, in co-operation with practical experiment, in the further study of the influence not only of manganese in the production, and upon the properties of steel, but also of elements such as titanium, tungsten, and boron, and of chromium, which exists, associated in considerable quantities with iron, in a very abundant Tasmanian ore, to which prominent attention has lately been directed. The achievements of the mechanical engineer have so facilitated the handling and perfected the means of production and the mechanical treatment of malleable iron and of steel, that the full advantage may now be reaped of any improvement of a chemical nature which may be effected in the production of those materials; and it must be a source of pride to the chemist to observe with what success the teachings of his science are being applied, by practical men of the present day, in the construction of furnaces capable of withstanding the high temperatures required for the production and working of iron and steel in large masses, and in combining the perfect consumption and consequent great economy of fuel with the attainment of those high temperatures and with a thorough control over the character of the gaseous agents to which the fluid metal is exposed in the furnace. I need not quote the names of those men who have already rendered themselves prominent by their services in this particular direction, but may refer, in special illustration of the results achieved by purely practical men, to the success in applying very simple furnace arrangements to the attainment of the above results which has recently attended the labors of Mr. William Price, a principal foreman in the Royal Gun Factories at Woolwich.

A few experiments made in the early days of the application of armoring to ships and forts appeared to demonstrate on the one hand that steel was quite incapable of competing with malleable iron of even very moderate quality as a material for armor-plates, and, on the other hand, that the penetrative power of projectiles made of chilled iron upon the Palliser system could not be surpassed or even attained, with any degree of certainty, by projectiles of steel produced at comparatively very great cost. But some recent results obtained on the Continent, and especially in the course of the important experiments instituted by the Italian Government at Spezzia, have afforded decisive indications that steel, the application of which to the construction of ordnance has since that time been very greatly extended, may now be looked to hopefully as capable of affording greater protection against the enormous projectiles of the present day than can be secured by proportionately large additions to the stupendous iron-armoring of the most modern ironclads, and also as applicable at a cost very moderate, when compared with that of ten years ago, to the production of projectiles of large dimensions superior in point of penetrative power and of uniformity in this respect to those of chilled iron, the difficulties in the production of which are very greatly increased by the formidable increase which has lately been made in their size. Promising results have also quite recently been obtained at Shoeburyness with a new system of applying steel in conjunction with malleable iron, by which a perfect union of the two materials at one of their surfaces is effected by the aid of heat.

The superiority of soft and very homogeneous steel over wrought iron of the best quality in regard to lightness, combined with strength and toughness, are leading to its very advantageous employment in the construction of a particular class of vessels for the navy; and the perfect confidence which can be placed in the uniformity in structure and strength of steel of such character as is produced by the Whitworth system of manufacture has greatly facilitated the production of air-chambers of small weight, but capable of being quite safely charged with sufficient air, under a pressure of 1,000 pounds on the square inch, to carry the Whitehead torpedo through water to a distance of 1,000 yards in little more than a minute and a half.

Thus, the results of the recent development of steel-industry, to which the labors of the chemist have not unimportantly contributed, give promise of exerting a great influence upon the resources of nations for defense and attack. Although the necessity for the continual expansion of such resources cannot but be deeply deplored, there can be no doubt that the problems which it presents, and the special requirements to which it gives rise, must operate, and perhaps as importantly as the demands created by peaceful industries and commercial enterprize, in encouraging the metallurgist, the chemist, and the engineer to continue their combined work in following up the successess, to the achievement of which the results of scientific research have greatly, though indirectly, contributed.

If it were necessary to add to the illustrations which Mr. Perkin gave in his address last year of the practical fruits of research in *organic* chemistry, I might be tempted to dilate upon the important results which have, especially during the last ten years, grown out of the discovery and study of the products of the action of nitric acid upon cellulose and glycerin. During the six years which have elapsed since I had the honor of bringing before the members of the British Association the chief points of scientific interest and practical importance presented by the history of those remarkable bodies, their application to technical and war purposes has been greatly developed. Nitro-glycerin and gun-cotton may now be justly classed among the most interesting examples of the practical importance frequently attained by the results of chemical research, while the history of the successive steps by which their safe manipulation and efficient application have been developed affords more than one striking illustration of the achievements effected by combined physical and chemical research in the solution of problems of high scientific interest and practical importance, and in the vanquishment of difficulties so formidable as for a time to appear fatal to the attainment of permanently practical success.

It is to a careful study of the influence which the *physical* character of gunpowder (its density, hardness, &c.) and its *mechanical* condition (*i.e.*, form and size of the masses and condition of their surfaces) exert upon the rapidity of its explosion under confinement, that we chiefly owe the very important advance which has been made of late years in controlling its explosive force; in its applications as a propelling agent, and the consequent simple and effectual means whereby the violence of action of the enormous charges now used in siege and ship-guns is effectually reduced to within their limits of endurance without diminution of the total explosive force developed. But, concurrently with these important practical results, the application of combined chemical and physical research to a very extended and comprehensive investigation of the action of fired gunpowder has furnished results which possess considerable interest from a purely scientific point of view, as in many respects modifying, in others supplementing, the conclusions based upon earlier experiments and theoretical considerations with respect to the nature and proportions of the products formed, the heat developed by the explosion, the tension of the products of combustion with the conditions which regulate it both when the explosion is brought about in a close vessel and when it occurs in the bore of a gun. The results of these physico-chemical researches have, moreover, already acquired practical importance in regard to the light they have thrown upon the influence exerted by variable conditions of a mechanical nature upon the action of and pressure developed by fired gunpowder in the bore of a gun, and in demonstrating that modifications in the *composition* of gunpowder, not unimportant from an economical point of view in dealing with the very large charges now employed, may importantly contribute to render the storing of the maximum of work in the projectile, when propelled from a gun, compatible with a subjection of the gun to comparatively very moderate and uniform strains.

Other interesting illustrations of the intimate manner in which physical and chemical research are linked together, and of the important extent to which some of our most illustrious workers in chemistry have contributed to demolish the semblance of a barrier which existed in past times between the two branches of science, are furnished and suggested by the recently published List of Grants of Money which the Government has made to scientific men, on the recommendation of the Royal Society, from the fund which, for the first time last year, was added to the very modest sum previously accorded from national resources in support of research. The perusal of that list, representing as it does a most carefully considered selection by the highest representatives of science in the country, from a very large number of applications, affords important evidence, on the one hand, of the active pursuit of science in Great Britain, and, on the other, of the very wide range of subjects of interest and importance, the full investigation of which demands the provision of adequate resources. That the necessity for such resources needs but to be thoroughly made known to ensure their provision, even from other than national sources, has been demonstrated by the success which, in a comparatively brief space of time, has attended the efforts of the Chemical Society to establish, upon the foundation patriotically laid by one of its original members, Dr. Longstaff, a special fund, to be administered by the Society for the advancement of chemical science. An inspection of the list of contributors to this special fund in aid of chemical research which, in about two years, has reached the sum of four thousand pounds, and from the proceeds of which the first applications for grants have recently been met, is suggestive of two observations. One is, that the proportion and amount of contributions hitherto received are comparatively small from the source whence the greatest support of such a fund may naturally be looked for, namely, from those who most directly benefit by the results of chemical research. It is to be hoped that there are many prominent representatives of the chemical and metallurgic industries in this country who still intend to give practical effect to their natural desire to aid in the advancement of chemical science, and to the appreciation which they can hardly fail to entertain of the usefulness of this fund. On the other hand, it is a matter well meriting special notice that a very prominent section of the contributors to the fund is composed of some of the most ancient corporate bodies of the city of London. Most welcome evidence is thereby afforded of the readiness with which the City Companies are prepared to respond to appeals for the substantial support of measures well calculated to promote progress in science. This evidence, and the combined action which they are even now contemplating for promoting the application of scientific research to the advancement of industry and commerce, by establishing an institution for technical education upon a scale worthy to serve as a monument of the true usefulness of wealthy confederations, must be cordially hailed as very substantial proofs that these representatives of our national wealth and commercial supremacy are entering upon a new sphere of activity which will more than restore their ancient prestige, by according them a new rank, more elevated than any which their civic importance could, in the past or future, confer upon them—a rank high among the chief promoters of our national enlightenment.

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GUNBOATS built for the Chinese Government have proved excellent sea boats in their outward voyages. Lord Napier of Magdala, who inspected them at Gibraltar, expressed a very high opinion of their qualities and powers of river defence and aggression.

THE BREMEN WATERWORKS.

By E. BOTCHER and H. OHNESORGE.

From "Zeitschrift fur Bauwesen."

The following were the chief points laid down, before this work was commenced, by Herr Berg, chief architect to the town. As the number of inhabitants was about seventy thousand, provision should be made for a supply of 900,000 gallons per diem. A reserve machine should, however, be provided, capable of pumping another equal quantity; and in laying the pipes, provision should be made for an increase in the supply, and for an eventual doubling. The level of the highest inhabited part of the town is 39 feet above Bremen datum.

It was at first proposed to procure the supply from the ground by natural filtration, but experiments showed that such water could not be used. It was found that the river was perfectly free from metallic salts in solution, but that after filtration through the clay bed sulphuret of iron (Schwefeleisen) appeared in appreciable quantities. It was therefore determined to obtain the water from the river, and to purify it by artificial filtration.

The works are situated on a small peninsula, formed by the junction of the great and small Weser. They consist of a handsome square building containing the pumping machinery, and two large water-tanks placed 128 feet above datum. The building has four towers, which are respectively appropriated for the chimney-shaft, the falling and rising mains, and the staircase. On the side of this building is the boiler-house, and on the other corresponding to it a dwelling. Behind these is a large covered reservoir and filtering basins. The water is obtained from the Great Weser, and is admitted by a pipe into a well, from which it is pumped into the filtering basins; thence it passes into the covered reservoir, and is pumped into the tanks and into the pipes.

The filtering basins have each an area of 808 square yards. The sides and bottom are of bricks set in trass mortar, and plastered over with Portland cement mortar. The bottom bend is 9.17 feet above datum. The following is the arrangement of the filter layers and their respective thicknesses, beginning from the bottom:

a.	Coarse shingle	in thickness of	20.87	inches.
b.	Middling "	"	7.56	"
c.	Fine "	"	5.71	"
d.	Coarse gravel	"	5.71	"
e.	Fine "	"	5.71	"
f.	Fine sand, washed	"	28.47	"

The top level of the sand layer is, therefore, 15.30 feet above datum.

Experience has shown that the deposited matter does not infiltrate beyond 1 inch, and it then forms such a dense crust that the filtering basin cannot be further used until it is removed. This is easily done by sod-cutters, and can be repeated for some time before the sand need be replaced. The pipe by which the water is admitted is 14.85 inches in diameter; its position being vertical, with a trumpet-shaped mouth. The water, having passed through the filters, is carried off by nine small channels into one larger, whence it passes into the covered reservoir through a stand-pipe having two outlets; one level with the bottom, and the other about 11 inches below the ordinary water-level of the filtering basins. When filtration is proceeding rapidly, the latter is used; when slowly, the former, by which the rate can be kept nearly uniform.

The covered reservoir has an area of 1,600 square yards, exclusive of the pillar space. The bottom, which is 7.4 feet above datum, is covered with a layer of Portland cement mortar, 1 inch thick. To protect the water in the reservoir from frost it is arched over between iron girders, and these arches are covered with a layer of clay 3 feet thick. The main building had to be erected on piles, and its size was dependent on the two tanks placed 128 feet above datum.

The sizes of the pipes in use vary from 20 to 3 inches. They are socket pipes, and their lengths and dimensions are given in detail. Hydrants for a 2½-inch hose are placed about 100 yards apart. One hundred and twenty fountains have also been provided for free use. The pipes were tested to 15 atmospheres; and at 12 atmospheres they were rapped with hammers varying in weight from ½ lb. to 3 lbs. The water has to be taken over two bridges; and from the construction of the largest one of them, the pipe of 19.8 inches diameter is split into three small ones of 13.88 inches diameter, one of them being a reserve pipe. It also passes underneath the timber harbor for 45 yards through wrought-iron pipes. The laying was done by Messrs. J. and A. Aird, between November 1872 and April 1873.

The five Cornish boilers are usually fed by the donkey in the engine-house, but have also an independent steam-pump. The steam-chest is placed across the end of the boilers, and the steam-pipe goes along the top of the flue into the engine-house. The chimney-shaft is 3.78 feet in diameter, and is constructed of fire-brick masonry to 39 feet above datum, from whence the remaining 136 feet are formed by a cast-iron pipe.

The two double-cylinder horizontal steam-engines are exactly similar. The diameter of each cylinder is 19.5 inches, and the stroke 58.5 inches. The piston-rods of each are prolonged, the one working a so-called filter-pump, the other a high-pressure pump. Each engine has its condenser, and works an air-pump, a feed-pump for the boilers, and a small air-pump for the air-pressure in the mains.

The filter pump has a diameter of 18 inches, and a stroke of 58.5 inches. It lifts the water from the well in communication with the Weser; draws it through two air vessels, from whence it flows by a pipe 20.82 inches in diameter, and common to both engines, into the filtering basins. The lift is usually about 15 feet, and the quantity discharged per minute 1,540 gallons.

The high-pressure pump has a diameter of 17.86 inches, and a stroke of 58.5 inches. It lifts the filtered water out of the covered reservoir in a similar manner through a 14.85 inch pipe, and into the rising main, and thus into the tanks above. The lift is usually about 120 feet; and the quantity raised per minute 1,500 gallons.

Details are given of the various steam and pipe arrangements. The rising main is 20.82 inches in diameter, and the falling main 19.81 inches. They are both constructed of flange pipes, and have the necessary compensating arrangement under the tanks. The engine room contains self-registering gauges of the water-level in the five filtering basins and the reservoir, and a bell which rings as soon as the tanks are full. There are self-registering guages of the water-level in the tanks and in the well, as well as a manometer giving the pressure in the rising main.

Experiments were made to test the guaranteed coal consumption with very satisfactory results. The quantity of coal required to raise 220 gallons 120 feet being 0.776 lbs. (34,000,000 of duty).

The size of the tanks is 73 feet by 35 feet, by 11 feet high, and they are able to contain 372,500 gallons of water each. They are constructed of iron plates, with angle-iron ribs 6.4 feet apart. The total weight of each is about 80 tons. Each rests on nineteen cross girders, constructed to carry 3.7 tons. These rest on fourteen principal girders, with a span of 40 feet. The width of one of these girders in the center is, in proportion to its length, as 1 to 8. Its lower side forms a parabolic curve, joined to the upper horizontal side by seven uprights, which are joined to each other by diagonals. They rest at each end on cast-iron blocks which work on rollers, and so allow for the consequences of change of temperature. The total weight of all the girders is about 180 tons.

The cost of the work was £152,250.

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A City Railroad is now under construction in Berlin. It is to extend from the Lower Silesian depot to Charlottenburg, and will be seven miles long, and is expected to cost $1,071,000 per mile, and as half the right of way and grounds is already acquired and the work has been in progress a year, the estimate should not be very far out of the way. The road is to have four tracks, two for the through traffic of the roads with which it connects, which are six in number. Each of these lines is to have three stations in the city. The other two tracks will serve exclusively for local traffic, and will have six stations varying from less than ½ to 2¼ miles, and probably two other stopping places will be provided.

THE MECHANICS OF VENTILATION.

By GEO. W. RAFTER, C. E.

Written for VAN NOSTRAND'S MAGAZINE.

II.

The velocity of 7.3 lineal feet per second will therefore be in the pipe under the register, and the discussion may be continued on the supposition that the register is absent by using a sufficiently large value for the coefficient of influx in the expression for head following.

Then if resistance from friction and elbows and all other sources of resistance were entirely eliminated the head necessary to produce a velocity of 7.3 lineal feet per second throughout the small ducts would be sufficient to exhaust all the impure air. In fact, the reduction for friction, elbows, etc., gives us widely varying velocities for different parts of the ducts, and requires in every case an addition to the head.

To find head$=h'$ we take formula (16) and using the following values:

$$\xi_1=0.840,$$
$$\xi_0=1.750,$$
$$\xi=0.0205,$$
$$l=36,$$
$$a=6''=0.5 \text{ feet},$$
$$b=8''=0.66 \text{ feet},$$
$$V^2=(7.3)^2=53.29,$$
$$2g=64.4,$$

we have

$$h'=\left\{1+0.840+1.750+\left(0.0205\,\frac{36(0.5+0.66)}{2(0.5\times0.66)}\right)\right\}\frac{53.29}{64.4},\ \therefore$$

$$h'=4.04$$

Applying similar reasoning to the main ducts in basement and complete expression for h' aside from the vertical exhaust flue is found as follows:

$$h'=4.71 \text{ feet.}$$

According to formulæ (1) and (2) illustrating Gay Lussac's law for constant pressure, the volume of one and the same quantity of air increases or decreases directly as the temperature.

The average temperature of the air in the school-rooms was found to be 68°, while the temperature at the foot of the main shaft was found to be from 52° to 56°, average 54°. The air had lost then in passing to that point 14°. This reduction of temperature mainly took place in the main ducts in basement, the volumes of which equal say 1500 cubic feet.

Suppose 1500 cubic feet of air at 68° to pass into the main ducts in basement, and there become cooled to 54°, a reduction of volume takes place found by formula (1) thus:

$$V_0=1500 \text{ cubic feet}$$
$$t=54^\circ$$

In place of 32° in the formula substituting 68° the temperature at which $V_0=1500$, and, because the volume at a reduced temperature is required, changing the signs of t and 68° and changing the sign of a, we have for value of V at 54°

$$V=1-0.00204(68-54)\,1500=0.97144\times1500,\ \therefore$$

$$V=1457 \text{ cubic feet,}$$

and $1500-1457=43$ cubic feet.

We see, therefore, that a vacuum is continually forming in the main ducts and adding materially to the exhausting capacity of the works. For this reason the ventilation will probably work more freely in winter than in summer, unless means are provided to raise the temperature in main exhaust flue in a corresponding ratio during the latter season. Taking formula (15)

$$h'=\frac{ha(t'-t)}{1+at}$$

it is required to find the value of t', which will produce h' as found by previous calculation. Substituting numerical values as follows:

$$h'=4.71,$$
$$h=\ 55,$$
$$t=\ 54,$$
$$a=0.00204,$$

and we have—

$$4.71=\frac{55\times 0.00204\,(t'-54)}{1+0.00204\times 54},\ \therefore$$

$$4.71=\frac{0.1122\,t'-6.058}{1.11},\ \therefore$$

$$5.228=0.1122\,t'-6.058,\ \therefore$$

$$0.1122\,t'=11.286,\ \therefore$$

$$t'=101^\circ.$$

101° is then the constant temperature at which the air in vertical shaft is to be kept in order to produce the action through the ducts of a head equal to 4.71 feet.

It should be remembered that this calculation involves the supposition that the pressure is that due to the air at 54°. This supposition is only partly true, as the outside temperature will at different times vary widely from that point. The instability of equilibrium will extend itself not only to the air in the ducts and building, but also to every particle of outside air. The actual effect will depend upon a variety of circumstances, as, for instance, the constancy of supply of outside air, the method of introducing it, the temperature at which the outside air is delivered, the closeness of fitting of doors and windows and the tightness of the ducts, &c. For summer ventilation an increase of heat over that necessary for winter ventilation will be required. The capacity of the heating apparatus at foot of main shaft should, therefore, be in excess of winter requirements.

We have seen that in fact the original volume of 850,000 cubic feet per hour must be considerably less than that amount when reaching the foot of the main shaft, by reason of reduction of temperature. In order to make adequate provision for summer ventilation we will continue the discussion on the supposition that we have to deal with a volume of 850,000 cubic feet at foot of main shaft.

To raise 850,000 cubic feet from 54° to 101° will increase its volume to 931,498 cubic feet. Discharge per second then, equals

$$\frac{931498}{3600}=259\text{ cubic feet.}$$

h' as found above equals 4.71 feet, also

$$h'=\frac{ha(t'-t)}{1+at}$$

therefore substituting in formula (13), for the present neglecting E, and we have

$$V=\sqrt{64.4\times 4.71}=\sqrt{303.324},\ \therefore$$

$$V=17.41\text{ lineal feet.}$$

Since E was neglected we have 17.41 lineal feet as the theoretical velocity which will produce the necessary exhaust. Experiment indicates a reduction of the theoretical velocity in this flue of .12 for friction and eddies, consequently the actual velocity with a head of 4.71 feet, or what is the same with the flue heated to a point to produce that head, will be 0.88×17.41=15.32 lineal feet. But a velocity of 15.31 feet in the vertical flue corresponds to a reduction of velocity in the ducts, we must therefore, increase h', and consequently t' by an amount sufficient to produce an actual velocity of 17.41 lineal feet per second. To calculate that increase we have 17.41 equals required actual velocity, consequently, corresponding theoretical velocity will be

$$\frac{17.41}{0.88}=19.78.$$

The value of h' which will produce a theoretical velocity of 19.78 lineal feet will be found by taking—

$$19.78=\sqrt{64.4\times h'}.$$

Squaring this equation—

$$391.24=64.4\times h',\ \therefore$$

$$h'=\frac{391.24}{64.4}=6.07\text{ feet.}$$

It has been shown by what has preceded that the value of t' depends upon the value of h'. It will be necessary then, to find a new value of t' to correspond with the value of $h'=6.07$. This will be found as in the preceding case by substituting in formula (15), whence we have for ultimate value

$$t'=114^\circ.$$

To prove the correctness of these calculations we substitute complete values in formula (13), giving

$$V=0.88\sqrt{64.4\,\frac{55\times 0.00204(114-54)}{1+0.00204\times 54}},$$

whence by reduction

$V = 17.41$ lineal feet as previously found.

Translated from algebraic into common language, the preceding discussion means that a confined column of air at a temperature of 54° and original height of 55 feet, when heated to a temperature of 114° expands to a height of $(55' + 6'.07) = 61.07$ feet. But the height of the flue remains constant at 55 feet. The heated confined column then, when issuing from the flue becomes immediately cooled. The 55 feet of air in the flue is lighter than the outside air, by the weight of a column of the density of the confined column and 6.07 feet in length. According to the principle of equilibrium of fluids, the confined column must rise 6.07 feet in seeking such equilibrium, or, what amounts to the same thing, the outside air falls by approximately that amount.

We are now able to say what sectional area of shaft is required. By transposition of general formula (17) for velocity we have

$$F = \frac{Q}{V},$$

that is to say, the sectional area equals the quantity per unit of time, divided by the velocity per unit of time, therefore

$$F = \frac{259}{17.41} = 14.9 \text{ square feet.}$$

As already stated the shaft is constructed with cross-section equal to 26.62 square feet.

Attention may here again be called to the fact that theoretically, velocity in the shaft is entirely independent of the cross-section, depending as previously shown, upon temperature, height and adaptation. In practice, however, it will be found of the greatest importance not to have the shaft larger than careful calculation shows necessary. Indeed the case of a confined column in motion by excess of temperature is one where it is absolutely better to have the shaft smaller than calculation shows, rather than larger. If the flue has less sectional area the required discharge may still be produced by increasing the heat, so attaining higher velocities. If, however, the sectional area is greater than really necessary, the following condition of affairs results: The current of hot air passes up along the axis of the flue, while at the sides a current of cold air passes down. The result is the production of eddies and unnecessary cooling of the confined column.

An experiment at No. 9 aptly illustrates the point in question. Temperature of air entering the shaft 52°; temperature eight feet above fire 120°. The air had then been raised through 68°. This would be ample were the shaft of proper size to produce the necessary exhaust. A piece of tissue paper thrown into the middle of the ascending current and remaining there was carried to within ten feet of top of shaft in three seconds. The motion was however very irregular. When at the above point the slip was caught in an eddy and brought back to foot of shaft, was there secured and again sent up. The second time it maintained its position in the middle of the flue and passed out at the top in a little more than four and one half seconds. Repetition of this experiment under many different conditions showed that with a difference of temperature of 50° at the foot of the shaft the lightest tissue paper would rise less than one half the length of the shaft, while with a difference of from 30° to 40°, 10 to 15 feet was the extent of its rise.

We have in these experiments a significant explanation of the cause of smoky chimneys. A chimney has usually a certain specified work to perform, and like every other contrivance for the performance of work will obey the law of adaptation. In short, even in the construction of apparently so simple a thing as a chimney, it is unsafe to set at nought nature's universal laws.

We see farther that the expedient of constructing everything a little larger than necessary, in order to have a reserve for contingencies, is not always a safe one after all.

The following criticisms can be justly made of the No. 9 ventilating arrangements as they at present stand.

1. The vertical shaft has too great a sectional area.

2. The square elbows add greatly to the resistance. An increase of power is thereby necessitated to perform the work.

3. The ducts leading from the rooms

are too small, for two reasons: first, to carry away the maximum amount with their present cross-section requires so high a velocity of influx as to be uncomfortable to the occupants; second, friction is greater in small pipes than in large ones, as may be shown by the following: The height of resistance of friction, due to dimensions of pipe and to velocity of discharge, in a round pipe is expressed by formula thus:

$$h_0=\xi\times\frac{1}{2g}\times\left(\frac{4}{\pi}\right)^2\times\frac{lQ^2}{d^5}$$

where ξ=the coefficient of friction determined by experiment, l=length of pipe, d=diameter and Q=discharge.

Let h_1=the height of resistance of friction, &c., for a pipe of length also equal to l and diameter equal to $2d$, the discharge to be Q as above, then,

$$h_1=\xi\times\frac{1}{2g}\times\left(\frac{4}{\pi}\right)^2\times\frac{lQ^2}{(2d)^5}.$$

dividing

$$\frac{h_0}{h_1}=\frac{\xi\times\frac{1}{2g}\times\left(\frac{4}{\pi}\right)^2\times\frac{lQ^2}{d^5}}{\xi\times\frac{1}{2g}\times\left(\frac{4}{\pi}\right)^2\times\frac{lQ^2}{(2d)^5}},$$

whence by reduction

$$\frac{h_0}{h_1}=\frac{32}{1},\ \therefore$$

$$h_0 : h_1 :: 32 : 1,$$

that is, the friction in the small pipe is 32 times what it is in a pipe of double the diameter, the discharge being constant. In the same way it can be shown that with a diameter three times as great the friction would be as 3^5 to 1 or as 243 to 1 and so on.

4. The exhaust openings into the rooms are directly in the floor; the pipes, therefore, soon become filled with dust and papers.

5. The small ducts leading to the rooms are all of a size, whereas, they should increase according to the law of friction as distance from the vertical flue increases.

6. The form of shaft is faulty. It should be smaller at the top than at the bottom to compensate for the inevitable shrinkage of volume of the air current, resulting from loss of heat by radiation.

We will now determine the amount of coal which when burned will produce the necessary head.

A pound of coal contains on an average 14,500 pound-degrees or heat-units, that is, the heat from the combustion of one pound of coal will raise 14,500 pounds of water 1°, or, what is the same will raise one pound of water 14,500°. Specific heat of air is 0.238 when water is unity; or more clearly, to heat a pound of air from any temperature a, through a given number of degrees b to a temperature $a+b$, will require only 0.238 as much heat as would be necessary to raise a pound of water through the same number of degrees.

As already noted the air arrives at the point of heating—the foot of the shaft—at a temperature of 54°. It is required to raise it to 114°, or through 60°. To raise one pound of air 60° we have $1\times60\times0.238=14.28$ heat-units. Farther a pound of air at 32° equals 12.34 cubic feet, therefore by formula (1) we have for volume at 54°

$$V=[1+0.00204(54-32)]12.34,\ \therefore$$

$$V=12.89 \text{ cubic feet.}$$

850000 cubic feet of air at temperature 54° will equal

$$\frac{850000}{12.89}=65865 \text{ pounds.}$$

The number of pound-degrees required per hour will be

$$65865\times14.28=940552.$$

The number pounds of coal required for seven hours, the time ventilation is required each day, will be

$$7\left(\frac{940552}{14500}\right)=454 \text{ pounds.}$$

There is, however, a certain loss of heat by conduction through the walls of the flue, determined by the formula

$$Q=ka\frac{t'-t}{d} \qquad (18)$$

in which the notation is as follows:

Q=quantity of heat conducted away per hour.

k=coefficient of conduction=0.085*.

a=area of interior surface of the flue =1135 square feet,

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* For values of k for various materials, see volume 2, Weisbach's Mechanics.

t'=inside temperature=114°,
t=outside temperature, taken at 32°,
d=thickness of wall=0.35 feet.

Substituting numerical values and making the expression for loss during seven hours, gives,

$$Q=7(0.085\times 1135)\frac{114-32}{0.35},\therefore$$

$$Q=158228 \text{ heat-units,}$$

requiring a consumption of coal to make good this loss of

$$\frac{158228}{14500}=10.8 \text{ pounds.}$$

Adding this to the above and we have 465 pounds of coal=total consumption each day.

The question naturally arises how shall this heat be applied? Obviously there is no loss of heat except as just indicated by conduction. The heat may be applied then, by large grate or by furnace, with ample opening for draught, and with smoke-pipe of large diameter extending to within 15 feet of top of flue.

One point in particular brought out by studying this system of ventilation is worthy of careful attention, namely: To ventilate a building means there is work to be done. The performance of work requires an expenditure of force, therefore, any system professing to secure ventilation without performing work, or what is the same, without expending force, is without effect.

It is not denied that natural ventilation may at times prove efficient, though it is so uncertain in its action that in buildings of any size it is practically useless.

The ventilation of a building should be like its water supply and sewerage, always ready for work and certain in its effects. When buildings are erecting such a condition can be attained at moderate expense, while it may be exceedingly difficult to accomplish after the completion. It is, then, of the highest importance that ventilation be considered in connection with design.

The warming at No. 9, is by stoves in each room. A cylinder of galvanized iron is placed about the stoves, thus converting them into hot air furnaces, the air in which may be taken as a heated confined column. Fresh air is introduced at floor from outside, by ducts of sectional area (12″×16″)=192 square inches, and from 2 to 3 feet in length. Average temperature of air in cylinder 220°, outside temperature taken at 32°. Cylinders from 6 to 8 feet in height. Velocity of inflow is found by formula (13). Amount of inflow under the most favorable conditions equals 21.5 cubic feet per second, giving a supply of 67,400 cubic feet per hour for each room.

This is a cheap way of improving the warming of a building already furnished with stoves. The diameter of the cylinders should be at least 2 feet greater than that of the inclosed stove, and their length should be nearly equal to the height of the room. The inflow duct should be of ample cross-section and provided with a slide in order that the supply may be regulated. The supply in any case should be sufficient to prevent the temperature of the issuing air rising much above 120°.

The amount of air required to support combustion may be calculated as follows: Let the amount of air to be supplied to any room be 65,000 cubic feet per hour or 455,000 cubic feet for seven hours, or in the same proportion for any number of hours, then

$$\frac{455000}{13.0}=35000=\text{number of pounds to}$$

be supplied for seven hours at temperature of 60°.

We will take outside temperature at 32°, and for purposes of this calculation, in order to make an allowance for loss by radiation, we will take the temperature to which the air is to be raised at 75°. This would involve raising the temperature through 43°. The number of heat-units required per day of seven hours, therefore, will be

$$35000\times 43\times 0.238=358190.$$

The daily consumption of coal under the above conditions is

$$\frac{358190}{14500}=24.7 \text{ pounds.}$$

Observation and calculation have demonstrated that the amount of air required to consume a pound of coal is on an average 150 cubic feet. From imperfection in burning appliances, etc., the consumption frequently rises above twice that amount, and 300 cubic feet to the pound is generally used for these calculations. We have for the consumption during seven hours

$$24.7 \times 300 = 7410 \text{ cubic feet.}$$

In applying this matter practically it must be borne in mind that usually *larger quantities* will be needed than calculation indicates unless very liberal allowances for loss by radiation and conduction are made.

A new phase of ventilation has recently been developed by a German philosopher, Pettenkofer by name, who found that by diffusion alone, the air of a room in his house (cubic contents 2650 feet) was changed once every hour when the difference of exterior and interior temperatures was 34°. With the same difference of temperature, but with the addition of a good fire in the stove, the change rose to 3320 cubic feet per hour. With all the crevices and openings about doors and windows pasted up air tight the change amounted to 1060 cubic feet per hour. With a difference of 40° between exterior and interior temperatures the ventilation through the walls amounted to 7 cubic feet per hour for each square yard of wall surface. The practical deduction from which is, that ventilation may be much improved by merely increasing the amount of free wall surface. In designing school houses, particularly, such an arrangement should be adopted as will insure at least two outside exposures, thus giving not only better ventilation, but also rendering it certain that a sufficient supply of light be admitted.*

One way of applying the vacuum method of ventilation is to carry the smoke flue from furnace in basement up through a vertical shaft from which openings into the rooms are made. Several examples of this form of construction recently examined by the writer have the smoke flue of vitrified sewer pipe. This offers a cheap method of forming the flue, though it can scarcely be recommended on account of the low conducting power of the material, the conducting power of fire-clay, the main material of vitrified pipe, being 39.6 less than that of wrought iron.

An important application of the formulæ for confined columns, etc., may be made in designing the flues for hot air in buildings warmed by furnaces. The method quite frequently followed at present is to make the flues leading to the various stories of a building all of the same size, and this without reference to the horizontal distance from the source of supply. The result of this unscientific proceeding is that down currents are created in the pipes leading to the lower stories. To remedy this difficulty it is customary to introduce slides into the longer flues by which a part of their capacity may be cut off. A curious misapplication of the above practice recently came to the writer's notice. School-house No. 11 is a new building, occupied about January 1st. It was found that no hot air could be brought into the rooms of the first floor, and that currents of air actually passed from those rooms into the flues. An examination into the matter revealed the fact that, the flues for the different floors were all of the same size and that the slides for regulating the supply had been placed in those leading to the first floor instead of to the floor above. The result was as just stated, the air passed from the rooms of the first floor into the flues leading to that floor, down to the furnace, and then up to the second floor. The first floor was, therefore, ventilated directly to the second, a condition of affairs neither conducive to comfort on the first floor nor good health on the second.

Obviously the better way is to decide how much air each flue is required to supply, and then apply the formulæ above given for cross-section, &c. In this way a constant balance of forces may be maintained.

It is a well-established principle of mechanics that every force acts along the line of least resistance, and no better illustration of its truth can be found than in the application of the laws of movement of heated, confined air-currents.

In order to farther illustrate the theory of ventilation as here set forth, a numerical application will also be made to the Assembly room in the city building of the City of Rochester.

Preliminary data is as follows:

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* Hartley's Lectures. "Air and its relation to life."

Length $=108'\ 0''$
Breadth $=\ 78'\ 0''$
Height $=\ 27'\ 0''$

Floor area, with an addition for stage $=8831$ square feet.

Total volume, including stage, without deduction for furniture, &c. $=244{,}949$ cubic feet.

The seats now in place accommodate comfortably 1200 people, with standing room for 400 more. Total capacity is therefore taken at 1600. Deducting one-half cubic foot for each seat, and 3 cubic feet for space actually occupied by each person, and making other deductions for cornices, raised floor at sides and ends, projection of stage into room, etc., and we have an available volume of 230,500 cubic feet.

Under the above conditions the floor area for a single person will be

$$\frac{8831}{1600}=5.52 \text{ square feet;}$$

while the volume for each person will be

$$\frac{230500}{1600}=144 \text{ cubic feet.}$$

Lighting apparatus consists of 112 gas burners, each consuming 4 cubic feet of gas per hour. The consumption of gas per hour, then, is $112\times4=448$ cubic feet.

As previously shown a cubic foot of gas in burning produces 0.43 cubic feet of carbonic acid. Production of carbonic acid per hour from combustion of gas, therefore $=448\times0.43=192.6$ cubic feet. Each person produces 0.6 cubic feet of the same gas per hour. Production from respiration, then $=960$ cubic feet. Total amount of carbonic acid per hour $=960+192.6=1152.6$ cubic feet.

Assume the heat from a single person to be 475 units per hour, and the heat from combustion of a cubic foot of gas to be 750 units: We have

$1600\times475=760000$ heat-units per hour from the audience; and from the lights $448\times750=336000$ heat-units per hour, making a total production per hour of

$$1096000 \text{ heat-units.}$$

Suppose the air supply to be at a temperature of 62° and equal to 2000 cubic feet per hour for each person, the amount per hour will be

$$1600\times2000=3200000 \text{ cubic feet.}$$

The amount of heat required to raise a single cubic foot of air 1° at an original temperature of 60° or thereabout is

$$0.0766\times0.238=0.01815 \text{ units.}$$

The increase in temperature of the air supply, due to the heat from lights and people will be found as follows:

$\frac{3200000}{13}=246154$ pounds of air per hour, and

$\frac{1096000}{246154}=4.4=$ number of heat-units to each pound of air.

A single heat-unit raises one pound of air through

$$\frac{1.0000}{0.238}=4^{\circ}.2 \text{ actual temperature.}$$

The increase of actual temperature of whole supply will therefore be

$$4.2\times4.4=18^{\circ}.48.$$

On the supposition that the adjustment of supply to exhaust is properly made the air on leaving the hall must have a temperature of

$$62^{\circ}+18^{\circ}.48=80^{\circ}.48.$$

The exit per second will equal

$$\frac{3200000}{3600}=888 \text{ cubic feet.}$$

Assuming the velocity of exhaust in vertical flue to equal 20 feet per second, we have for area of said flue

$$\frac{888}{20}=44.4 \text{ square feet,}$$

or what would be preferable, two flues each with area of 22.2 square feet. Assume farther that the inflowing air shall have a velocity of 4 feet per second, the area of inlets will be

$$\frac{888}{4}=222 \text{ square feet.}$$

Assume a sectional area of 6 square feet for each inlet, and

$$\frac{222}{6}=37=\text{number of inlets.}$$

Assume again that velocity in outlets is 6 feet per second,

$$\frac{888}{6}=148 \text{ square feet}=\text{area of outlets.}$$

Also let area of each outlet be 6 square feet, same as inlets

$$\frac{148}{6}=25 \text{ number of outlets.}$$

The discussion has taken no account of the increase in volume due to the increase of temperature. Were such account taken the dimensions of outlets would be somewhat increased.

The number of changes per hour under the above conditions may be determined by two methods, either by dividing the supply per hour for each person by the volume for each person, thus,

$$\frac{2000}{144}=13.9$$

or by dividing the total supply per hour by the total available volume of the hall, thus,

$$\frac{3200000}{230500}=13.9$$

giving the same result in either case.

The discussion has also proceeded upon the supposition of no loss by conduction and radiation. We will now consider the modifications of the above calculations due to these sources of loss.

The hall is warmed by steam, direct radiation. Number of sets of radiators 18, each having an area of 60 square feet. Total radiating surface, therefore,

$$60\times18=1080 \text{ square feet.}$$

Amount of space heated by one square foot of surface equals

$$\frac{230500}{1080}=213.4 \text{ cubic feet,}$$

which is certainly ample for the coldest weather likely to be experienced here.

To find loss by conduction through walls and windows, we will assume inside temperature at a mean between temperature of entering air and temperature to which it is raised by heat from persons and lights, or say at 71°. Outside temperature is taken at 32°. The outside exposures are three in number, two sides and one end. We will consider the loss of heat only from the three outside exposures, neglecting the protected end, ceiling and floor.

Area of the three exposed sides exclusive of windows=6306 square feet. Area of windows=1696 square feet.

Taking formula (18) we have for the loss through walls exclusive of windows:

a = 6306 square feet,
t' = 71°,
t = 32°,
k = 0.095,
d = thickness of wall=2 feet,

then

$$Q=0.095\times6306\frac{71-32}{2}=11487=\text{num-}$$

ber of heat-units conducted away through walls per hour.

For windows we have

a = 1696,
t' = 71°,
t = 32°.

Experiments have been made in the case of window glass, showing that neglecting d the value of k varies from 1 to 3. We will take it for this calculation at a mean value, namely, 2. We have therefore

$$Q=2\times1696(71-32)=132288.$$

hence the total loss by conduction per hour under the conditions of temperature assumed is

$$11487+132288=143775 \text{ heat-units.}$$

Suppose however, instead of allowing 2000 cubic feet per hour for each person, we take what is more nearly the actual supply, that is 850 cubic feet. With that assumption we have a total supply per hour of 1,360,000 cubic feet. The amount of heat from people and lights will be the same as before. Assume temperature of air supply at 62° as previously. Amount of air in pounds

$$\frac{1360000}{13}=104615.$$

Number of heat-units per pound of air is

$$\frac{1096000}{104615}=10.4,$$

making an increase of actual temperature of

$$10.4\times4.2=43°.68.$$

Assuming no loss by conduction and radiation, and the temperature of the outflowing air is

$$62^\circ + 43^\circ.68 = 105^\circ.68.$$

In fact loss by conduction, &c., increases in proportion as inside temperature increases, consequently the amount conducted away will be correspondingly greater than in the preceding case. Observation will show air in upper part of room to range from 85° to 95°. The writer has on several occasions observed it higher than 80° in lower part of room, and this too with windows open, and outside temperature at from 35° to 40°.

A partial exhibit has now been made of the main principles underlying the Mechanics of Ventilation. In conclusion it will be well to collate principles and facts for convenience of reference.

1. Perfect ventilation is hardly automatic. A certain amount of attention is necessary to keep any system in working order.

2. Ventilation by draught is preferable to ventilation by forcing air in, it having the advantage of supplying fresh air as fast as foul air is removed, provided of course the design is properly carried out.

3. The vacuum may be produced in the case of rooms or suites of apartments by fire-places or by small flues properly connected with the rooms at or near the floor, in which gas jets are kept burning whenever ventilation is needed. In large buildings, however, the vacuum will of necessity be produced by vertical shafts designed in accordance with the principles herein contained.

Corollary.—Where gas jets are used the amount of heat from one cubic foot of gas may be taken as given in the beginning of this paper. Knowing the amount of air to be carried away per hour, the size and number of burners is easily found.

4. Perfect ventilation will cost something. To lift a thousand pounds of air 50 feet, requires exactly the same expenditure of force as to lift a thousand pounds of iron, or any other substance 50 feet.

Corollary 1.—Since to ventilate a building means there is work to be done, and consequently an expenditure of force necessary, any system professing to ventilate without such expenditure of force can hardly be other than a failure.

Corollary 2.—Careful study of the matter shows the importance of reducing friction in flues, ducts, elbows, etc., to a minimum; otherwise a large percentage of force will be expended for that purpose only.

5. Ventilation is a branch of mathematical investigation, actual construction should, therefore, be preceded by careful calculation.

6. Heating is a branch of ventilation and should always be considered in connection with it.

Corollary 1.—In warming by heated air the current should be introduced at or near the ceiling. It is a violation of first principles to introduce it near the floor.

Corollary 2.—In designing a system of warming by heated air, provision should be made for introducing cold air in connection with the warm current, in order that the air may not enter the room at too high a temperature.

Corollary 3.—The cold air currents as per Corollary 2 should be under perfect control as well as the heated currents, in order that the proper temperature inside may be maintained under every variation of outside temperature.

Corollary 4.—Warming by steam or hot water, by a combination of the two, or by steam and hot air combined may all be exceedingly healthful, provided adequate ventilation accompanies them. These methods are all reasonably economical as regards consumption of fuel.

Corollary 5.—Warming by ordinary stoves cannot be considered desirable except on the score of economy. The objections to their use are :

(1) Difficulty of producing ventilation in connection with the warming.

(2) Cast iron, the usual material for their manufacture, when red hot allows the gases, carbonic acid and hydrogen to pass through into the rooms.

(3) The heat is irregularly distributed causing great variations in temperature in different parts of the rooms.

7. It should be understood that ventilation by opening doors and windows is nearly as bad as no ventilation.

8. In large buildings much satisfaction, and what at the present time is of the greatest importance, much valuable information could be obtained by putting up in connection with such a system of ventilation by the vacuum method, apparatus for determining and accurately

registering the results under varying conditions of outside and inside temperature. An anemometer in the main shaft with electric recording apparatus in the superintendent's office, would show at any instant exactly the condition of the ventilation. In this way regularity of action will be assured, and at the same time a collection of data of the greatest value in future building operations can be made.

Farther, in conclusion it may be stated that, so far as the writer is aware, there is nothing in the present paper especially original with himself, except possibly one or two of the combinations of formulæ, and the only reason that can be urged for supposing originality in even that direction is the fact that such combinations have never fallen under his observation. The object of the paper therefore, is to partially systematize our knowledge of this important subject, at the same time putting that knowledge in a form for convenient use. Such systematization is necessarily crude owing to the undeveloped condition of the subject.

The expert will recognize at a glance the great indebtedness to the works of Weisbach and Deschanel.

General Morin's paper before referred to contains valuable information upon this subject, and has been frequently drawn upon in the preparation of this paper.

ON THE EXCLUSION OF SEWER AIR.

By RICHARD WEAVER, C. E., F. C. S., Sanitary Surveyor.

From "Journal of the Society of Arts."

THERE are various reasons why the more perfect exclusion of sewage emanations from the interior of buildings has not attracted that close attention of sanitary engineers which the national importance of the subject demands.

The magnitude and the emoluments of out-door works entirely dwarf the comparatively trifling matter of the internal arrangements of domestic drainage, and the indifference of the public to the subject, combined with the simplicity of faith and unwarranted assumption that things out of sight are right, have certainly not hitherto been very encouraging to the reformer. Those gentlemen who are professionally engaged under sanitary authorities well know the repugnance evinced by tenants and landlords to carry out valuable suggestions, incurring, it may be, trifling expense, often, indeed, in instances where grievous defects are long tolerated and the benefits are mainly for those who prove the most obstructive.

In another direction the same apathy to the advantages of improvements beneficial to health are shown, from a source altogether devoid of official character. A gentleman largely interested in property has had some houses placed in a sound condition in respect of drainage and water supply, not with a view altogether of deriving benefit but more as a test to gauge the susceptibility of his *clientèle.* But he informs me that the great majority evince no interest in the affair, and are more concerned as to the shade of the papers and the tint of the paints, than in the quality of the respirable air and the drinking water. Nevertheless, as the schoolmaster is abroad, and the diffusion of the laws of health, if slow, is established on a sure basis, this impassiveness must cease ere long. It is not far back since a want of knowledge of the principles of hygiene operated as a source of retardation of advancement; but the abundance and general excellence of the literature of sanitation, and the frequent reference to such subjects in the columns of the daily press, sufficiently disposes of the plea of ignorance; whilst a gradual awaking of the public sense to the imperfections of current modes of treating domestic offices, especially when that is combined with an acquaintance with the fact that the remedies are not expensive, in ameliorating existing buildings, which in new structures amounts to nothing additional upon the usual charges, will unite in good season to leaven the present universal indifference. It is a sign of the times, that will be earnestly read by the people, that a most eminent and popular statesman, rises in his official capacity, and from his exalted position declares to the world, that the sanitary condition of this metropolis and of the country is not satisfactory—a fact long known, of course, to those gathered here in conference, and which will not be lost upon the masses to whom the information is conveyed. I would venture humbly to go further, and to say, and say it boldly, that the condition of many of the metropolitan buildings, against which it may be thought presumption to raise a breath of aspersion, are in an eminently unsatisfactory condition, and where the more urgent is the necessity of observance of sanitary laws, the less they are in practice.

An enterprising, plodding, prying, hard-working Charles Dickens might open out a revelation of evil influences within public institutions and private dwellings—existing for the most part in blind faith, which seems to me inexcusable—as would, beyond doubt, shock the public sense of the proprieties.

Let me ask whether it is known to be anyone's business in particular to be intelligently acquainted with the state of the water supply, and the condition of the local drainage, with the means adopted against the entrance of sewage air? Let me go further, and ask whether it is anyone's particular duty—not nominally, but actively—to become responsible for the satisfactory quality of, and to maintain the breathable air and drinkable water of any establishment reasonably clean and wholesome?

The ladies of the duster and knights in livery are held strictly accountable for the satisfactory appearance of the furniture, floors, and stairs, with other needful matters of the household, but in the best ordered mansions that not unimportant item, the domestic water, is left to the careful attention of cockroaches and spiders, buried away in some dark and for the most part barely accessible position, revelling in an atmosphere replete with exhalations from the common sewers, conveyed through the waste pipe of the cistern. The general atmosphere of the house, too, being a cheap commodity, and apparently not worth consideration, is left to chance for governance.

True, it is charged with organic pollution of cloac, an origin freely admitted through the numerous vents in the scullery, baths, water-closet, and slop basins, mingled with the vapours of cooking viands, and flavored with a *soupçon* of burnt gases. But what of that? Such matters are universal; we rather like it; it keeps us warm during the prevalence of these cutting east winds, and why should our children not do likewise?

Unclean and expired air, with damp, are accountable for 100,000 annual deaths in the shape of bronchitis and consumption. Unwholesome air and polluted water dispose of another 150,000 or so of lives carried off by zymotic diseases. But then this may be a useful provision, and tends to allay the too rapid growth of population, and as every man dies sooner or later, some folks think the sooner the better, and so, upon the whole, things are best left alone. A great deal has been said and written upon the potency of fixed and volatile organic substances in the stages of putresence upon the mortality of the country, much of which I agree with. I know of no one who is more keenly sensitive than myself to the inconvenience of breathing tainted air or drinking foul water, but I cannot approve of the often-uttered expressions by men, even of experience and influence, such as the "deadly sewer-gases," and "death in the cistern." Their opinions, given with good intentions, defeat their object, for people are apt to think that the shades are drawn deeper than requisite, and if the dangers were so great as presented, there would be fewer of us left to talk about it, considering that we all take our peck of dirt in the form of aërial and solid sewage rather frequently. It has been my privilege for some years past to describe the sanitary conditions of many provincial towns in the columns of a journal long interested in such matters, and I think that our country friends will not be averse to a description of the shortcomings of metropolitan household arrangements, for grievous although the former may be, they are at least equaled in the latter.

A careful examination of the substantial buildings in London in any direction reveals glaring evils, probably causing much injury in the aggregate, although the average death rate would seem to indicate that the general damage sustained is not great. From this knowledge, gleaned during many years' investigation, and an acquaintance with the fact that we are, whilst within doors, always breathing a sewage-tainted atmosphere —and this building is no exception—I am convinced that the actual injury from infringement of the laws of health is much less than is often represented to be the case.

There are congregations of populations within my knowledge who regularly consume their own filth, and drink up the fluid refuse with the water. Placcs where the receiving cesspits are in such near juxtaposition with the domestic well that the water lines become identical, and intermittent exchange is maintained, and yet many of these places are considered healthy, and figure so in the Registrar's returns. It is only when the virus of specific forms of disease is introduced that death descends, and, like the cock-sparrow on the ant-hill, gobbles up the people right and left and round about him.

An example is within recollection of a small town where nothing more loathsome can well be conceived than the social habits of the inhabitants, where, for 14 years, the community enjoyed good health before an imported epidemic attacked more than a third of the population, of whom many died. There are numerous instances where families and communities exist under the most unfavourable circumstances; but such is the repellent power of the human system, that evils are borne with year by year, apparently without injurious results, and this is probably a reason why so many people are indifferent to and actually resist improvements. Englishmen are fond of games of chance, and there is a charm in taking the odds against the risk. Ten men may live to an advanced age, they may breathe sewer air and drink sewer water every day of their lives, and in the end not more than two of them die of diseases which, as we now reckon such things, can be attributed to disregard of the laws of hygiene. The danger is nevertheless, considerable, more especially in young children, and I think it may be within the experience of most of us that whole families have been swept off by causes quite preventable. In again addressing the public upon this subject, I would refrain from calling attention other than in passing to the dangers to health and the irritation to temper, incurred by living under insanitary conditions, and I would rather take up other ground, and appeal to the high sense of the proprieties characteristic of English women and men to the high value attached to the exercise of scrupulous cleanliness; and in doing so, if I wound the sensitivenesss of some and raise any feelings of disgust, it must be borne with me that I simply state the truth of facts —I do not create them.

The remedy for prevailing errors lies in a recognition of their existence, less by legislative action than by reform within the bosom of every family without distinction, and it should rightly begin with those who sit in high places, for my range of observation, if limited, is sufficiently wide to enable me to say, with some confidence, that the larger and more important residences are relatively in a worse condition than the humbler abodes of the working classes, causing necessity for a more frequent change of air and locality, due to the foul state of the atmosphere of metropolitan mansions, a condition of things rarely suspected, but universally existing. If it may be said, there is not much detriment to health from the present state of dwelling houses, if viewed from the point presented by reference to the mortality rates, I still venture to think that much of that indefined indisposition of families—headache, nausea, dyspepsia, lassitude, and such small complaints, are often created by breathing the foul atmosphere of the house, a foulness, most likely, not perceived by the usual inmates. At all events such effects are produced upon myself and family, and I observe that children become rosy-cheeked and firm fleshed when they are supplied with good fresh air whilst in the house or school—not draughty air—and especially in the dormitories. Possibly, the time even may come in the future, when the sanitary man will be as freely consulted upon the remedy of prevention of such matters as our medical *confrêres* now are for their cure.

There is no occasion for me appearing rude, and I have no intention of being so, in the following criticisms, but I am induced to submit them, so as to bring home to every one the fact that, notwithstanding we pride ourselves on our decency and cleanliness, and our inclination to set ourselves up as patterns to Continental neighbors, I say, the fact is, we are a dirty and an unclean people. Indeed, could we but see ourselves as we really are, I think we might even admit we are a filthy folk, and deserve to be scourged. There is a handy phrase that pointedly excepts the present company always, but I will take no shelter under that conventionalism, and I say that the charge is against me and you, and I believe every family in the kingdom. Some medicines are disguised with saccharine matter of less or more cloying sweetness, but I have no sugar to offer, and you must take my facts as I find them.

I repeat this in letters large writ, and hung up that those that run may see and read, for the proceedings of this Conference are not confined within these four narrow walls. If a hound strays from our heels and laps up the soil from the street, left by neglected children, he is switched and called a brute; and if a servant were guilty of polluting the domestic beverages with the contents of the slop-pail, the act would be considered a heinous sacrilege. But practically we do this every day, and what do we care when the foulness of our water-closets is injected from the containers or the soil pipes to the cisterns, and mingles with the water we consume, which I am able to say, from personal examination of many hundreds of houses, actually occurs in nine out of ten residences not of recent erection. We boast of our civilization, and send forth missionaries abroad to teach the heathen. In the good old book of Leviticus are excellent sanitary morals, but do our ministers cull from them their full excellence? It is often alleged by speakers, and I have heard it said in this hall, that the errors of old buildings do not exist to nearly the same extent in new erections, and especially greater improvements are effected in the storage of water. It would be interesting to know how this conclusion is arrived at, whether as an effort of observation, or conviction, or mere hearsay. As regards the latter, I know of no direction in which fallacy is more persistent than in respect to house sanitation.

There is a fixed determination amongst house-holders that things must be right, and that they are right. But a little reflection in the mind of every responsible head of a family will show there is no special dispensation, and careful search would show that gross defects exist.

A few days ago, I made an inspection of a medical gentleman's house—a man who is a frequent attendant upon social conferences, and well read in matters of hygeia. Well, when I came to the water cistern, he assured me the waste pipe delivered into the ground a little below the surface, but as I make it a point to take nothing for granted in my examinations, and must have personal demonstration, we opened out the subsoil and found the overflow orifice discharged into a drain directly communicating with the sewer up which air of a fetid character arose. In point of fact, the waste pipe acted as the upcast shaft to ventilate the house drains and public sewer, the delivery taking place in the cistern an inch or two above the water line.

Of course, seeing the defect himself—one of many—the fact was confirmative, otherwise I think it would have been difficult to convince this gentleman of the nuisance. In new residences there is an average improvement upon the water service storage, for I find that seven houses in ten only are contaminated locally by connection with the sewerage, or with facilities for the absorption of gaseous sewage at some point.

If a guest enters a drawing-room with boots spotted with honest mud, he is, peradventure, looked upon askance; but it is a matter of no moment that the host immediately charges his lungs with abominations vomited forth from the common sewers, through defective closets and scullery sinks, with which the atmosphere of the house is tainted, curiously hidden in vapor of preparing viands ascending from the kitchen, which no mansion, however modern, seems without. We are, forsooth, a peculiar people, and our notions of decency seem sadly erratic.

It is a singular thing, and, perhaps, suspected by few minds, that the conditions essential for the enjoyment of sound, robust health, least exist in localities where they are most required; and I put it to you as the fruit of observation, that our hospitals, as types of public institutions, are the most indecent in respect of their sanitary measures; whilst the gin palaces, as representative of another class, are the best. In the first, I include public buildings, houses, churches, clubs, schools, hotels, and coffee-houses, with many others; and, in the second, the business parts of butchers', bakers', greengrocers', confectioners', with some other shops. Basing the calculation of averages from an examination of several hundred buildings, made within the last year, I am in a position to say that 99 per cent. of metropolitan dwellings are polluted very seriously by the admittance of sewage air through the various openings for the removal of liquid refuse. In many are no trapping arrangements whatever for keeping back aërial sewage, which flows uninterruptedly into the house, often, indeed, to the extent of tens of thousands of cubic feet per day, and the odd thing is that this is always unsuspected by those who should know it, and it is not thought of much consequence when pointed out. But let me ask whether this is common decency to charge the respiral air of a mansion with the exhalations of filth, and to breathe the same, whilst professing some sentiments of cleanliness and refinement? If there were any adequate means of air ventilation of houses, the nuisance now enveloped in the vapors of cooking would not be so great; but there is no systematic attempt and the internal atmosphere, from basement to garret—and the higher up the more stagnant—is generally fetid with foul air, more especially during the nocturnal hours. So general is this defect, that, on admitting a guest, he is met at the front entrance of the establishment with a gush of mixed air, tainted with smells from the kitchen and drainage evolutions, which a trained nostril has no difficulty of identifying. The proper ventilation of houses—that is, the introduction or pure, fresh air without draughts, and the speedy removal of consumed air, so as to maintain the inner atmosphere in a condition approximating to the outer air—has occupied my attention during many years, and as the objects sought for are accomplished, I hope soon to find an opportunity of making known the system, which the scope of this Conference does not now permit.

Some recent discoveries relative to the state of certain modern—I was going to say model—Government offices, the Mansion-house, and others, have been looked upon as alarming, if I understand the matter, but I can say with some knowledge that, if the published accounts represent all the defects found, then the efficiency of those buildings is certainly above the average; and as there seems no palpable reason for their being so, I am constrained to think it is just possible that other indecencies might have been brought to light, if the search had been more closely followed.

Specimens of new dwellings were described in the *Journal* of this Society a few months ago, and since then I have seen others equally defective and indecent, and the samples now furnished refer to older buildings, of jottings from my note-book on the spots, and may best serve to illustrate the average condition.

In a mansion at Regent's-park, which the owner considered perfect in its sanitary appointments, and with that object had expended considerable sums, deleterious gases from the drains entered to the extent of several thousand cubic feet per day. The service cistern contained a filter covering the whole of the bottom, so that all water withdrawn for supply underwent the formula of cleansing—even that taken for water-closet use; but, with the logic of irony, the waste pipe discharged directly into the public drains, the trumpet pipe communicated openly with the pan of water-closet, whilst each action of the valves squirted a jet of water from the closet container into the cistern of potable water.

In a West-end club, noted for the scientific attainments of its members, I found the interior of the premises in direct contact with the street sewer, from which some portion of the air supply was derived with which the visitors are regaled. At all events, through a single untrapped aperture, mephitic vapors flowed at the computed rate of 12,000 cubic feet per day. Most of the water cisterns are polluted in various ways, the greater part of the waste pipes going right into the drains, whilst one cistern supplying the cook with his requirements, is flavored with an injection of water shot up from the pipe serving the water-closet, each time that necessary convenience is used by the servants.

Into a well-known restaurant near Regent-street there is as much sewage air entering as would seem sufficient to ventilate the sewerage system of a small town, seeing that in the cellarage alone it is estimated that the intake of foul gases equals 30,000 cubic feet; and the exhalations from the closets, urinals, sinks, and lavatories add to the sum total. On inquiry from the superintendent of a department, the atmosphere of which appeared highly charged with impurities, what was the tone of health of the staff, and whether any of them died? He said none of them stayed long enough for that. Just by way of taking off the rawness of the gaseous sewage the establishment arrangements are very complete for distributing the kitchen fumes and the burnt air from probably thousands of gas jets throughout the building, and for this liberal supply of lung food I believe the management most considerately make no charge upon the guest.

In a block of substantial houses, which were in perfect sanitary efficiency—so the architect and surveyor assured the owner for whom he acted—I found in each of them sewer air of a peculiarly fetid character, entering at the rate of several thouand cubic feet *per diem*, distributing its noxious qualities amongst the domestic water and air.

A very fair example of the condition of a London hospital is found in one upon which I lately reported, and the medical staff should be better judges than me whether the arrangements are conducive to the health and convalescence of patients. The drainage system ramifies below the basement floor the length of the hospital, rising by vertical pipes to the wards and upper premises, and between the sewer at one end and the wards and chambers at the other, there is, through the elongation of pipes, no trap intervention whatever, so that perfect channel communication is established between them. The fact is, as no air ventilation is provided, the deficient supply is augmented from the sewer, from which the soil, bath, and lavatory conduits act as up-cast shafts, thus ventilating the sewers into the hospital wards.

Some of the cisterns are also polluted by exhalations, one that supplies the dispensary, I understand, rather considerably so from the closet, as the motion of the lever discharges foul air in bubbles through the water stored in the cistern.

The saturated air of the hospital, stagnant from want of motion, with no apparatus for extraction, without inlets for fresh air beyond the usual crevices, is particularly unpleasant to sensitive lungs. Many examples might be furnished of similar examinations, but the faults are alike, differing only in detail, and the effects are not dissimilar, viz.:—indelicate adulteration of air and water, the cheapest but most necessary elements of existence.

Before passing on to the practical object of this paper, the means available for effectually avoiding the current nuisances common to all dwellings, I will intrude upon you one more illustration of the indecencies of modern living, taken from the residence of a sagacious, well-informed gentleman of eminence—who, always interested in social subjects, is not unknown in this hall—which I was invited to inspect so recently that the evils pointed out still remain unameliorated. In the first place, in the kitchen, as there is no open fire-place, but close stoves and hot plates, the whole of the volatile culinary products are discharged into the basement, the bulk of which ascend to the upper premises. There are no ventilating appliances in this palatial establishment, but a fair amount of air of a sort is procured from outside the building, conducted through the yard grids, and thence by the scullery-pipes to the several passages of the house. Another supply, voluminous in quantity, is emitted from the soil-pipes of the water-closets, another source being through the waste-pipes of the service cisterns. The air of the whole building—of the magnificent chambers of great length and height, and of the upper offices, is singularly stale and unpleasant. Nevertheless, the servants express perfect satisfaction, and profess to enjoy excellent health; but, if this is so, are the conditions of living proper—are they decent even?

It is many years since I became acquainted with the inadequacy and insecurity of the usual hydraulic traps applied to house drainage to stop the back flow of sewer air. At the most, and under the best conditions, assuming perfect joints, and with sound materials, they merely obstructed the rush of air; the passage from the drains to the house took place more slowly and insidiously. Well, after laboring for some time for improvement, I adopted a simple device formed out of a siphon glazed ware pipe, with an opening at the socket, and communicating with the ground surface for the entrance of fresh air, and then, by the aid of openings at the tops of stack, bath, and soil-pipes, to keep up a system of natural and self-acting air circulation throughout the drainage; so that any passage effected through the porous pipes, joints, or traps—very little, perhaps, by reason of the equal tension of the air within and without the pipes—would be robbed of virulence, because the air, being ordinary atmospheric air, and not sewer emanations, is quite harmless. The idea was taken from—and, in fact, was an attempt to adopt underground—the system not much practised in London, but common in provincial towns, of severing connection with the scullery, by delivering the pipe into the air over a trapped grating, but which had never been carried into practice, so far as I know, with water-closets, nor with drains passing through the house.

As the arrangement has become known through the professional press, and by the proceedings of a kindred association, and possibly is known to you, I need not further describe it. I may, however, say that with some years' experience of its working it gives me every satisfaction, as it thoroughly effects complete severance between the house and sewer, preventing the admittance of any foul air and pollution to water, the whole being accomplished at a cost which, I think, is not immoderate considering the substantial benefits derived; and that it does not generally exceed £5 per house in occupied buildings, in new structures practically amounting to nothing extra upon the usual drainage outlay.

Some question has arisen as to the feasibility of implied action of the siphon system of ventilating and trapping drains and sewers, which is best answered by stating the result of many hundred examinations. The air circulation is produced by compound causes—first, we have the currents due to the natural mobility of the air, as exemplified in all vertical shafts open at each extremity, and well illustrated by a chimney flue without a fire. This motion is accelerated by the passage of water down the pipes, creating a reversed aërial current, and again by the warm discharges from the kitchen. The upward flow is augmented by the heat absorbed from the sun's rays by the ventilating pipes. But the most potent agent in keeping up the circulation is the wind blowing squarely across the mouth of ventilating pipes, creating an exhaust and consequent upcurrent, for it is old knowledge that the rapid passage of a fluid across the orifice of a tube reduces the tension within that tube.

Under exceptional circumstances and local obstructions, there is occasionally a down draught through the pipes, which act as the long leg of the siphon system of ventilation, but this is of no moment, for the outlet being lower down and outside the building, the aerial discharge takes place there after sweeping through the drain pipes, and is generally devoid of smell, for I find that, after a few weeks, operation with fairly laid glazed pipes, although they may have been down for years, the oxidising effect of the continuous body of fresh air passing over the surface deposits within the pipes, entirely consumes the putrid matters. The currents however, are generally ascendant, and deliver at the roof level. A series of observations conducted under varying conditions and localities determine the average velocity of flow at three to four lineal feet per second in calm weather, which increases to nine feet, and often 12 feet with a strong wind. And it is to this severance of the house from the sewer by the water seal of the siphon trap, in conjunction with the sweeping air currents maintained through every drainage pipe of the house, that I depend for clean air and clean water within the building, and is most certain in its action; whilst the means I have devised for the effective disposal of the locally generated exhalations—the expelled breath of the body, the burnt gases, the emanation from the walls, paper, furniture, and miscellaneous effects stored upon the premises—admitted on all sides as an object well worthy of attention—as I have said, I will take another opportunity of describing.

ON FROZEN DYNAMITE.

By Capt. PHILIP HESS.

From Selected Abstracts, translated for the Institution of Civil Engineers.

Nobel's blasting oil belongs to the numerous class of artificial products, extensively used for industrial purposes, of which the physical and chemical qualities are scarcely understood. But the small number of experiments which have been instituted to bridge over the gaps in the knowledge of so dangerous an explosive can hardly be a matter of surprise.

The difficulty of such a study chiefly arises from the fact that it is rarely possible to time the stages in the combustion of the explosive.

The use of Nobel's material, and especially that of nitro-glycerin when frozen, has greatly added to the above difficulties. The freezing point of nitro-glycerin varies between 39° Fahr. and 53.6° Fahr., and this arises from the differences in manufacture and the varying nature and action of the oil under cold. Respecting the first cause, the Author's investigations show that the oil usually sold never contains 18.5 per cent. of nitrogen, the quantity necessary for tri-nitro-glycerin; therefore in the oil at least two kinds of nitric ether must be assumed to be present; but, on the other hand, there may exist all the three ethers theoretically possible, viz., mono-, bi- and tri-nitro-glycerin; this fact will explain some peculiarities which would otherwise seem paradoxical. The Author begins by tabulating briefly the observations made in respect of this matter.

The blasting oil, when free, will bear sometimes for days low temperatures without solidifying. Samples from Nobel's factory in Zamky remained liquid in the Military Laboratory under temperatures varying between 32° and 17.6° Fahr.

Blasting-oil samples would not, it was found, solidify when exposed even for hours to cold varying from 10° to 5° Fahr., according to Champion. Dr. Gladstone has noticed that nitro-glycerin, when subjected to cold produced by strong carbonic acid and alcohol, will only become thick or glutinous without freezing.

From experiments in the Author's laboratory, it was found that in a considerable quantity of nitro-glycerin, exposed for several days to a temperature between 50° and 32° Fahr., some crystals had detached themselves, remaining for a time floating in the oil, which did not itself congeal.

Contact with absorbent substances, such as a silicious marl, undoubtedly hastens the freezing of the nitro-glycerin in temperatures below 53° Fahr.

Nitro-glycerin, when fluid, renders the silicious admixture pellucid, but when frozen it loses this power; so that thoroughly congealed nitro-glycerin appears no longer flesh-colored or brown, but white. This peculiarity indicates whether a dynamite cartridge is frozen completely. For this it must be broken, which, according to Mowbray, can be done without danger.

If a cartridge is exposed for a day in a parchment box the oil will completely freeze in a temperature below 10° Fahr., if the cartridge has not a greater diameter than 1 inch.

If dynamite is spread over metal plates 1 millimèter thick, half an hour's exposure produces solidification. When blasting oil and nitro-glycerin powder are frozen, and then subjected to shocks which would explode them in a softer state, peculiar variations ensue. The general result of the trials showed that nitro-glycerin and blasting preparations formed from it become less sensitive to mechanical shocks directly the nitro-glycerin contained in the preparation is completely frozen. During the experiments made by the Military Committee, the preparations in question (silicious dynamite being chiefly dealt with when the temperature of the surroundings did not exceed 10° Fahr.) were cooled by artificial means (mixtures of snow and common salt being used), the temperatures obtained being between 1.4° and 69° Fahr., and at which the samples were kept and completely frozen.

Under these conditions the silicious dynamite showed the least liability to explode when tried with a capsule filled with fulminate of mercury, employed to bring about combustion with safety; for the similar reason a cartridge made of gun-cotton and nitro-glycerin was effectively made use of.

With dynamites composed of absorbent matters having a combustible tendency, it was noticed that the diminution in liability to explode was rather lessened when frozen; for example, this was the case with Nobel's dynamites Nos. 2 and 3, where the absorbent material is composed of wood and saltpeter, or with the gun-cotton and dynamite before alluded to. Cartridges of this last-mentioned material exploded, when not frozen and frozen, with the same effect, from the initial shock of the mercury capsule before referred to, when this last contained 9.25 troy grains of chlorate of potash.

Silicious dynamite, when fired at, and soft dynamite, when freely exposed, would not explode when struck with a bullet from the old form of rifle at a distance exceeding 2,500 paces; at 2,000, combustion was produced. Frozen dynamite, however, would not explode even at sixty paces. The Author gives details of the trials by striking or ramming dynamite samples, which were carried out by the Military Committee.

The various preparations were in layers of one millimèter high each, and placed on a plate of 1.34 square centimèter, where they were struck perpendicularly above and below by a steel hammer, the force of the blow being regulated by the amount necessary to explode the sample. Silicious dynamite exploded, when soft, with a blow of 0.75 kilogrammèter; but when frozen this force was increased to one kilogrammèter, and one blow sufficed. Under similar conditions gun-cotton dynamite, when not frozen and frozen, required blows of 0.5 and 1.25 kilogrammèter respectively. With nitro-glycerin powders in a transition state of freezing, the force of the stroke producing combustion is not sensibly increased until complete congelation sets in. With samples in a transition state, explosion is produced at a second or third blow, when the force is less than would be required to effect combustion at a single stroke, and these light blows may be repeated until the strength of the preparation is so much reduced that no result after a time ensues. The limit between explosion and non-explosion is considerable; but when in a soft state the tendency to combustion is more defined.

The results of the Committee's trials are shown in the following table; the weight is eleven lbs., the dynamite being not frozen:

Fall,	19.7	inches		Explosion.
Fall,	15.7	"		"
Fall,	11.8	"		"
Fall,	7.9	"		"
Fall,	5.9	"		"
Fall,	5.9	"		"
Fall,	5.9	"		"
Fall,	3.9	"		No explosion.
Fall,	3.9	"	2 blows...	"
Fall,	3.9	"	3 blows...	"

Experiments with dynamite in a transition state, between liquid and frozen:

Fall,	19.7	inches		Explosion.
Fall,	15.7	"		"
Fall,	11.8	"		"
Fall,	7.9	"		"
Fall,	5.9	"		"
Fall,	3.9	"		No explosion.
Fall,	3.9	"	2 blows...	Explosion.
Fall,	2.0	"		No explosion.
Fall,	2.0	"	2 blows...	Explosion.
Fall,	1.6	"		No explosion.
Fall,	1.6	"	2 blows...	"
Fall,	1.6	"	3 blows...	"

The sensibility of frozen nitro-glycerin to mechanical shocks is shown by these experiments to be less than that of non-frozen nitro-glycerin. In the transition state, however, this sensibility is still further increased, especially in respect to repeated light shocks, such as those accruing through the operation of packing for transport; the experience both of the laboratory and the factory confirms this view.

In dynamite factories, most accidents

take place upon the filling of the cartridges with the nitro-glycerin powder, and when the temperature of the atmosphere fluctuates. To obviate this difficulty, the sheds where the cartridges are made are now heated with water, as the agent best fitted to distribute the temperature equably, the unequal heating caused by ordinary fires being a source of danger.

Mowbray first proved, on a large scale, that dynamite can be carried about, shaken, or broken, with impunity when in a frozen state; and experiments have been instituted to show whether powdered nitro-glycerin in a free state behaves differently from the liquid at low temperatures. Experience proves that with oil in a powdered form congelation takes place more easily than when free, owing, perhaps, to its greater cohesion. It must, however, be remembered that with oil combined with absorbent materials the latter are bad conductors of heat, and therefore a greater degree of cold is required to solidify them. Chemical analysis also proves that nitro-glycerins contains different ratios of nitrogen, and for that reason they have different freezing capacities. It should be remembered, too, that glycerin will only solidify under—40° Fahr., and that Ott lays down the highest point at which rigidity is possible at 53° Fahr.; it should, therefore, be allowed that the freezing point of any three possible nitrates lies between 53° and 40° Fahr. As a rule, in a mixture of different fluids, the one least sensitive to cold prevents the others from freezing, so that the zero point of tri-nitro-glycerins is reduced, according to the quantitative relations of the three nitrates. Several facts tend to elucidate the manner in which nitro-glycerin crystals are formed, which partially influence the cohesive strength of nitro-glycerins when in a powdery form.

Frozen dynamite, as a rule, secretes a portion of its oil when thawed; a portion collects in the lower part of the silicious marl and renders it extremely fatty, sometimes causing a deposit. When thoroughly thawed, re-absorption of this oil sometimes takes place. The above series of experiments show that dynamite, in a transition state, more easily emits nitro-glycerin when under pressure than it does in a soft condition.

If silicious dynamite be placed under water, the oil is separated, owing to the greater affinity of the water to the marl, and at the moment of separation it is more easily frozen, a fact which was proved in the laboratory.

When all these facts are combined, it seems to show a series of harmonising observations, such as the comparative absence of danger in handling large masses of frozen dynamite, its insensibility to the shocks from a gun or a hammer, and to the mechanical caloric impulses of the initial explosion. The result to be deduced seems to be, that with nitro-glycerin in general, and therefore, probably, in each of its three gradations, it is more indifferent to shocks, both mechanical and caloric or chemical, when in a frozen than a fluid state; although it remains to be proved whether it is so under all circumstances, in particular whether well-formed nitro-glycerin crystals are able to resist destructive impulses equally well in all directions of cleavage.

With the oils of commerce, as a rule, congelation takes place partially, and under the influence of lengthened cold the process of thawing is slow and gradual; in practice, it is difficult to determine whether nitro-glycerin powder is wholly frozen until it has been observed and exposed for a considerable time. Such portions of the oil as are frozen seem to alter the relations of cohesion of those portions still fluid with their absorbent material, so that this absorbent seems only feebly able to retain the fluid portion at the now reduced surface. These parts being no longer protected as before by their surrounding absorbent material, are merely loosely embedded between inelastic frozen hard particles, and evidently are more exposed to mechanical impulses than when the whole of the material is in a state of complete solution. The less the absorbent quality of the mixing powder, and the less it has taken up, the more its cohesion has been reduced by the presence of moisture, and, therefore, the more easily the oil will be secreted, and the greater consequent danger during manipulation; this may, it is true, be much reduced by a superabundance of the absorbent, and by well-dried powder mingled with it; but the danger will exist during the greater part of the cold season, owing to the single cohering particles having but a weak heat-conducting power. The Author insists much on the necessity for equally heating all laboratories where nitro-glycerin is handled. It is certain, from these investigations, that frozen dynamite is less liable to explode than when it is fluid, and that it is more sensitive still in a semi-condition, especially in respect of continuous mechanical impulses. The natural reticence of manufacturers has prevented authentic statistics of accidents and their causes being formed, and consequently a rational code of restrictions on the manufacture and transport of this dangerous material has not yet been devised; but the Author strongly urges the importance of the appointment of good technical chemists to superintend and inspect manufactories, &c., and considers the appointment of untrained government officials highly injurious to the public interest.

MOMENTUM AND VIS VIVA.

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IV.

V. THE MEASUREMENT OF CONTINUOUS FORCES.

MUCH confusion has arisen from the use of the word *force* in mechanics in several different senses. It is sometimes possible, even in science, to use a single word in various senses, making the context of any treatise show what meaning the word must have; but the use of the word *force* has been so thoroughly muddled that, even in reading authors usually clear, one is often at a loss as to the precise meaning, *if any*, which the writer has in mind. Sometimes *force* is defined as any agent or cause which produces, or in any way changes, the motion of a body; sometimes it is taken as something which can be measured in pounds, or their equivalents; sometimes as something to be measured in foot-pounds; sometimes something measured by the acceleration produced in masses, as by gravitation; and we have lately had the eminent Prof. Tait, in his lecture to the British Association, (see *Nature*, Sept., 21, 1876), define *force*, not as something to be *measured* by the rate of its doing work, but as being *itself* simply the *rate of doing work*. Prof. Tait attempts to justify this definition of the word by reference to Newton's *laws of motion*; but I cannot but think that the Professor, as suggested by W.O.P., (*Nature*, Oct., 26, 1876), has confused the idea of *measuring* something with that of *being* something. For he admits in the same lecture that the only allowable meaning of the word *force* in those laws is "any *pull, push, pressure, tension, attraction or repulsion.*" This must, by the Professor's previous statements, be capable of measurement in pounds; for he says that "the British *unit of force* is about the former weight of a penny letter—half an ounce." Can this weight be regarded only as a *rate of doing work?*

In the previous articles of this series I have used the word *force* sparingly, and where used without qualification I meant to leave no doubt that it was used as equivalent to *pressure* or *tension*,* measurable in *pounds.* I shall continue to use it with this meaning; and I hope to show that this is the meaning, and the only meaning, that the word must have in Newton's second law.

The fundamental standards which it is necessary to establish in a system of mechanics are the unit of time, the unit of space, the unit of mass of matter, and the unit of pressure or tension, called the unit of force. These *units* must remain invariable; or if any new unit for measuring time, space, matter or force is introduced, it must have a known and invariable ratio to the original unit. Any three of the four units may be taken at

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* Except perhaps in the phrase "correlation of forces," where I should possibly have done better to have said "conservation of energy."

our own pleasure, but nature imposes a connection between the fourth and the others.

The unit of time has been thoroughly agreed upon by all civilized nations, viz: the mean second. The unit of space is chosen by each nation to suit itself; in England the common unit of space is the foot. There are two distinct ways of deciding on the two remaining units of mechanics, either of which might be adopted. First, we may choose at pleasure a unit of mass, and then make the unit of force depend on this; or, second, we may choose at pleasure a unit of force, and then determine our unit of mass to correspond. By the former method, English writers conveniently take for the unit of mass, in mechanics, the quantity of matter in a standard piece of platinum, called the pound, which is carefully kept by the Government in a certain place, as a basis of reference in the comparison of quantities of commodities to be exchanged in commerce. Adopting this standard of mass, there is still some choice as to the most convenient way of making the *unit* of *force* depend upon the unit of mass. One way is to assume that the unit of force shall be the amount of pressure, due to gravity, exerted downwards by this unit of mass, when at rest in the specified place of keeping. We say *in this place*, because if the same piece of platinum were carried to a different latitude it would exert by gravity a different downward pressure. The pressure produced at the place of keeping may be taken on any spring balance, and the stress or tension shown by the index may be called a *pound* pressure; and whatever variations may occur in the weights of bodies at different localities, the *pound pressure* in any system of mechanics ought to be as invariable a unit as the standard piece of platinum. The context of any treatise must always show whether pressures or masses are meant by the term pound.

Retaining, as above, the standard pound of platinum as the unit of mass, late writers have introduced another way of determining the unit of force, and have sought to dignify it by calling it an *absolute* unit of force. They say, let us connect the unit of force with our other units by the following principle: The unit of force shall be *that force which by acting for one unit of time on one unit of mass shall give it one unit of velocity.* There is no objection to this method in itself, and there ought to be no difficulty in finding out accurately the relation between the unit of force thus determined, and the pound pressure before defined. The real trouble that the rule makes, is that many students are liable to get from it the idea that this new unit is a different *kind* of force from the pound, whereas the only difference is as to *amount.* The *kind* of force measured is identical, just as much as a gallon of water is of the same kind as a barrel of water; and in fact, this new unit of force is about the same fraction of a pound that a gallon is of a barrel. One English pound of pressure is equal to 32.1912* of the new units, and either the pound pressure or this new unit of pressure will produce motion, or be held in equilibrium, according to the circumstances of any special case.

The remaining method, which has long been taught, of fixing upon the units of mass and force for a system of mechanics, consists in choosing a standard unit of pressure, and then deriving the unit of mass from it. This is the method which I took at the beginning of this series of articles, though without claiming that it has much, if any, advantage over the previous methods. By this method we assume the unit of force to be the pressure due to gravity, produced at the appointed depository, by the quantity of matter known as the commercial standard pound. This pressure is to be observed on a spring balance or other dynamometer, and the position of the index marked as unity. Instead of afterwards retaining as the unit of mass the quantity of matter in the commercial pound, it is thought more convenient for some purposes of calculation in mechanics to take a different quantity of matter as the unit, viz: *that quantity of matter which, when free to move, and when acted on for one second by a pressure of one pound shall acquire a velocity of one foot per second.* From this definition it follows, as explained on page 129† of the current vol.

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* This is the value of g at London. Atkinson's Ganot's Phys., p. 13.

† If account is made of the variations of g, that explanation requires the pound weight to be determined always by the spring balance.

of this magazine, that g pounds of matter, weighed anywhere by a standard spring balance, is the unit of mass. This will be an unvarying unit of mass; since, although at various localities g will vary, it will vary in such a way that the quantity of matter which by its weight will pull or drive the balance to a pound tension, will be inversely as g.

Some writers, (Thomson and Tait, Nat. Phil., Vol. 1, p. 166) say that in the common system followed in modern mathematical treatises on dynamics, the definition of the unit of mass is such as to make it variable at various places. But if any writer on dynamics has given a definition that can be so understood, he has simply made a slip; for it would be too great an absurdity to *measure* dynamical quantities by a fluctuating standard. The formula $M=\frac{G}{g}$, by which in this method we determine *mass*, will give the same value for the same body at any place on the earth, if the value of G obtained by a standard spring balance, and the value of g for that place, be substituted. The quantity of matter in the *unit* of mass, determined as now explained, will be equal to 32.1912 times that in the standard pound of platinum; for this would be the quantity to which, if free to move, the standard pound pressure would in one second impart a velocity of one foot per second.

It would seem desirable to have a short and convenient name for this unit of mass. Why can we not call it a *matt?* The *matt* will then be a quantity of matter equal to 32.1912 times that of the standard pound of platinum.

I see no good reason for calling one particular unit of force the *absolute* unit. For if it be said that the pound pressure, even as now defined, would vary with possible variations of gravity at London, we might require it to be determined once for all at a particular time; and its results in maintaining the distortion of a spring, and in producing motion could of course be recorded. Furthermore, the so-called absolute unit has to be determined practically by observations on pendulums, falling bodies, &c.; and any considerable alterations in the gravitation force of the earth would alter the results of these, and undoubtedly also the actual length of the unit of time, on which, together with the arbitrary units of space and mass, the so-called *absolute* unit of force depends.

As to practical convenience there may sometimes be advantage in using this unit of force. If I wished to use it at New Haven, I should need a standard spring balance, graduated by comparison at London with the pressure produced there by the standard pound of platinum; or, if I had here a copy of that standard piece of platinum, I could graduate my own spring balance to give the "absolute" unit, by suspending the standard by the balance, and noting the position of the index. By dividing the distance from the zero point to this position by the value of g found here by experiment, I could get the distance which would corrrespond on the balance to the "absolute" unit of force.

But whichever of the previous modes of defining the *units* of force and mass is taken, these units must be so defined, in any one system, as to be invariable, and we must understand that the unit of force in one system is a certain definite number of times greater or less than in the other, and they may both *measure* the same kind of force, whether it produces visible motion or not. If we are to use the "absolute" unit, ought we not to give it a shorter and better name, and have its ratio to the pound pressure, as previously defined, definitely understood? Would there be any objection to calling this unit a *tend?* This word might be regarded as suggestive of *tendency* or *tension.* If the word were adopted, and a pound pressure were defined as the pressure due to gravity, exerted now at London by the standard pound of mass, experiments prove that the *tend* would be equal to a pound pressure divided by 32.1912; or, one *pound* pressure equals 32.1912 *tends.*

We cannot do better than to quote a few words from the preface to Newton's Principia, to show the object he had in view in formulating the *laws of motion.*

"All the difficulty of philosophy seems to consist in this—from the phenomena of motions to investigate the forces of nature, and then from these forces to demonstrate the other phenomena; and to this end the general propositions in the first and second book are directed. In the third book we give an example of this in the explication of the System of the World; for by propositions mathematically demonstrated in the first book, we there derive from the celestial phenomena the forces of gravity with which bodies tend to the sun and the several planets. Then from these forces, by other propositions which are also mathematical, we deduce the motions of the planets, the comets, the moon and the sea."

It seems to me that writers on mechanics of late years have made difficulty in this subject by taking what ought to be regarded simply as a means of solving a problem, and turning it into a fundamental definition.

What we want is simply a rule by which from given changes of the motions of bodies we may infer the intensities, as measurable in pounds, of the acting forces, or by which if the intensities of acting forces are given in pounds, we may infer the resulting changes of motion. But instead of stating, as Newton's second law, properly understood, does state, that *changes* of momentum, in bodies free to move, are *proportional* to the intensities of acting forces (pressures or tensions), the law is generally turned about nowadays, and given in the following form :

"*The measure of a constant force is the product of the mass moved by the velocity imparted in a unit of time.*"

This would not be so objectionable, except for the fact that it gives the idea that we must measure forces *only* by momentum produced, and thus that there is an essential difference between the nature of forces which produce motion and of those which appear simply as pressure. Thus it comes that there are plenty of writers on mechanics who will gravely state that forces are of two *kinds*, those which produce motion and those which produce pressure; just as if the imparting of motion to a body were done by something whose intensity could not be measured in pounds pressure, and as if it were not an acting pressure which really produces motion*. We are apt not to see that in *measuring* or comparing forces by this rule the comparison has reference solely to the precise attribute, quality or quantity, which we compare when we measure forces by observing the points at which they will hold a spring balance compressed or extended; and that the rule furnishes only *one way* of measuring forces, to be used for estimating pressures or tensions actually operating to cause motion, in cases where it is inconvenient or impossible to measure those pressures or tensions by a spring balance or other dynamometer. But Newton, after giving his eight *definitions*, adds a scholium, near the end of which, in discussing true and apparent motions, the following occurs :

"If two globes, kept at a given distance one from the other by means of a cord that connects them, were revolved about their common center of gravity, we might from the *tension of the cord* discover the endeavor of the globes to recede from the axis of their motion, and from *thence* we might compute the quantity of their circular motions. And then if any equal forces should be impressed at once on the alternate faces of the globes, to augment or diminish their circular motions, from the increase or decrease of the *tension of the cord* we might infer the increment or decrement of their motions." And further on he says, "But if we observed the cord, and found that its tension was that very tension which the motions of the globes required, we might conclude the motion to be in the globes, and the bodies [among which they were apparently moving] to be at rest; and then, lastly, from the translation of the globes among the bodies, we should find the determination of their motions. But how we are to collect the true motions from their causes, effects, and apparent differences; and, *vice versa*, how from the motions, either true or apparent, we may come to the knowledge of their causes and effects, shall be explained more at large in the following tract. For to this end it was that I composed it." Newton immediately goes on from this to state his famous *axioms*, or *laws of motion*. Near the beginning of his *System of the World*, he again shows the object of his *laws* in nearly the same words as before quoted: "And how from the motions given we may infer the forces, or from the forces given we may determine the motions, is

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* Whether all pressure may not be due to innumerable molecular impacts I do not undertake to say. But even if it is, it is something which the engineer has to measure and allow for in pounds, and the word *force* in the rule is simply one name of it.

shown in the two first books of our *Principles of Philosophy.*"

The *law of motion* as stated by Newton, or the rule above given for measuring force, ought to be regarded then merely as a means for solving certain problems, and must not be understood as being a definition of the only proper way to *measure* what we call simply *force.*

To learn conclusively that the word *force*, as used in that rule, means simply pressure or tension, may be done by experiments with Atwood's machine. You have simply to balance two equal masses on the cord, and then place a small additional mass on one of them. So long as you prevent motion of the bodies, the small mass will press on the one beneath it with a pressure equal to its weight, and the motion of the system which will follow on releasing the apparatus, will depend on the pressure, equal to the weight in pounds or ounces exerted by gravity on the small unbalanced mass. You arrange a catch by trial, so that after moving for one second the small extra mass shall be caught away, and then observe how far the balanced bodies move in the next second. You then experiment again by placing the small mass once more upon one of the balanced bodies, and removing from the other one-half as much also, which is transferred to the first, so as to leave unchanged the total mass which is to move, but making the difference between the two sides twice what it was in the first experiment.* The effective pressure of gravity producing motion in the system will now be twice what it was before, and it will be found that this pressure, acting for one second, will give to the moving mass twice the velocity before acquired. And in general, if the total mass remain unchanged the velocity imparted in a second will be found to be proportional to the difference between the two suspended masses, and therefore proportional to the effective pressure exerted in moving the system.

To show that the constant pressures necessary to give in a certain time a stated velocity to different masses must be proportional to those masses needs no experimental proof; for we may readily conceive of equal masses placed side by side and acted on by equal parallel pressures, which will produce in the masses equal velocities; and the sum of the pressures will be proportional to the sum of the masses, whatever be their number or the total mass.

Since then a constant pressure, acting for a unit of time, must be proportional to the velocity imparted by it to a given mass, and also to the mass to which it imparts in a unit of time a given velocity; if we change both the mass to be acted on, and the velocity to be imparted in a given time, the pressure must be *proportional* to the product of the mass and the velocity imparted. By taking suitable *units* of mass, velocity and pressure, the *numerical value* of the pressure in pounds may be rendered the same as that of the product of mass by velocity imparted in the unit of time, and thus this product may be taken, in certain cases, as a convenient way of comparing or *measuring* pressures, as stated in the rule. If a constant pressure acts for a *unit* of time on the *unit* of mass, (*g* pounds of matter) free to move, the velocity imparted will be equal in *numerical value* to the number of pounds in the acting pressure, and is called the *acceleration*, and sometimes the *accelerative effect* of the force.

This last is apt to be a misleading name. For the word *effect* is more frequently used, especially by practical engineers, to apply to the *work* done by a force; they are therefore liable to be confused, and to fail to distinguish clearly between *work* and that which we compare by *acceleration.* Now the *acceleration* of the unit of mass, due to any force, will depend simply on the intensity of this as measurable in units of pressure, whether the body acted on be originally at rest or moving with any velocity; but the *work done* in producing any *acceleration* will depend on the original velocity. If that is zero, it has been shown in the first article of this series that the work will vary as the square of the velocity afterward imparted.

The failure to recognize that in measuring forces by *accelerations* we are concerned with nothing but *intensities*, as comparable, at least allowably, in pounds, leads even highly educated physicists

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* We neglect the mass of the moving wheels of the machine, since their motion will be uniformly accelerated by the same laws as that of the balanced masses; and friction is made so small as to be almost insensible.

into what seems to me needless perplexity. The following paragraph is from a paper by Prof. Henry Morton, Ph. D., in the *Journal of the Franklin Institute* for November, 1868.

"We say and know that the *vis viva*, or work done by a moving body, varies with the square of its velocity, while we know, by our previous reasoning, that the force expended in giving it that velocity only varies with the velocity itself. Thus the force of gravity will give a falling body a double velocity in a double time, during which it must have exerted a double force upon it. Here, then, we have a double force, doing a quadruple work. Is this because by some wonderful and recondite property inherent in velocity the double power has been indued with an again doubled efficiency? Many writers leave us to think so; but we, on the contrary, believe that the work done *only seems to increase* more rapidly than the power implied in the increased velocity, by reason of a *loss of efficiency* in the resistances, in the overcoming of which the "work" consists, and in fact, that work in this sense, is no true measure of force."

By the phrase "force expended" in the first sentence, the writer could not have meant "work done," and yet he must have meant to speak of a *quantity* of some sort. What the unit of that quantity is, is not quite clear. If he had said, "We know that the vis viva, or work which will be done by a moving body, in coming to rest, varies with the square of its initial velocity, and we also know that if two constant pressures or tensions, acting for the same time, give different velocities to equal masses, the pressures are proportional to the velocities," he would have said what can be proved by experiment; and the statements would not conflict at all with each other, or with the fact that in a *double time* a *constant* pressure, like that of gravity, acting on a body free to move from rest, will perform a *quadruple* work; though certainly not by reason of "exerting a double force," but a constant one. For gravity is of such a nature that it exerts a constant* force (pressure or tension) on a body, whether that is moving or not, and it is able to perform a quadruple work on a falling body in a double time, simply because it exerts this pressure on the falling body through four times the distance in two seconds that it does in one. To be sure, if gravity acted for one second, and then ceased, a falling body would move thirty-two feet in the next second, and the actual pressure in this second is not responsible for this thirty-two feet motion; but *because* of this motion, the constant pressure can make its proper addition to the velocity only by being exerted through this thirty-two feet, besides the sixteen feet which it alone in this second would move the body from rest. Thus the total distance moved by a constant pressure in imparting a double velocity must be quadruple, and the work done in imparting velocity not only *seems* to increase as the square of the velocity, but actually does so increase.

Prof. Morton claims that the *effect* of a *force* must be proportional to the *time* of its action. This would be true of the *effect* of an engine, for example, working at a *constant rate;* but why should it be true of a force like gravity which acts with an intensity of the same number of pounds at all known velocities? Suppose a heavy body to be raised against the action of gravity, by some other force, at a *uniform* velocity. Then suppose another body of the same weight to be raised at a velocity, also uniform, but ten times as great as the velocity of the first. This body will be raised ten times as high in a given time as the first. Would the real *work*, or *effect*, performed against gravity be no more in this case than in the first? If not, it would be a good thing for the engineer to know; for by gearing up his hoisting machinery to the highest possible velocity he could produce the most wonderful results with the least possible force.

There is, indeed, one sort of effect, or *result*, of the action of a constant force, which is proportional to time, viz: the change of *velocity* of a moving body, when acted on by the force. That is to say, if a force of constant intensity act so as to impart motion to a body, the *change* of velocity of the body will be the same in each unit of time, no matter what the velocity actually becomes; so that the total change of velocity produced by a constant force in any number

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* Neglecting the slight change due to difference of distance from a center.

of seconds will be proportional to that number of seconds. But simple *change of velocity* is not what is usually understood by the *effect* of a force. Writers have generally agreed to measure the *effect* of a force by *work*, or the product of the *force* and the *distance* through which it is exerted, whether the work is done in imparting velocity to bodies or in overcoming any sort of resistance. It seems to me that we need in mechanics a name which shall apply to *the exertions of force during time*, whether this produces apparent motion of masses or not, just as much as we need the name *work* to denote the exertion of force through distance, independent of velocity. The word *toil* appears to me well adapted to supply the need, as I am not aware that it has hitherto been used in any technical sense in mechanics. I would suggest for the consideration of writers on this subject, that we take the word *toil*, and define it for mechanical purposes, as *the exertion of force* (*pressure or tension*) *during time.* The exertion of a pound pressure for one second, whether producing changes of velocity, or overcoming any resistances, or simply maintaining the distortion of elastic bodies, like springs, etc., might be taken as the unit of *toil*, and called a *second-pound.* If any one preferred to use the *tend* as the unit of force, the unit of toil could be made the *second-tend.*

In another number I will return to a consideration of the use of this word *toil* with the above meaning. At present, as illustrative of the conflicting views and statements to which ill-defined or ill-used words may lead, let us compare an explanation of Prof. Morton's with one from Cooke's *Chemical Physics.* Prof. Morton, *loc. cit.* p. 347, says: "If then, a moving body with a certain velocity, overcomes a certain number of these resistances, or, for example, penetrates a medium to a certain depth, before its motion is arrested, it has overcome so many resistances, each acting for such a length of time. If now the same body with a *double* velocity, meets the same medium, it will penetrate each resisting element in *half the time*, and so receive from it but *half the resistance* it experienced before."

Cooke's *Chemical Physics*, p. 52, says concerning the resistance met by a railroad train: "With a double velocity the moving train passes over double the space each second, and therefore encounters twice as many points of resistance. Moreover, it strikes each of these points with *double* the velocity, and hence meets at each point *twice the resistance.* It therefore meets, during a second, twice as many points of resistance, and suffers *at each point* twice as much resistance. The resistance during a second is thus four times as great as before and must require four times as much force [whatever that means] to overcome it."

The two views quoted cannot both be right, and it seems to me that they are both wrong, and the truth between them. The mere friction of a rail-road train, not including the resistance of the air, would, within some limits of speed, be almost invariable in intensity. That is, if a train were drawn *at a constant rate*, by a weight hanging over a pulley, then if an outside power gave a higher velocity to the train, and afterwards ceased to act, the same hanging weight as before would keep the train at the new velocity, so far as mere friction is concerned.[*] Hence the work done in overcoming friction in a given time would not vary as the square of the velocity, but simply as the velocity; the number of pounds in the resistance being *the same* as at the former speed, instead of either *one half* or *double.* The resistance of the air is another matter, and would have to be considered by itself; but at low velocities it forms a very small part of the total resistance, and I believe that no practical engineer would say that in running a train for a day on a level road at the constant rate of six miles per hour he would burn four times as much coal as he would in running for the same time at three miles per hour. In *starting the train* and *getting up speed* the work due to inertia varies as the square of the velocity produced, but this is entirely aside from friction and the resistance of the air. And if doubling the initial velocity of a moving body causes it to

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[*] This is a result reached by the experiments of Morin and Coulomb; and the valuable experiments of Prof. A. S. Kimball, described in the *American Journal of Science* for May, 1877, show that it would be sensibly true within certain limits, though at high velocities Kimball's experiments, as well as the previous ones of Bochet, show that the coefficient of friction would *diminish*, instead of increasing.

penetrate a certain medium to a quadruple depth, it is proof enough that the average intensity of the resistance at the various points of the medium is unchanged.

We have explained how to show by experiment with Atwood's machine, that the word *pressure* or *tension*, as denoting a quantity measurable in pounds, is perfectly suited to take the place of the word *force* in the ordinary rule for the measurement of *force*, and hence that *force* as used in that rule means *pressure* or *tension;* and since the word cannot be supposed to have several meanings in a single rule, we exclude all meanings conflicting with this. The only objection that could be made to the demonstration from experiment would be to inquire, how do we know that gravity exerts the same *pressure* or pull on a body in motion as on the same at rest? Indeed, Prof. Morton, *loc. cit.* would explain the fact that a double initial velocity causes a body to rise against gravity to a quadruple height, by saying that the body, "traversing each distance in half the *time*, gravity would exert but *half* its former *effect* within the same space." But aside from the alteration in the pressure, due to change of distance from the earth's center, and which is so slight that for almost every purpose it may be wholly disregarded, we have no reason to suppose that any possible velocity would alter the intensity of the pressure or pull exerted on a body by gravity. For we find by experiments with Atwood's machine, that the velocity of a falling body *increases* as much in the third or fourth second, as in the first; which shows that the intensity of the pressure does not diminish when the actual velocity has become greater.

If any one should suppose that the action of gravity in the experiments described is peculiar, in that the small mass used to produce motion forms practically an essential part of the mass moved, and thus that there is any uncertainty as to the action of pressure on the whole, we can devise experiments in which a constant pressure or tension shall be applied to a mass through a coiled spring which shall indicate directly the amount of pressure in pounds or ounces, which is active in imparting velocity.

Suppose first a certain mass of matter to be moving without friction on a horizontal plane, drawn by the action of gravity on another mass suspended by a cord passing over a frictionless pulley and attached to the first. Let us suppose this cord to be perfectly flexible and inextensible, and without weight, (if that were conceivable), so that the two masses shall move with identical but increasing velocity. What would be the tension on the cord? If the velocity of the masses is uniformly accelerated, our theory requires that the effective pressures by which they are urged shall be proportional to the masses. Let the mass of the one moving horizontally be M, and that of the one moving vertically be m, the weight of the latter in pounds be w, and the tension on the cord in pounds be t; the effective pressure with which the mass m will be urged, will be the difference between its weight and the tension on the cord, viz: $w-t$, and the tension on the mass M will be t; so that we shall have the proportion

$$t : w-t = M : m,$$

whence we may find

$$t=\frac{Mw}{M+m}. \qquad (4)$$

In our hypothetical case, this formula would enable us to calculate the tension of the cord connecting the two masses. In nature there is no inextensible cord; and the question occurred to me, what would be the result if the mass M were resting on a frictionless plane, held in place by the hand or otherwise, and were solicited horizontally by the action of gravity on the mass m, suspended over a pulley by a cord attached to the mass M by a coiled spring. It is evident that, so long as motion of the mass M is prevented, the tension on the cord would be equal to w. If M be suddenly released, it will then be drawn at the first instant by the tension w, which is evidently greater than $\frac{Mw}{M+m}$. To begin with, therefore, the mass M will follow m with a greater velocity than it ought to have if the two masses are to move with equal velocities. And similarly m will be more retarded if its motion be resisted by the tension w, than it would be by $\frac{Mw}{M+m}$. Hence the two masses will not immediately move with equal velocities, but in moving will be drawn nearer together. But, as the distance between them becomes smaller, the spring will yield so as to exert a diminished tension on the masses, and this tension may finally become reduced to $\frac{Mw}{M+m}$. Will it then remain at this value? The formula for the inextensible cord makes the tension for given values of M and m invariably this, no matter what actual velocity is obtained. But while the tension on the supposed spring has been reducing itself to this value, the mass M has had more than its proper share of work done on it, and must be moving with considerably greater velocity than the mass m; hence it will continue to approach this, and so will still further reduce the tension on the spring.

This approach of the bodies and reduction of tension will go on until the now greater proportional effective action of gravity on the mass m shall have raised its velocity to equality with that of M. But at this point, the spring will be exerting less than the normal tension due to a common velocity, hence the mass M will now immediately begin to lag behind, relatively to m, and thus the spring will be again elongated. If it were perfectly elastic it would be stretched to the original tension given by the weight of the mass m when at rest, and would then begin another similar oscillation, and so keep on oscillating, as long as the mass M continued to be drawn by the action of gravity on the mass m. The extent of each oscillation must evidently be such as to give a *mean* tension on the spring equal to $\frac{Mw}{M+m}$.

It is difficult to find a plane sufficiently free from friction to test this result, but a similar series of oscillations ought to occur if a mass were suspended by a spring from one of the balanced masses of an Atwood machine, and then these set free to move. In the absence of Prof. Loomis, Prof. Wright kindly placed the fine Atwood machine of Yale College at my disposal, and in the first rough experiments which I made with it, to verify this conclusion, there was no difficulty in detecting the oscillations. I had a coiled spring made, of about 130 turns of fine brass wire, making the length of the spring, when hanging by its own weight, about seven inches; and when supporting a weight of 31.1 grms. its length was about thirty inches. The weight of the spring was 7.43 grms. I attached one end of a silk thread to the top of the spring, and allowed the thread to hang down through the interior of the spring. To the bottom of the thread I fastened a small paper scale of equal parts, so that it would hang by the side of the 31.1 grm. weight. On attaching the spring supporting this weight, to one of the balanced masses of the machine, and releasing the apparatus, this paper scale would fall with a velocity equal to that of the balanced masses, and the oscillations of the 31.1 grm. weight along the scale were easily though not very accurately observed. With a balanced mass equal in weight to 602 grms. the oscillation of the end of the spring was at least $2\frac{3}{8}$ inches. I give no numerical comparison of this result with the formula, as the spring in this experiment formed a part of the unbalanced weight, and the mode of observation was not close. But with a lighter propelling weight and shorter spring, so as to get quicker oscillations, there was no difficulty in observing four or five oscillations before the fall had to be stopped.

I afterwards altered my arrangements, by running a stiff wire down through the center of the spring, attaching the spring at top to this wire, and this in turn to one of the balanced masses, and using a piece of brass tubing for the propelling weight. This hung at the bottom of the spring, and the stiff wire passed through it without touching it. On the wire below the position of the weight when at rest, I arranged a new and finer scale of equal parts. To determine with considerable accuracy the amount of oscillation, I attached a fine silk thread to the bottom of the tubular weight, and allowed it to pass down the face of the paper scale, being lightly held against it by a turn or two of another silk thread around the scale at its top and bottom. A small knot in the vertical thread served to mark its position. This position being noted by drawing the thread just straight when the weight was hanging at rest, on releasing the apparatus the first oscillation would draw the thread up along the scale, and leave it at its highest position. To reduce friction here as much as possible, when making the observations, I first found by trial approximately the value of the first oscillation, and then by raising the tubular weight by hand, drew the thread nearly to the right height, so that afterwards when raised by the oscillation of the apparatus the work should be almost inappreciable.

I arranged the masses on the machine so as to have all my apparatus except the tubular weight form a part of the balanced mass, the value of this being 330 grms., neglecting the silk suspension cord and the wheels of the machine. The tubular piece (with fine wire hooks for attachment), which produced the tension, weighed 17.63 grms. My friend, Mr. H. A. Hazen, kindly assisted me in releasing one of the masses suddenly from a brass plate below.

After one or two trials to get the approximate oscillation, for the purpose explained, I made five observations of the first oscillation, with the following results:

$1\frac{4}{32}$ inches.
$1\frac{5}{32}$ "
$1\frac{4}{32}$ "
$1\frac{6}{32}$ "
$1\frac{4}{32}$ "

The mean of these five trials, gives for the extent of the first oscillation, 1.144 inches.

Before proceeding to inquire whether one-half of this oscillation of the spring corresponds to a reduction of the tension to the value $\frac{Mw}{M+m}$, it is necessary to take into account the effect of the inertia of the machine, which is very considerable. If we balance two known masses on the cord, and then add a small piece to one of them, the velocity of the fall will be retarded by the inertia of the machine, as well as by that of the balanced masses. But, the velocity of the wheels of the machine being uniformly accelerated, if we could deprive the machine of inertia, and leave friction and resistance of air out of account, there would be a certain particular mass of matter, which, added to the balanced mass, would be just equivalent in resistance to that actually opposed by the inertia of the machine.

Hence we may find the value of this equivalent mass as follows: since in any locality masses are proportional to their weights, let X be the weight of the balanced load on the machine, *including* this unknown equivalent, and suppose the machine to have no inertia. Let w be an extra weight applied to one of the masses to cause motion, d the distance through which it causes the mass to move in a second, and s the distance a body falls freely in the first second. From well known principles we shall have the proportion

$$d : s = w : X + w,$$

whence

$$X = \frac{w(s-d)}{d} \qquad (5)$$

from which, by observing d and w, we may calculate X; and knowing how much the separate load on the machine is, we find the equivalent to the machine by difference. This difference ought, so far as simple inertia is concerned, to be the same for all variations of loads and moving weights; but, on account of friction and atmospheric resistance, it is better to determine it by using about the same values as in the particular experiment with which comparison is to be made. By doing so we very nearly eliminate the effect of these other resistances.

With so rapid a fall as that given by the masses of my experiments, it was difficult to determine the precise distance fallen in a second, (we could not be sure in a single trial to get the value within an inch or so), but Mr. Hazen's long practice in observing time by the ticks of an astronomical clock enabled us, by estimating fractions of a second, to get results not far out of the way. We removed the coiled spring, and took first a balanced load of 330 grms., and a propelling weight of 18 grms. I manipulated the machine and allowed twenty falls of the weights through distances varying from five to twenty inches. I numbered each experiment, and noted the distance, while Mr. Hazen, not knowing this latter, sat with his back to me and estimated by sound the time of each fall, to the tenth of a second. On comparing our lists, and calculating from each observation the distance fallen in one second, the mean of the twenty observations gave 8.1 inches as the value of d. Reducing this to feet, taking $s=16.08$, and $w=18$, the above formula gives for the value of X, 410.81 grms.; from which, by subtracting the load of 330 grms., we find the resistance of the machine to be equivalent to that of a mass equal to 80.81 grms. added to the balanced masses.

We are now in condition to calculate from our formula (4), for the mean tension of the coiled spring, what value we ought to expect for t in the experiments described. And since we may substitute weights for masses in that formula, we write it, $t=\frac{Ww}{W+w}$, in which W must be the balanced load on the machine, plus the equivalent of the inertia of that. We have then, $W=330+80.81=410.81$, $w=17.63$, and by substitution we find

$$t=16.905.$$

I found by trial that when my spring was stretched by a weight of sixteen or eighteen grms., an additional weight of five grms. produced an elongation of 3.75 inches; hence one inch motion of the end of the spring represents a change of tension equal to 1.333 grms. But in the experiments with the spring above described, the extent of the oscillation was found to be 1.144 inches. The tension at the middle of the oscillation, that is at the point of mean tension during the fall, would therefore be found by subtracting $\frac{1}{2}\times 1.144\times 1.333$ grms. from the weight of the tubular mass, which was 17.63. Making this subtraction we find the value of t, as given by observation of the oscillations, equal to 16.868 grms., as against 16.905 found by calculation from the weights used and the estimated inertia of the machine. The difference between these results is 0.037 grms., which though a far greater error than would be allowed in work for any important purpose, is perhaps as small an error (less than five per cent. of the loss of tension) as could be expected without taking pains to construct more perfect apparatus for making the observations. The loss of tension on the spring at the mean point of oscillation is, by the above observations,

$$17.63-16.868=0.762 \text{ grms.}$$

I made a second set of experiments having the same tubular piece for the propelling weight, but reducing the balanced load to 130 grms. The oscillations noted were

$$\left.\begin{array}{ll} 1\frac{15}{16} & \text{inches} \\ 2\frac{1}{16} & \text{"} \\ 2 & \text{"} \end{array}\right\} \text{mean}=2 \text{ inches,}$$

showing a reduction of tension at the middle of the oscillation equal to 1.333 grms. A special set of sixteen observations for determining the resistance of the machine and air at the new speed gave it equivalent to 93.2 grms., unless I were to reject four rather doubtful observations; which would have made it 83.66, a value nearer that found for the other speed. If I assume $W=130+93.2$, and $w=17.63$ the formula $t=\frac{Ww}{W+w}$ will give $t=16.339$, corresponding to a loss of tension equal to $17.63-16.339=1.291$ grms., against that of 1.333 grms. indicated by the oscillations; the error being 0.042 grms., or a little over three per cent. If I were to take the other equivalent of the inertia, making $W=130+83.66$, the value of t by the formula would be 16.286, and the corresponding loss of tension therefore 1.344; the difference between this and the result of the oscillations being only 0.011 grms., or less than one per cent.

There would seem to be no reason for expecting the tension on the spring to ever return in the oscillation, so as to exceed that at the beginning, when the masses are at rest. Yet I thought it worth while to try once or twice to see if I could discover such an excess, but was unable to do so. If then, we can find as above, by calculation or otherwise, what the mean tension on the spring during the fall should be, and can begin the experiment by supporting the propelling weight in some way, so as to let it start the fall with only this mean tension on the spring, there ought to be no oscillation either way from this mean. It ought then to go on, exerting this tension on the balanced masses with invariable intensity, no matter what velocity should be obtained, (disregarding friction, atmospheric resistance, and the slight increase of gravity on approaching the earth's center). During my first experiments with the 31.1 grm. propelling weight, which was found to give an oscillation of as much as $2\frac{3}{8}$ inches, I attached a silk thread to this weight, and by catching it on a screw, turned it so as to draw the weight up $1\frac{1}{8}$ inches from its hanging position. I also arranged another silk thread to prevent the motion of the balanced masses until desired. These threads crossed each other, and could be cut at the same moment by a pair of scissors. On my cutting the threads, Prof. Wright watched the falling weight, and stated that if there was any oscillation it was less than $\frac{1}{8}$ inch. The experiment was repeated with similar result.

The experiments herein described tend to show, what, of course, theory first indicates, that a falling body which is made to exert a pull or pressure on other bodies so arranged that they offer no resistance except that due to their inertia, cannot exert a force on them equal to its weight; the reason being that a part of the real pressure of gravity on the body used as a propellor is resisted by reaction of this body against acceleration. But as the experiments tend to confirm the formula, $t = \frac{Mw}{M+m}$, they also indicate that, for any given values of M and m, the masses might acquire any conceivable velocity, without the slightest diminution of the mean tension exerted between them; since the formula is independent of the velocity.

We are then, I think, justified in expecting that with sufficient care we could, by means somewhat similar to those described, apply to a body free to move, a force which should be shown by a spring balance to be constantly a fixed number of pounds or ounces, and which should give the body a velocity varying directly as the time, and should make the total space traversed proportional to the square of the velocity acquired; thus leaving no room to doubt that the *work* performed on the body had not only apparently but actually varied as the square of the velocity imparted, and thus that the real *work* which the body ought to be able to do, in virtue of its velocity, should be proportional to the square of this velocity.

JOINTS IN WOODWORK.

By Mr. HENRY ADAMS, A.I.C.E.

From "The Builder."

This subject is seldom treated in a sufficiently systematic manner in the text-books used by the junior members of the profession, especially as among engineers iron is looked upon as the staple material of construction, the properties and economical uses of which receive in consequence an almost exclusive share of their attention. Although wood is used much less than formerly in permanent structures, it is still of sufficient importance to warrant a careful study of its nature, properties, and uses.

All trees are divided by botanists into three classes,—*exogens*, or outward-growers; *endogens*, or inward-growers; and *acrogens*, or summit-growers, according to the relative position in which the new material for increasing the substance of the tree is added, viz., whether toward the outside, the inside, or the top. Typical trees of each class would be the oak, the palm, and the tree-fern. We have to deal with the exogenous class only, as that furnishes the timber in general use for construction, the term "timber" including all varieties of wood which, when felled and seasoned, are suitable for building purposes.

If the stem of an exogenous tree be cut across, it will be found to exhibit a number of nearly concentric rings, more or less distinct, and, in certain cases, radial lines intersecting them. These rings represent the annual growth of the tree, which takes place just under the bark. Each ring consists of bundles of woody fibre, or vascular tissue, in the form of long, tapering tubes, interlaced and breaking joint with each other, having a small portion of cellular tissue, at intervals. Towards the outer edge of each ring the woody fibre is harder, more compact, and of a darker color than the remaining portion. The radial lines consist of thin hard vertical plates formed entirely of cellular tissue, known to botanists as "medullary rays" and to carpenters as "silver grain." As the tree advances in age the rings and rays become more irregular, the growth being more vigorous on the sunny side, causing distortion. It must be borne in mind that the strength of wood "along the grain" depends on the tenacity of the walls of the fibres and cells; while the strength "across the grain" depends on the adhesion of the sides of the tubes and cells to each other.

Tredgold proposed a classification of timber according to its mechanical structure. This, as modified by Professor Rankine, is given in the following table:

Class I. Pine-wood (coniferous trees): Pine, fir, larch, cowrie, (New Zealand pine), yew, cedar, &c.

Class II. Leaf-wood (non-coniferous trees): Division I., with distinct large medullary rays; Sub-division I., annual rings distinct: Oak; Sub-division II., annual rings indistinct: Beech, alder, plane, sycamore, &c.

Division II., no distinct large medullary rays: Sub-division I., annual rings distinct: chestnut, ash, elm, &c. Sub-division II., annual rings indistinct: mahogany, teak, walnut, box, &c.

Having glanced at the microscopical structure of the wood, we shall be in a position to understand the process of seasoning and the shrinking incidental to that operation. While wood is in a growing state there is a constant passage of sap or nutritive fluid, which keeps the whole of the interior of the tree moist and the fibres distended; but more especially towards the outside. When the tree is cut down and exposed to the air the moisture gradually evaporates, causing the fibres to shrink according to certain laws; this is the natural process of seasoning. There are various methods of seasoning timber artificially; in each case the object in view is to expedite the process of evaporation. The shrinkage in length is very slight, and need not therefore be considered, but the shrinkage transversely is so great that it is necessary to look closely into the nature of it, as the question of jointing is affected considerably thereby. The behavior of timber in shrinking was demonstrated by Dr. Anderson in one of the Cantor lectures at the Society of Arts, of which the author of this paper has availed himself to some extent.

As the moisture evaporates the bundles of woody fibres shrink and draw closer together, but this contraction cannot take place radially without crushing or tearing the hard plates forming the medullary rays, which are unaffected in size by the seasoning. These plates are generally sufficiently strong to resist the crushing action, and the contraction is therefore compelled to take place in the opposite direction, *i.e.*, circumferentially, the strain finding relief by splitting the timber in radial lines, allowing the medullary rays in each partially severed portion to approach each other in the same direction as the ribs of a lady's fan when closing. The illustration of a closing fan affords the best example of the principle of shrinking during seasoning, every portion of the wood practically retaining its original distance from the centre. If the tree were sawn down the middle, the cut surfaces, although flat at first, would in time become rounded, the outer portion shrinking more than that nearer the heart, on account of the greater mass of woody fibre it contains and the larger amount of moisture. If cut into quarters each portion would present a similar result.

If we assume the tree to be cut into planks, then, after allowing due time for seasoning, it will be found that the planks have altered their shape. Taking the center plank first, it will be observed that the thickness at the middle remains unaltered, at the edge it is reduced, and both sides are rounded, while the width remains unaltered. The planks on each side of this are rounded on the heart side, hollow on the other, retain their middle thickness, but are reduced in proportion to their distance from the center of the tree; or, in other words, the more nearly the annual rings are parallel to the sides of the planks the greater will be the reduction in width. These remarks apply more especially to oak, beech, and the stronger home firs. In the softer woods the medullary rays are more yielding, and this slightly modifies the result, but the same principles must be borne in mind if we wish to avoid the evils of shrinking which may occur from negligence in this respect. The peculiar direction which "shakes," or natural fractures, sometimes take is due to the unequal adhesion of the woody fibres, the weakest part yielding first. In a "cup-shake," which is the separation of a portion of two annual rings, the medullary rays are deficient in cohesion. So far we have considered the shrinking only as regards the cross section of various pieces. Turning now to the effect produced when we look at the timber in the other direction : If we take a piece of timber with the end cut off square, as this shrinks the end still remains square, the width alone being affected. If, however, the end be bevelled we shall find that in shrinking it assumes a more acute angle, and this should be remembered in framing roofs, arranging the joists for struts, &c., especially by the carpenters who have to do the actual work of fitting the parts.

We may now leave the question of shrinkage and proceed to a consideration of the more immediate object of the paper. In the following table an attempt has been made to classify timber under the different terms by which it is known, according to its size and other accidental characteristics. This is only a rough approximation, as no definite rule can be laid down, but it may be of some assistance to those who have occasionally to deal with workmen using the terms.

CLASSIFICATTION OF TIMBER ACCORDING TO SIZE
(*approximate.*)

Balk	12	by	12	to	18	by	18
Whole timber	9	“	9	“	15	“	15
Half timber	9	“	4½	“	18	“	9
Scantling	6	“	4	“	12	“	12
Quartering	2	“	2	“	6	“	6
Planks	11	to	18	by	3	to	6
Deals	9	by	2	to	4½		
Battens	4¼	to	7	by	¾	“	3
Strips and laths	2	“	4½	“	½	“	1½

Pieces larger than planks are generally called timber, but when sawn all round are called scantling, and when sawn to equal diminsions each way are called die-square. The dimensions (width and thickness) of parts in a framing are sometimes called the scantlings of the pieces. The term "deal" is also used to distinguish wood in the state ready for the joiner from "timber," which is wood prepared for the use of the carpenter. A "log" or "stick" is a rough whole timber unsawn.

The use of wood may be discussed under the two heads of carpentry and joinery. The former consists principally of the use of large timbers, either rough, adzed, or sawn; and the latter of smaller pieces, always sawn, and with the exposed surfaces planed. The carpenters' work is chiefly out-door; it embraces such objects as building timber bridges and gantries, framing roofs and floors, constructing centreing, and other heavy or rough work. The joiners' work is mostly indoors; it includes laying flooring, making and fixing doors, window sashes, frames, linings, partitions, and internal fittings generally. In all cases the proper connection of the parts is an essential element, and in designing or executing joints and fastenings in woodwork, the following principles laid down by Professor Rankine should be adhered to, viz.:

1st. To cut the joints and arrange the fastenings so as to weaken the pieces of timber that they connect as little as possible.

2nd. To place each abutting surface in a joint as nearly as possible perpendicular to the pressure which it has to transmit.

3rd. To proportion the area of each surface to the pressure which it has to bear, so that the timber may be safe against injury under the heaviest loads which occurs in practice, and to form and fit every pair of such surfaces accurately in order to distribute the stress uniformly.

4th. To proportion the fastenings so that they may be of equal strength with the pieces which they connect.

5th. To place the fastenings in each piece of timber so that there may be sufficient resistance to the giving way of the joint by the fastenings shearing or crushing their way through the timber.

To these may be added a sixth principle not less important than the foregoing, viz., to select the simplest forms of joints, and to obtain the smallest possible number of abutments. The reason for this is that the more complicated the joint, or the greater the number of bearing surfaces, the less probability there will be of getting a sound and cheaply-made connection. To insure a fair and equal bearing in a joint which is not quite true, it is usual after the pieces are put together to run a saw-cut between each bearing surface or abutment; the kerf or width of cut being equal in each case the bearing is then rendered true; this is often done, for instance, with the shoulders of a tenon or the butting ends of a scarf, when careless workmanship has rendered it necessary. When the visible junction of two pieces is required to be as close as possible, and no great strain has to be met at the joint, it is usual to slightly undercut the parts and give clearance on the inside. In pattern-making the fillets which are placed at the internal angle of two meeting surfaces are made obtuse-angled on the back, in order that when bradded into place the sharp edges may lie close. The prints used by pattern-makers for indicating the position of round-cored holes are also undercut by being turned slightly hollow on the bottom. This principle is adopted in nearly all cases where a close joint is a desideratum. Clearance must also be left in joints of framing when a settlement is likely to take place, in order that after the settlement the abutting surfaces may take a fair bearing to resist the strain.

The various strains that can come upon any member of a structure are: *Tension*, stretching or pulling; *compression*, crushing or pushing; *transverse strain*, cross strain or bending; *torsion*, twisting or wrenching; and *shearing*, cutting; but in woodwork, when the latter force acts along the grain, it is generally called *detrusion*, the term shearing being limited to the action across the grain. The first three varieties are the strains which usually come upon ties, struts, and beams respectively. The transverse strain, it must be observed, is resolvable into tension and compression, the former occurring on the convex side of a loaded beam, and the latter on the concave side, the two being separated by the neutral axis or line of no strain. The shearing strain occurs principally in beams, and is greatest at the point of support, the tendency being to cut the timber through at right angles to the grain; but in nearly all cases if the timber is strong enough to resist the transverse strain, it is amply strong for any possible shearing strain which can occur.

Keys and other fastenings are especially subject to shearing strain, and it will be shown in that portion of our subject that there are certain precautions to be adopted to obtain the best results.

The following tables will serve as an introduction to the remaining portion of the paper:

CLASSIFICATION OF JOINTS IN CARPENTRY.

Joints for lengthening ties, struts, and beams:—Lapping, fishing, scarfing, tabling, building up.

Bearing-joints for beams:—Halving, notching, cogging, dovetailing, tusk-tenoning, housing, chase-mortising.

Joints for posts and beams:—Tenon, joggle, bridle, housing.

Joints for struts with ties and posts:—Oblique tenon, bridle, toe-joint.

Miscellaneous:—Butting, mitreing, rebating.

CLASSIFICATION OF FASTENINGS IN CARPENTRY.

Wedges; keys.

Pins:—Wood pins, nails, spikes, trenails, screws, bolts.

Straps; sockets.

And for joinery must be added glue.

We will consider these joints in the order given above. One of the first requirements in the use of timber for engineering purposes is the connection of two or more beams to obtain a greater length. One method of doing this is by lapping, the two pieces being held together by straps, and prevented from sliding by the insertion of keys. A similar joint may be made by using through-bolts instead of straps, and wrought-iron plates instead of oak keys. This makes a neater joint than the former, but they are both unsightly, and whenever adopted the beams should be arranged in three or five pieces in order that the supports at each end may be level, and the beams horizontal. This joint is more suitable for a cross strain than for tension or compression. The common form of fished beam is adapted for compression; if required to resist tensile strain keys should be inserted in the top and bottom joints between the bolts.

Tabling consists in bedding portions of one beam into the other longitudinally. Occasionally the fishing pieces are tabled at the ends into the beams to resist the tendency to slip under strains, but this office is better performed by keys, and in practice tabling is not much used. The distinction between fished beams and scarfed beams is that in the former the original length is not reduced, the pieces being butted against each other, while in the latter the beams themselves are cut in a special manner, and lapped partly over each other; in both cases additional pieces of wood or iron are attached to strengthen the joint. A form of scarf adapted to short posts has the scarf cut square and parallel to the sides, so that the full sectional area is utilized for resisting the compressive strain. When the post is longer, and liable to a bending strain, the scarf should be inclined, to allow of greater thickness being retained at the shoulder of each piece, the shoulder being kept square. In this joint a considerable strain may be thrown on the bolts from the sliding tendency of the scarf, if the shoulders should happen to be badly fitted, as any slipping would virtually increase the thickness of the timber where the bolts pass through. The width of each shoulder should be not less than one-fourth the total thickness. Joints in posts are mostly required when it is desired to lengthen piles already driven, to support a super-structure in the manner of columns. Another form of scarf for a post put together without bolts has the parts tabled and tongued, and held together by wedges. This is not a satisfactory joint, and is, moreover, expensive because of its requiring extra care in fitting, but it may be a suitable joint in some special cases in which all the sides are required to be flush. In the common form of scarf, in a tie-beam, the ends of the scarf are bird-mouthed, and the joint is tightened up by wedges driven from opposite sides. It is further secured by the wrought-iron plates on the top and bottom, which are attached to the timber by bolts and nuts. In all these joints the friction between the surfaces due to the bolts being tightly secured up plays an important part in the strength of the joint, and as all timber is liable to shrink, it is necessary to examine the bolts occasionally, and to keep them well tightened up. Sometimes the scarf is made vertically instead of horizontally, and when this is done a slight modification is made in the position of the projecting tongue. The only other scarfs to which attention need be called are those in which the compression side is made with a square abutment. These are very strong forms, and at the same time easily made. Many other forms have been designed, and old books on carpentry teem with scarfs of every conceivable pattern, but in this as in many other cases the simplest thing is the best, as the whole value depends upon the accuracy of the workmanship, and this is rendered excessively difficult with a multiplicity of parts or abutments.

In building up beams to obtain increased strength, the most usual method is to lay two together sideways for short spans, as in the lintels over doors and windows, or to cut one down the middle and reverse the halves, inserting a wrought-iron plate between, as in the flitch-girders. The reversal of the halves gives no additional strength, as many workmen suppose, but it enables one to see if the timber is sound throughout at the heart, and it also allows the pieces to season better. A beam uncut may be partially decayed in the center, and hence the advantage of cutting and reversing, even if no flitch-plate is to be inserted, defective pieces being then discarded. When very long and strong beams are required, a simple method is to bolt several together, so as to break joint with each other, taking care that on the tension side the middle of one piece comes in the center of the span with the two nearest joints equidistant. It is not necessary in a built beam to carry the full depth as far as the support. The strain is, of course, greatest in the centre; and, provided there is sufficient depth given at that point, the beam may be reduced towards the ends, allowance being made for the loss of strength at the joints on the tension side. A single piece of timber secured to the underside of a beam at the centre is a simple and effective mode of increasing its strength. The straps are bedded into the sides of the beams: they thus form keys to prevent the pieces from slipping on each other. This weakens the timber much less than cutting out of the top or bottom, as the strength of a beam varies only in direct proportion to the breadth, but as the square of the depth. The addition of a second piece of timber in the middle is a method frequently adopted for strengthening shear-legs and derrick-poles temporarily for lifting heavy weights.

We now come to the consideration of bearing-joints for beams, the term "beam" being taken to include all pieces which carry or receive a load across the grain. The simplest of these is the halving-joint, where two pieces of cross bracing are halved together, or where the ends of two wall-plates meet each other. When the joint occurs in the length of a beam, it is generally called a scarf. In each of these examples, half the thickness of each piece is cut away so as to make the joint flush top and bottom. Sometimes the outer end of the upper piece is made thicker, forming a bevelled joint, and acting as a dovetail when loaded on top. When one beam crosses another at right angles, and is cut on the lower side to fit upon it, the joint is known as single notching; when both are cut, it is known as double notching. These forms occur in the bridging and ceiling joists of double and double-framed flooring. When a cog or solid projecting portion is left in the lower piece at the middle of the joint, it is known as cogging, cocking, or caulking. Dovetailing is not much used in carpentry or house joinery, owing to the shrinkage of the wood loosening the joint. A wedge is sometimes inserted on the straight side to enable the joint to be tightened up, as the wood shrinks. Tredgold proposed the form known as the "Tredgold notch," but this is never seen in practice. Tusk-tenoning is the method adopted for obtaining a bearing for one beam meeting another at right angles at the same level. This occurs round fire-places, hoistways, and other openings through floors. The advantage of this form is that a good bearing is obtained without weakening the beam to any very great extent, as the principal portion of the material removed is taken from the neutral axis, leaving the remainder disposed somewhat after the form of a flanged girder. When a cross piece of timber has to be framed in between two beams already fixed, a tenon or chase-mortise is one of the methods adopted. If the space is very confined, the same kind of mortise is made in both beams, but in opposite directions. The cross piece is then held obliquely, and slid into place. Occasionally it is necessary to make the chase-mortise vertical, but this is not to be recommended, as the beam is more weakened by so doing. In some cases a square fillet is nailed on to take the weight of the joists, without cutting into the beam. While speaking of floors, the process of firring-up may be mentioned. This consists of laying thin pieces or strips of wood on the top of the joists or any surfaces to bring them up to a level. Firring-pieces are also sometimes nailed underneath the large beams in framed floors, so that the underside may be level with the bottom of the ceiling-joists, to give a bearing for the laths, and at the same time allow sufficient space for the plaster to form a key. Housing consists of letting one piece of wood bodily into another for a short distance, or as it were a tenon the full size of the stuff. The treads and risers of staircases are housed into the strings, and held by wedges. Housing is likewise adopted for fixing rails to posts. The most common joint, however, between posts and beams is the tenon and mortise joint, either wedged or fixed by a pin. The friction of the wedges when tightly driven, aided by the adhesion of the glue or white lead with which they are coated, forms in effect a solid dovetail, and the fibers, being compressed, do not yield further by the shrinking of the wood. When it is desired to tenon a beam into a post without allowing the tenon to show through, or where a mortise has to be made in an existing post fixed against a wall, the dovetail tenon is sometimes adopted, a wedge being driven in on the straight side to draw the tenon home and keep it in place. In joining small pieces the foxtail tenon has the same advantage as the dovetail tenon, of not showing through, but it is more difficult to fix. The outer wedges are made the longest; and in driving the tenon home these come into action, first splitting away the sides and filling up the dovetailed mortise, at the same time compressing the fibers of the tenon. This joint requires no glue, as it cannot draw out. Should it work loose at any time, the only way to tighten it up would be to insert a very thin wedge in one end of the mortise. Short tenons, assisted by strap bolts, are commonly adopted in connecting large timbers. The post is cut to form a shoulder, so that the beam takes a bearing for its full width, the tenon preventing any side movement. When a post rests on a beam or cill-piece, its movement is prevented by a "joggle," or stub-tenon.

But too much reliance should not be placed on this tenon, owing to the impossibility of seeing, after the pieces are fixed, whether it has been properly fitted. It is also particularly liable to decay from moisture settling in the joint. For temporary purposes posts are commonly secured to heads and cills by dog-irons or "dogs;" the pieces in this case simply butt against each other, the object being to avoid cutting the timber, and so depreciating its value, and also for economy of labor. The double tenon is used in framing wide pieces, and the haunched tenon when the edge of the piece on which the tenon is formed is required to be flush with the end of the piece containing the mortise. Tredgold recommended a bridle joint with a circular abutment, but this is not a correct form, as the post is then equivalent to a column with rounded ends, which, it is well known, is unable in that form to bear so great a load before it commences to yield. A strut meeting a tie, as in the case of the foot of a principal rafter in a roof truss, is generally tenoned into the tie by an oblique tenon, and the joint is further strengthened by a toe on the rafter bearing against a shoulder in the tie. Tredgold strongly advised this joint being made with a bridle instead of a tenon, on account of the butting surfaces being fully open to view. A strut meeting a post, or a strut meeting the principal rafter of a roof-truss, is usually connected by a simple toe joint. The shoulder should be cut square with the piece containing it, or it should bisect the angle formed between the two pieces. It is sometimes made square with the strut; but this is incorrect, as there would, in some cases, be a possibility of the piece slipping out. In ledged and braced doors or gates, this joint is used, the pieces being so arranged as to form triangles and so prevent the liability to sag or drop, which is so difficult to guard against in square-framed work without struts or braces. When a structure is triangulated its shape remains constant so long as the fastenings are not torn away, because, with a given length of sides a triangle can assume only one position; but this is not the case with four-sided framing, as the sides, while remaining constant in length, may vary in position. Among the miscellaneous joints in carpentry not previously mentioned, the most common are the butt joint, where the pieces meet each other with square ends or sides, the mitre-joint, where the pieces butt against each other with bevelled ends, bisecting the angle between them as in the case of struts mitred to a corbel-piece supporting the beam of a gantry, and the rabetted or "rebated" joint, which is a kind of narrow halving, either transverse or longitudinal. To these must be added, in joinery, the grooved and tongued joint, the matched and beaded joint, the dowelled joint, the dovetailed joint, and other modifications of these to suit special purposes. "Flooring laid folding" is a method of obtaining close joints without the use of a cramp. It consists of nailing down two boards, and leaving a space between them rather less than the width of, say, five boards. These boards are then put in place, and the two projecting edges are forced down by laying a plank across them, and standing on it. This may generally be detected in old floors, by observing that several heading joints come in one line, instead of breaking joint with each other. It is worthy of notice that the tongue or slip feather, which in good work is formed generally of hard wood, is made up of short pieces, cut diagonally across the grain of the plank, in order that any movement of the joints may not split the tongue, which would inevitably occur if it were cut longitudinally from the plank.

With regard to fastenings, wedges should be split or torn from the log so that the grain may be continuous, or, if sawn out, a straight-grained piece should be selected. Sufficient taper should be put on to give enough compression to the joint, but too much taper would allow the possibility of the wedge working loose. For outside work wedges should be painted over with white-lead before being driven, this not being affected by moisture as glue would be. In scarf-joints the chief use of wedges is to draw the parts together before the bolt-holes are bored. Keys are nearly parallel strips of hard wood or metal; they are usually made with a slight draft to enable them to fit tightly. If the key is cut lengthways of the grain, a piece with curled or twisted grain should be selected, but if this cannot be done the key should be cut crossways of the log from which it is taken and inserted in the joint with the grain at right angles to the direction of the strain, so that the shearing stress to which the key is subject may act upon it across the fibers. In timber bridges and other large structures cast-iron keys are frequently used, as there is with them an absence of all difficulty from shrinkage. Wood pins should be selected in the same way as wedges from straight-grained hard wood. Square pins are more efficient than round pins, but are not often used on account of the difficulty of forming square holes for their reception. Tenons are frequently secured in mortises by pins, the pins being driven in such a manner as to draw the tenon tightly into the mortise up to its shoulders and afterwards to hold it there. This is done by boring the hole first through the cheeks of the mortise, then inserting the tenon, marking off the position of the hole, removing the tenon, and boring the pin-hole in it rather nearer the shoulders than the mark, so that when the pin is driven it will draw the tenon as above described. The dowelled floor gives another example of the use of pins.

Nails and their uses are too well known to need description; it may, however, be well to call attention to the two kinds of cut and wrought nails, the former being sheared or stamped out of plates, and the latter forged out of rods. The cut nails are cheaper, but are rather brittle; they are useful in many kinds of work, as they may be driven without previously boring holes to receive them, being rather blunt-pointed and having two parallel sides which are placed in the direction of the grain of the wood. The wrought nails do not easily break, and are used where it is desired to clench them on the back to draw and hold the wood together. The following table gives the result of some experiments on the adhesion of nails and screws:

Adhesion of Nails (Experiments by Mr. Bevan).

Description of Nails used.	No. to the lb. avoir.	Inches long.	Inches forced into wood.	Lb. pressure to force in dry Christiana deal.	Lb. pressure to extract from dry Christiana deal.
Fine sprigs	4560	0.44	.40	—	22
Fine sprigs	3200	0.53	.44	—	37
Threepenny brads.......	618	1.25	.50	—	58
Cast-iron nails	380	1.00	.50	—	72
Sixpenny nails..........	73	2.50	.25	24	—
" "	"	"	.50	76	—
" "	"	"	1.00	235	187†
" "	"	"	"	end grain	= 87‡
" "	"	"	1.50	400*	327
" "	"	"	2.00	610	530
" "	"	"	"	end grain	= 257
Fivepenny nails.........	139	2.00	1.50	—	320

Summary.	Across grain.		With grain.
Adhesion of nails in deal..................................	2	to	1
Adhesion of nails in elm	4	to	3

Entrance to extraction is as 6 to 5.

Common screw .2 in. diameter = 3 times the adhesive force of a sixpenny nail.

Spikes are nearly of the same form as nails, but much larger, and are mostly used for heavy timber work. Trenails are hard wood pins used in the same way as nails. In particular work with some woods, such as oak, they are used to prevent the staining of the wood, which would occur if nails were used and any moisture afterwards reached them. Compressed trenails are largely used for fixing railway chairs to sleepers, as they swell on exposure to moisture and then hold more firmly. Screws are used in

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* Or 4 blows by a weight of 6.275 lbs. falling freely through 1 foot.

† These nails required a pressure of 327 lbs. to extract them from dry elm; 507 lbs. from dry oak; 667 lbs. from dry beech; and 312 lbs. from green sycamore.

‡ These required a pressure of 257 lbs. to extract them from dry elm.

situations where the parts may afterwards require to be disconnected. They are more useful than nails, as they not only connect the parts, but draw them closer together, and are more secure. For joiner's work the screws usually have countersunk heads. Where it is desired to conceal them, they are let well into the wood, and the holes plugged with dowels of the same kind of wood, with the grain in the same direction. For carpenter's work the screws are larger, and have often square heads. These are known as coach-screws. The bolts, nuts, and washers used in carpentry may be of the proportions given in the following table:

Thickness of nut............$=1$ Diam. of bolt.
....ditto head....... $=\frac{3}{4}$ ditto.
Diameter of head or nut over sides.....................$=1\frac{3}{4}$ ditto
Side of square washer for fir$=3\frac{1}{2}$ ditto
Ditto ditto oak$=2\frac{1}{2}$ ditto
Thickness of washer........$=\frac{1}{3}$ ditto

The square nuts used by carpenters are much too thin, unless they are equal in thickness to the diameter of the bolt. The full advantage of that diameter cannot be obtained, the strength of any connection being measured by its weakest part. A large square washer is generally put under the nut to prevent it from sinking into the wood and tearing the fibers while being screwed up, but it is also necessary to put a similar washer under the head to prevent it sinking into the wood. This is, however, often improperly omitted. Straps are bands of wrought iron placed over a joint to strengthen it and tie the parts together. When the strap is carried round one piece, and both ends secured to a piece joining it at right angles, as in a king-post and tie-beam, it is known as a stirrup, and is tightened by means of a cottar and gib keys. When straps connect more than two pieces of timber together, they are made with a branch leading in the direction of each piece, but they are usually not strong enough at the point of junction, and might often be made shorter than they are without impairing their efficiency; sockets are generally of cast-iron, and may be described as hollow boxes formed to receive the ends of timber framing.

The author has confined himself principally to those joints which are used in general construction; many others might have been mentioned, but not being required in ordinary practice they have been omitted, to avoid extending the paper to an undue length. He cannot conclude without acknowledging the assistance he has derived from the works of Tredgold, Rankine, and others, which contain valuable tables and information relating to the subject under consideration. His thanks are also due to the council of the City of London College for the loan of a set of fifty models of joints. Other joints were illustrated by a few larger models, and one of these, a quadruple dovetail, may be referred to as a curiosity of jointing.

FIREPROOF BUILDINGS.

From "The Engineer."

So-called fireproof buildings were, as most of our readers know, first introduced in Lancashire for the construction of cotton mills. The brick walls carried arched floors of brick, supported on cast iron beams resting on the walls and on intermediate rows of columns. The well-known Murdoch is believed to have devised the general outline of such structures, and their rapid introduction which followed was due not merely to the supposed immunity from conflagration, but to the great increase of stiffness of these beam and arched floors as compared with those of timber previously in use, whereby it was thought that a large per centage of the actuating steam power, previously wasted by the yielding nature of the shaft and gear bearings, was saved. For some time no noticeable conflagration took place in any of these "fireproof" mills, which continued to multiply, with little variation from the original type of structure, until at length one of them took fire; and although it could not be said to have been burnt down as one of the old timbered, floored and roofed buildings might have been, still the structure was as effectually destroyed, the heavy floors being broken down in succession as the cast iron beams and columns, softened by heat, gave way, and the floors fell upon each other, the walls also being split and distorted by the unequal expansion of their materials. Still, fireproof structures—in name, at least—continue to be produced both for mills and for stores; and amongst the many large examples to the latter purpose were the colossal fireproof structures of the Albert Dock warehouses at Liverpool, in which some very notable improvements in the details of the connections of the columns and beams of the floors, and some decided errors in the design of the beams themselves, were noticeable. It is to be regretted that no account suitably illustrated has, so far as we are aware, ever appeared of those ponderous, and in many respects meritorious structures, the designs of which emanated from the office of the late Mr. Jesse Hartley, then engineer to the docks trustees of the port of Liverpool. During the quarter of a century intervening between 1830 and 1855 several great conflagrations occurred in large warehouses, composed almost entirely of iron and brick, and sometimes with more or less timber in the floors, which demonstrated how small was the title of such structures to be called fireproof. The tide of professional and public acquiescence in the fireproof pretensions of brick and iron structures began to turn, and in a few years more there were not wanting those who were presumed to be authorities who boldly affirmed that a fireproof building was an impossibility. Amongst those who gave prominence to this oracular and sweeping condemnation were the late Mr. Braidwood, of the Metropolitan Fire Brigade, and Mr. Bidder, with a good many others of smaller note as authorities. Some unseen circumstances everywhere—except perhaps in Lancashire—tended to prevent much consideration being given by competent engineers and architects to the subject of fireproofing, prominent amongst which has been the fact that fire insurance companies have deemed it their policy to discountenance any attempt to construct genuine fireproof buildings, as not only sapping the very foundations of their business, but on the grounds that an occasional conflagration is advantageous to them as inducing the extension of fire insurances. Now, we venture to affirm that it has not been proved, and so far is not provable, that fireproof buildings in some form are an impossibility; and moreover, the subject of their construction has never yet met with a truly scientific and adequate investigation. Little progress is likely ever to be made while the general design of buildings proposed to be rendered fireproof continues to follow those of buildings constructed upon long-established models, into which combustible material largely entered, every detail being dictated in fact by the use of such materials, and without reference to the question of indestructibility by fire; nor are we ever likely to make much progress while the minds of architects and engineers are led away from the true problem by paltering with those designs which profess to make fireproof structures out of materials in great part destructible by fire, such as the cheap mongrel constructions in which the floors of large buildings are formed of wood imbedded in concrete, the whole supported on rolled iron joists. Various expedients have been proposed, and to a certain extent adopted, with a view to retard the progress of destruction by fire in buildings into which timber and like combustible materials enter largely as constituents. Such were the inventions of Lord Stanhope, at the close of the last century, those of Fox and Barret, and of a host of others of more recent date, including the patent of Evans and Swain, described in *The Engineer* of 29th ult.; but all these, however partially useful, do not properly belong to the class of truly fireproof buildings, to which our present considerations are limited. These, like the far simpler and cheaper scheme proposed in the last century, of covering the lower side of wooden floors with sheet iron, may no doubt delay the progress of fire, but it is a mere delusion to suppose it can ever form parts of a fireproof structure. The choice of materials available for the construction of a genuinely fireproof building is but small; cast iron, wrought iron, and steel are the only metals available to us; these may indeed constitute the sole material of even the largest fireproof structures, as in the case of the Custom houses of Valparaiso, designed by Mr. Lloyd, which are of great magnitude and wholly of wrought iron, and which have considerable pretensions also to rank as earthquake-proof—buildings suitable to that shaky country. But in general, large structures must consist in great part of non-metallic materials, that is, bricks or stone. To the former of these we must mainly look, as by its means we may adapt the forms of the integrant parts of the brick-work, so as best to permit, without dislocation, the free expansion and contraction of large masses, and yet prevent, as one effect of rapid heating or cooling, the total dislocation of the structure. The composition of brick-earths, which, in fact, may include all forms of pottery ware, may admit of being so varied as to produce but a minimum change of volume under considerable differences of temperature. Almost the whole of our commonly employed and useful building stones are excluded by reason of their natural rigidity of structure, which causes them when somewhat rapidly heated to spall off, crack, and fly to pieces. Yet much remains to be investigated in respect of this branch of the subject. There are some stones well adapted for building, admitting of division in good flat beds and joints, readily allowing the use of the punch and chisel or stone-cutter's saw, which yet may be rapidly heated or cooled without disintegration. Amongst these are certain trappean rocks and those of consolidated ash. There are some metamorphic rocks, chiefly of Silurian age, into which magnesia enters largely as a constituent, which are very infusible, have but a small coefficient of dilatation, and bear rapid heating and cooling, even through considerable ranges of temperature, without becoming split or shattered. Stones of this class, which pass by insensible degrees into steatite, which is found in vast masses in Northern India, is employed in many parts of Europe, Asia, and the United States as a fireproof material for the lining of stoves or fireplaces for domestic or other purposes, and stone of this class are well adapted for the walls of fireproof buildings. Those well acquainted with the products of volcanic countries cannot but be impressed, by the phenomena there often presented, with the fact that many volcanic tufas are capable of being formed into bricks by suitable choice and admixture, so that they may be subjected to certain and violent heating without any cracking or dislocation in a far higher degree than any fire or other bricks hitherto employed; indeed, not far from Bonn, on the Rhine, bricks are made of pumice mixed with other volcanic material, which are largely employed in Germany for oven and furnace building, and are well suited for fireproof structures. The range of temperature through which building structures may be raised by accidental conflagration is not accurately known. Like every other branch of the whole subject, our knowledge is misty, and still awaits a careful investigation by exact methods of the physicist experimenting in concert with the scientific engineer or architect. We may roughly affirm, as experience shows, that the temperature to which burnt-out buildings have been exposed is generally greater as the area of conflagration is greater, and that even in the largest fires the highest temperature reached but just touches the fusing point of cast iron. Some instances have been recorded in which materials have been fused, indicating a temperature considerably in excess of this, but a more careful examination than appears to have been given would most probably show that in these cases the high temperature has been due to local and exceptional causes, such as the storage of nitre or other oxygen-yielding compounds in contact with combustible drugs, such as velonia, resins and the like, or have been due to the conflagration having been urged at certain points by rushing currents of air, produced either by high wind or by upward currents of already heated air. We have thus some data for determining the amount of expansion to which the brick-work or masonry is likely to be exposed. Were we only in possession of a large range of exact determinations of the coefficients of dilatation of the materials themselves, such experimental researches would be the real alphabet of the whole subject of fire-proofing. We then come to the more complex questions as to how we are to combine the integrant parts of the walls and floors so that we shall admit freely their irresistible expansion and consequent contraction without dislocation or other destruction of the masses of brick or stone work. It would far exceed the scope or space here at command further than thus to hint at the subject to be systematically examined. We boldly affirm, however, that the point has never yet been approached in a truly scientific, and pursued in an exhaustive manner, and that until this shall have been done by really competent men, who, for the work, must possess much other qualifications than those of mere engineers or architects, it is both premature and unwise to dogmatize, or oracularly to affirm, that a fireproof building is an impossibility. We are able to build furnaces many of which are equal in capacity to houses or stores. The limits of form and structure of these have been in the course of long experience determined, so that none need doubt the capability of a new furnace to withstand the effects of the high temperature to which it would be exposed. Why should the principles and methods which have led to success here be impossible when the interior cavity of the structure is designed for something else than merely a receptacle in which fuel is to be burnt? Whether we design a furnace or a fireproof building—which to be really fireproof is but a furnace applied to a new purpose, and perhaps upon a larger scale—two entirely different though intimately connected classes of conditions have to be provided for: no essential part of the structure must soften or melt at the highest temperature to which it would be exposed; the materials of the structure, and the structure viewed as a whole, must be so put together that its parts—chiefly walls, floors, and staircases—must be perfectly free to expand and contract again, while at the same time capable of resisting such extraneous mechanical forces as must be applied to them by reason of the necessary uses of the building itself. We are not here writing a treatise, however brief, upon the practice of fireproof construction—how these conditions, admittedly complex and difficult, are best to be fulfilled; such a treatise to be of the least use, must involve much space, and thoughtful consideration in detail. All we here propose is to point out what are the conditions that must be fulfilled, and to add, chiefly by way of example, a few negative propositions, indicating what must not be done if we are to attain the object. The dimensions of the separate chambers into which the building, however large as a whole, may be divided, must not exceed a certain and very moderate limit; and this must bear a certain proportion to the area and transverse resistance of the floors, and to the construction and mass, the nature of the perforations for the admission of light, etc., of the walls. Some guiding analogy as to all this we already possess in the dimensions which experience has assigned to the thickness and modes of construction of the walls of furnaces, as compared with the size of the heated cavity within. External walls—perhaps, indeed, dividing walls also—should be double, with a space between, so that each wall shall be free to alter its dimensions irrespective of the other. No portion of the floors must be rigidly bound into the walls, or so that each floor shall not be free to expand or contract without involving injury or destruction to the walls themselves. The ideal to be approached should be that of a flat plate—the floor, however constructed, viewed as a whole—resting simply upon a flat ledge or projection inwards, or from the interior side of the walls, with a free space all round the plate to admit of its expansion, and the grasp of the plate upon the walls by friction to be nowhere such as to drag in with it the walls as the former contracts. Complicated forms in ground plan, in which architects so much delight, with curves and reëntering angles, should generally be avoided, and the forms of the chambers should consist of simple paralellopipeds; and the staircases would probably best be external and separate from the building, and divided at frequent intervals of the ascent by wrought iron doors, to cut off ascending draughts of air. Stone staircases, especially as ordinarily constructed, with each step built into the walls at one end, and the remainder overhanging, are wholly inadmissible; and perhaps the best hints for construction in detail might be derived from the spiral ascents or winding roadways available for carts and mules, and having no steps at all, but merely being twisted planes, which exist in the great church of St. Peter's at Rome, and give access to its vast roofs. The total height above the ground of the building should not be excessive—perhaps three stories reaches the limit in buildings intended to be stored with merchandise; stores of eight or more stories in height, such as are to be found at London, Liverpool, Marseilles, and many other great ports, would tear themselves to pieces, no matter how constructed, as the effect of a persistent conflagration in any of the lower storeis. Lime, mortar, or any analogous cement destructible by a red heat, is inadmissible, and perhaps the most available cements will be found in siliceous or silico-ferruginous powders mixed with soluble alkaline or with earthy silicates into a plastic mass; these will adhere strongly to almost any brick or stone. Much might be said, too, as to how, and with precautions, fireproof buildings should be treated and managed. If nitrate of soda in bags, rough brimstone in heaps, and coffee beans or oil seeds in bags, are to be stowed away in the same loft, as may be often observed, the utmost perfection conceivable in fireproof construction is useless and thrown away. We might as well expect a magazine filled with gunpowder to withstand the effects of its ignition.

Space, however, forbids our pursuing the subject further here.

THE NEW THEORIES OF THE MOTION OF FLOWING WATER.

By Dr. G. Hagen, Director of the Engineering Service of Prussia.

Translated, for Van Nostrand's Magazine, from "Zeitschrift für Bauwesen" for 1868,

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In the year 1850 the Congress of the United States ordered an exact investigation to be made of the regimen of the Mississippi River, in order to be able to prevent with some certainty the devastations which had so often previously happened in times of flood. The two engineer officers detailed for this purpose, Capt. A. A. Humphreys and Lieut. H. L. Abbot, published in 1861 the important results of this investigation under the title "Report upon the Physics and Hydraulics of the Mississippi River." The observations there communicated are without doubt highly important for river engineering, and they contain also many facts which may serve to clear up the mechanical relations existing in the flow of large masses of water. The authors have also endeavored to deduce therefrom general laws, and these have here in Germany not only excited great attention, but have, since the appearance of the translation, been regarded for the most part as completely proved. This translation, comprising the most important chapters of the Report with several Appendixes, was made by H. Grebenau, a Bavarian engineer officer, and was published at Munich in 1867 under the title "Theorie der Bewegung des Wassers in Flüssen and Canälen."

I have only lately succeeded in obtaining a copy of the original Report, and as my perusal of it has not, as I had hoped, cleared up the many doubts which pressed upon me in reading the translation, it appears to me necessary to subject the method by which these laws were found, and endeavored to be substantiated, to an open criticism. But if thereby these supposed theories are not to be accepted as such, no doubt therefore arises against the observations themselves; but we may rather conclude, from the lack of agreement between them and those theories, that they have been communicated complete and unchanged. It also cannot fail to be recognized, from the description of the methods of measurement, that they were carried out with expertness and judgment. Undoubtedly they belong among the best hydraulic measurements which we possess, and the fact that they were made upon so great a river as the Mississippi, gives them a yet higher value. It is however to be regretted, that the observations were not directly given, but are only stated by averages; also that some details were left undescribed, which were by no means unimportant. Nevertheless they contain as a whole very important data, which promise to give some disclosure concerning the hitherto so little known laws of the motion of water in river beds. I have begun an investigation of this kind, and in case of success will publish my results at some future time.

I. THE VARIATION OF VELOCITY AT DIFFERENT DEPTHS.

On pages 230–232 of the original (pages 56–63 of the translation) the authors record the observations which they have used in deducing the law of the variation of velocity at different depths, for the same point of a stream under a constant flow. The single observations are however not given, but with one exception, 2, 4, 8, 9, 10 or 16 measurements are combined. The tables contain only the averages of these combinations, and they consist of six groups, the first three of which give the observations made at high stages of water, and the others, those made at low stages.

Group I refers to a water-depth of 110 feet, and contains in thirteen series the averages of the velocities resulting from seventy-two series of observations, each made at nine different depths. Group II has a depth of seventy feet, and gives in eight series the forty-two velocities measured at seven different depths. In group III the water is fifty-five feet deep, and there are four series resulting from twenty-eight measurements, each made at six different depths. Group IV—water-depth 100 feet,—has five series of averages from twenty-four observations, at fifteen different depths. Group V—water-depth eighty feet—contains three series of twenty observations, at thirteen different depths. Lastly, group VI has a water-depth of sixty feet, and contains six series of thirty-six observations, each made for eight or nine different depths. There are hence communicated in these six groups and thirty-nine series of averages the results of 222 series of observations. The authors expressly mention that they have deduced from the latter the law of the variation of velocity at different depths.

The velocity measurements were made by establishing on shore two cross-section lines 200 feet apart, and observing the passage of floats with theodolites. The floats consisted of kegs fifteen inches high and ten inches in diameter, which were so loaded as to sink and remain upright. They were connected with surface floats of cork about eight inches square and three inches thick, submerged an inch and a half. The connecting cord was of the proper length so that the keg floated at the depth where the velocity was to be measured. This apparatus was placed in still water on a very windy day, and it remained immovable, from which it was concluded, that it was not only free from wind influence, but that it actually gave the velocity of the water at the depth of the keg. In perfect strictness this is not the case, and it would have been more suitable to give to the upper float the form of a boat, and to so fasten the cord that it should always keep parallel to the current, in order to have the least possible resistance.

This apparatus was thrown into the water from a boat lying at anchor at a suitable distance above the upper section-line, so that it should acquire the velocity of the water before the beginning of the observation. The time of passage past the upper section-line was noted by an observer with a theodolite, who then followed the float till it passed the second line. The passage of this was noted in a similar way by a second observer, who also indicated to the first the time by a loud shout, and the latter then measured the horizontal angle made by the float with the base line. In this way the distance of the float from the bank or the base was determined. The boat remained at the same spot while the floats were sent out for the different depths, but the order of observing was continually varied in every possible manner, in order to counteract, as far as possible, any change in the flow of the river then occurring.

The places in the river where the observations were made, or different bases measured, are only partly, and even then very incompletely described. We cannot learn whether the given depths of water continued for some distance or whether deep pools existed. The observations given by averages refer twenty-seven times to the place where the "prime base" was measured. This, according to a small sketch given in Fig. 2, Plate III, is situated near Carrollton, just below a very sharp bend of the river. The same drawing also shows that the "Race course base," for which four series are given, lies lower down the stream where it is very straight. Of the three other places of observation, at the "Locks base," the "Baton-Rouge upper and lower bases" nothing whatever is stated. The breadth of the river at these places is also nowhere given, although the distance of the float from the base is always recorded. According to the small sketch, the Mississippi is about 2,200 feet wide at the first base and 2,500 feet at the second.

In these tables the velocities are recorded to the ten-thousandth part of a foot, or accurately to the 30th part of a millimeter. Such exaggeration may influence favorably those readers who do not understand the matter, but in general it only shows that the investigations were carried on with reference to the degree of precision attainable. The two section-lines were 200 feet apart, and the velocities were found by dividing this distance by the number of seconds which the floats occupied in passing from the upper line to the lower. Conversely if we compute these times from the given velocities, we find, for example, from the first series of group I, 30, 30, 30, 30, 30, 31, 32, 34 and 38 seconds—every time a whole number of seconds. To measure halves or tenths of seconds is more difficult, and when, as here, one observer measures the beginning and another the end of the phenomena, such divisions must remain inaccurate, especially when the time is also to be marked by signals or shouts. Hence in these measurements half of a second is not to be relied on, or the probable error is equal to half a second at least. For this case then it is an even wager that the error in the computed velocity is not greater than $\frac{1}{60}$, that is, not greater than 0.1 feet. The first decimal place can hence not be regarded as accurate, while the authors give four.

It is still more remarkable that we find commonly only entire seconds in those cases where the given velocities are the averages of two series of observations. This happens for all the depths in series 2, 3, 12, and 13 of group I, and in the first and last series of groups II and III. The two observations must hence have always had the same number of seconds, and this in itself is not probable. Further, it seems strange that, without any particular reason being given for it, at all the depths in some series the velocities are only expressed by two decimals, cyphers being introduced for the last two; this happens in series 6, 7 and 8 of group I, and in series 3 and 5 of group II. Lastly it may be mentioned that in five series of group II, comprising thirty-six series of observations, the depth of water is given as sixty-five feet, while velocities are said to have been measured at a depth of sixty-six feet.

These are the 222 series of observations, from which the authors have deduced a very simple law for the variation of velocity at different depths, that with incredible precision agrees with the averages of the observations. The result is that the velocity-scale is a common parabola whose axis is horizontal and at a depth $0.297D$ below the surface, D representing the depth of water. The velocity v for any distance d from the axis of the parabola, is given by the expression

$$v = 3.2611 - 0.79222d^2,$$

in which d is taken in parts of D. The values of v computed from this formula for $0.1\,D$, $0.2\,D$, etc., were compared with the mean values from the 222 series of observations and an agreement within a few thousandths of a foot found for each point. The greatest difference is 0.0066 feet. The square root of the mean of the squares of all these differences is 0.0033, and hence the probable error is only 0.0023 feet. This accordance surpasses so greatly the imaginable limit of precision, that it not so much confirms the result, as renders it suspicious in the highest degree. The method by which the law was found and compared with the observations is described on pages 230–234 of the original (pages 57–65 of the translation).

In this method,—the conviction having been first arrived at that the velocity decreased from the surface downward and then again decreased toward the bottom—each single series of observations was plotted on so large a scale that thousandths of a foot of velocity were distinguishable. The drawing had hence a much greater precision than the measurements which it represented. Between these points a curve was then drawn which conformed to the above condition and agreed the most closely with the observations.

The reader cannot repeat this process, since only a single series of observations, separate from others, has been communicated to him. For this series a curve may be drawn quite agreeing with the above requirement. The other series are, however, combined and only the averages are given. In this combination the errors have already been partly eliminated, and the curves, which may be drawn for them, must correspond much closer to the true law than in the single series; but, nevertheless, they exhibit excessive anomalies which make every trial of this kind seem entirely arbitrary. If the law of the curve were already known the most probable values of its constants might be accurately found by the Method of Least Squares; but here it might easily happen that any other curve or even a straight line would result as more probable than that assumed. These plots, however, served only for the discovery of the curve, and hence nothing remained but to draw it by eye. What might be done by such drawings may be easily seen if we represent graphically some of the recorded series.

For example, Fig. 1 represents series 4 of group I, Fig. 2 the series 2 of group II, and Fig. 3, the series 3 of group III. These belong indeed to the most irregular, but they are already the result of the combination of 10, 8 and 16 single series of observations. In these last the anomalies must be still greater. But although even in these combinations such great deviations occur,—for example, in Fig. 2 the velocity continually increases with the depth to a point below the bottom of the river,—the mean values of all 222 series of observations agree with the parabola shown in Fig. 4 to such a precision that no deviation would be perceptible upon a much larger scale.

In explanation of these figures it must

Fig.4

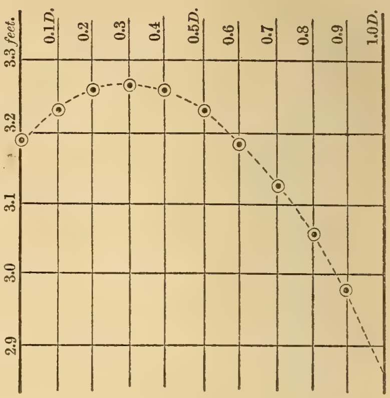


Fig.3

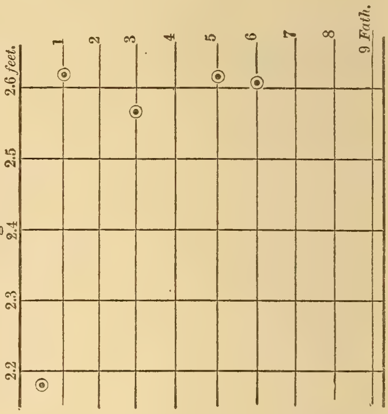


Fig.2

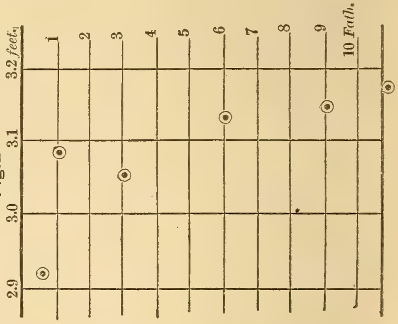
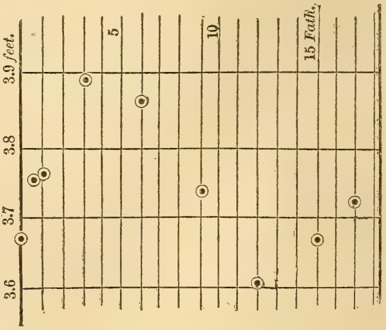


Fig.1



be mentioned that the depths, in fathoms of 6 feet, are represented by the vertical ordinates, and the velocities in feet by the horizontal abscissas. If the authors selected for the graphical representation a scale fifty times as large as that here used, the difficulty of drawing the curves would certainly not be lessened, but rather increased.

Another way of treating the observations must here be alluded to. It is required to know the velocities for each series not merely at the surface but also at the depths $0.1D$, $0.2D$, and so on to $0.9D$, D being the total depth of the river. For this not only the sketched curves were used, but the values sought "were found by the most accurate interpolation that could be made." One cannot understand how an interpolation can be made between given quantities which exhibit such irregularities as in the case before us. The authors however succeeded so completely that the averages exhibit a fabulous harmony.

In order to find the kind of curve given by these averages the authors assume (as Dubuat did in the last century for another problem) that it must be a conic section. A comparison of the upper and lower branches showed that the axis lay very nearly at a depth $0.3\,D$ below the surface. Then each two of the computed velocities were used to find the coefficients of the two terms which contain the first and second powers of the abscissa (the vertex being the origin of coördinates), and it appeared that the coefficient of x^2 was very near zero, and hence the curve a parabola. The problem then was, to find that parabola agreeing best with the observations. Three constants were to be determined, namely, the abscissa and ordinate of the vertex and the parameter, the axis being assumed as horizontal. The Method of Least Squares, by which this problem may be accurately solved, was entirely unknown to the authors; they used therefore another graphical process and drew upon transparent paper many parabolas of different parameters. These were laid upon the points plotted from the averages of the observations, and in this way the best parabola and its best position determined. And here it is said that "after a little experience it is seldom necessary to make more than one or two trials." The agreement with the computed velocities is, as already remarked, excessive; it would, however, have been greater if a methodical process had been employed, for the differences although small are very regular, and may hence be lessened by a better choice of the constants.

The question remains to be answered, how this accordance of the new law with the observations above pointed out was caused. That the errors should have annulled themselves so completely by chance, is certainly not to be thought of, for the probability of it is far too small. From the thirty-nine recorded series, composed with one exception of many series of observations where the probable error has already been diminished in the ratio of the square root of the number of series, it results that the probable error of a single observation is greater than 0.1 feet. The assumption of this as its value is perfectly justifiable for velocities of 2 feet or over, for periodic oscillations prevail in rivers of a strong flow which exert great influence upon the measurement. If the probable error of a single observation is 0.1 feet, that of the arithmetic mean of 222 observations is

$$\frac{0.1}{\sqrt{222}}=0.0067 \text{ feet.}$$

But, according to the computations of the authors, we find this probable error as mentioned above, equal to

$$0.0023 \text{ feet.}$$

The probability of so small an error is only $\frac{43}{957}$ or 0.045. And the probability that this very small error should occur ten times in succession, namely at each of the depths, is,

$$0.045^{10}=0.000000\ 000000\ 0335.$$

We can hence wager thirty-billions against one that such an elimination of errors would not occur of itself. In order to obtain an idea of the extremely small value of this probability, let us consider that a certain event occurs once every second, and that it may take by chance a particular form whose probability is the same as that of the above adjustment of errors, then we could only expect this particular form to appear once in a million years.

This form has however appeared, and from the probability of other causes having produced it, we can determine the probability of those causes themselves. One such might have been an intentional selection made from the data of observation, those being rejected which did not agree with the law assumed as true. This cause is in itself not improbable. Persons who are not accustomed to scientific accuracy, generally consider such a proceeding as allowable and perfectly justifiable. In the case before us where the measurements were carried on under official, and hence at least under formal control, it might not have been easy to reject single observations, against which there was no suspicion when taken in the field. Moreover if there had been such rejections, the series, which so apparently contradict the law, as those represented graphically above, would not have been communicated. Lastly, it must be pointed out that the rejection of some series of observations might lead to a moderate reduction of errors, but that through this alone the agreement actually obtained cannot be explained. The probability for it, although perhaps ten or even 100 times greater than the former, would remain so very small, that chance could not be regarded as the cause of the event.

There is, however, another and indeed a very plausible explanation of this agreement. Let one try to draw curves with the above given properties, for the series here graphically shown, and it will be evident that the operation is entirely arbitrary. The most different curves have equal validity. The curves may be altered throughout their whole extent, or in chosen parts, without introducing an error. In this way an easy way was offered of making the observations agree with any curve which had been previously assumed, and thus decreasing at pleasure the computed differences. There would indeed have been no difficulty in drawing the curves so as to make the averages of the observations agree with the computation to seven places of decimals, or even closer.

The hydraulic student is obliged to view many principles as true and proved which are really very doubtful, but never before has it been suggested to him that he should accept a demonstration like this and therein recognize an advance of science.

As the supposed law, according to which the velocity-scale for every point in the stream is a parabola, has been found deficient in proof, the further test of the consequences derived therefrom is unnecessary, and its modifications may also be passed over. And if the same agreement is here found with the averages of the observations, it may be explained in a similar way, namely; when the velocities were sought for at different depths than at those observed, opportunity was again offered to draw curves at pleasure. I pass over also the investigation concerning the variation of velocity at different distances from the shore, to take up the problem of mean velocity, so important in river engineering.

II. DEPENDENCE OF THE MEAN VELOCITY UPON THE SLOPE AND CROSS-SECTION OF THE RIVER.

The authors endeavor first (pages 300–301 of the original, 125–128 of the translation) to solve this problem theoretically. They lay great weight on the resistance between the surface of the water and the air, and introduce also other different modifications of the usual assumptions. They arrive by this at an expression which is materially different from those regarded as correct—although without proof—in Germany and France. According to these last the product of the mean hydraulic depth by the relative slope is in general proportional to the square of the mean velocity, and in the more precise investigations is taken equal to the sum of two terms, one containing the first power of the mean velocity and the other its square root. The last expression however does not accord with all observations so well as the first. The authors observed this also and hence endeavored (pages 310–313), without regarding that theoretical development, to deduce an expression agreeing satisfactorily with the measurements. The trial cost, as they expressly mention, "much labor," and led finally to the result that, disregarding the small terms that are of little importance, the square of the mean velocity is proportional to the product of the mean depth by the *square root of the slope.* The expresssion at which they arrived is, for large streams

$$\sqrt{v}=\left(225\frac{a}{p+w}\sqrt{s}\right)^{\frac{1}{4}}-0.0388$$

in which v denotes the mean velocity, a the area of the cross section, p the wetted perimeter, w the breadth of the river and s the slope. For a horizontal water surface, where $s=0$ there is hence a slight flow, and this only disappears for a certain slight slope. This anomaly may indeed be overlooked, since the formula is an empirical one. If we disregard the last unimportant member, the expression becomes

$$v^2=225\frac{a}{p+w}\sqrt{s}.$$

The authors now collect thirty complete observations in which the area and perimeter of the cross-section, the slope and the mean velocity have been measured, and show that their formula accords better with them than any other previously given. Of these observations, nineteen were made on American rivers and canals, two are taken from Dubuat's measurements, one was made by Watt on a very small canal, five by Kragenhof on different branches of the Rhine, one on the Tiber and two on the Neva.

The coincidence shown between these observations and the new formula is by no means too great, for the error in velocity is once found 0.8 feet, and the probable error is nearly three inches. But by assuming the more simple expression

$$v^2=kts^x,$$

in which t is the mean depth, x the unknown exponent of the slope and k a constant factor, I found from all of Dubuat's observations, from Woltmann's and the numerous ones made by Lahmeyer on the Weser, that for $x=1$, the sum of the squares of the errors was smaller than for $x=\frac{1}{2}$. According to the observations of Dubuat, this sum of the squares was greater for $x=\frac{1}{2}$ than for $x=1$ in the ratio of 5 to 2, for Woltmann's in the ratio of 3 to 1, and for those of Lahmeyer in the ratio of 2 to 1. These measurements do not therefore confirm the new formula, but those made on the great American stream, which conclusively point to $s^{1/2}$, certainly do. The matter is hence not yet clear, but we may perhaps assume that measurements made upon small and upon great rivers are connected in other ways than through the exponents of the slope. To this is to be added, that the first power of the slope is in itself far more probable than the root.

As far as the observations instituted in America are concerned, it is here again to be regretted that many important data have not been published, and that the details of the measurements remain almost entirely unknown. Nevertheless they are, in comparison with previous observations (which certainly leave much to be derived) of great importance, and in further investigations on the flow of water in rivers they will certainly not be disregarded.

SOFT STEEL AND INGOT IRON.

From the "London Mining Journal."

A PROCESS for the manufacture of so-called soft steel or ingot iron, containing as low a percentage of carbon as .10, or possibly lower, or (say) containing an amount of carbon ranging from .18 to .10—that is to say, a soft steel can be made at any notice containing any desired amount of carbon within the range mentioned, has been patented by Messrs. Harvey, of North Woolwich, who propose to produce a soft steel especially suitable for wire ropes, telegraph wires (where very low electrical resistance is required), cable wire, and wire of various kinds which have to stand great elongation, twist, breaking strains, or other tests. Also for engines and other forgings, nail-making, plates, sheets, hoops, and every purpose where metal is required superior to charcoal iron, and where greater tensile strength, ductility, and toughness are required than can be obtained from the best wrought-iron worked in the ordinary way. The furnace preferred for this manufacture is the well-known so-called "Siemen's regenerative" open hearth steel melting furnace, which is heated by coal gas and atmospheric air; or other regenerative gas or reverberatory flat-bottomed furnace may be substituted, capable of supplying a heat sufficient to melt wrought-iron, and keep the said wrought-iron in a fluid or molten state for some time. A tap-hole is provided at the back of the furnace through which the finished metal is run out. The said metal may first be run into a ladle of requisite capacity as is usual, and from thence it is tapped into round, square, or other shaped moulds, as may be required to suit the purposes for which it is intended.

The improved process consists in charging on the bottom of the furnace (such furnace to have attained previously the highest temperature, or nearly so, which it can command) from 40 to 50, or (say) 45 per cent. (of total charge exclusive of ore) of pig-iron, the quality of which pig-iron shall be hereinafter specified, to which is added on charging 10 per cent. of ore, which ore will begin to oxidise the carbon contained in the pig-iron as soon as the latter melts, and if the pig-iron contains less manganese that 1.5 per cent., the manganese in the pig-iron must be made up by charging with the pig-iron from 1 to 4 per cent. of spiegeleisen containing 20 per cent. of manganese; thus, if the pig-iron contains about 1.5 per cent. of manganese no spiegeleisen is charged, but if it contain (say) only .5 per cent. of manganese then about 2 per cent. (of total charge exclusive of ore) of the said spiegeleisen should be charged. Different raw material is used according to the tests which it is intended that the finished metal should be best able to bear.

When the whole is melted a little more air is put on in order to assist in decarburising the metal, and the full heat is kept up until the ore has all worked off the surface of the metal, or until the ore has become reduced by the action of the carbon contained in the pig-iron. At this time the metal will be seen to sink below the former level, and after some time it will boil uniformly over the entire surface. A sample of the boiling metal should now be taken out, cooled in water, dried, and hammered on a smith's anvil; if on the first blow it breaks and flies 1 cwt. (about) of ore should be thrown in, which operation is repeated until the sample stands a blow on the anvil without fracture, thereby indicating that the metal contains from .60 to .75 per cent. of carbon. When this point is reached from 30 to 40 per cent. (of total charge, exclusive of ore) or (say) 35 per cent. of wrought-iron or soft-steel scrap is charged on the banks of the furnace, in small quantities at a time (say) 5 to 10 cwts. at either end, which wrought-iron, in whatever shape, is allowed to melt almost entirely by heat while still on the banks, without being pushed into the bath of metal. That portion, however, of the wrought-iron or soft steel scrap which is charged much above the level of the metal may with advantage be turned in when at a good white heat, in order to save waste by oxidation. The object of this mode of charging the wrought-iron or soft steel scrap is to prevent the cooling of the molten metal, which would probably result if the charges were introduced without previous heating, thereby incurring the risk of portions assuming a pasty condition, instead of being uniformly thin and well melted. Should the furnace, however, be working very hot, some of the wrought-iron or steel scrap may be turned in with advantage when at a good heat. During this melting in of the wrought-iron or steel scrap the flame should be kept bordering on a non-oxidising quality.

By running in the wrought-iron or soft steel scrap as described—at this period, it liquefies and renders very thin the thick scum or too silicious slag which accumulates on the surface of the molten pig-iron after being nearly decarburised by the ore, as is the case in working the so-called "pig and ore" process, thereby preventing the heat (gas and air) from penetrating through the metal, and so retarding the decarburising effect of the oxygen contained in the flame, and increasing the liability of the metal to assume a pasty condition and sink to the bottom by reason of the deficiency of heat; but after the wrought-iron or soft steel scrap is run in, the slag being in a very liquid state allows the heat and flame to penetrate perfectly, thereby preventing the metal from sinking to the bottom. Owing to the wrought-iron containing but little carbon, it is found that when the whole of it is run in in the manner described, the said wrought-iron by its diluting action and the oxygen in the flame by its decarburising action (during the time the wrought-iron was melting) will have reduced the carbon contained in the metal to not more than .14 or .15, and sometimes even lower, thus reducing the carbon much quicker than by working with ore for the same object, as the time required for the melting in of the wrought-iron does not exceed 1½ hour, though it is possible to work it in less time than this.

When the whole of this wrought-iron or soft steel scrap has been melted and turned well in the bath of metal, a rabble is inserted to ensure a thorough admixture of the wrought-iron or steel scrap with the other metal. A piece of ore should now be thrown into the bath, and if after a minute or two the ore floats on the surface, and there appears to be no action between the metal and the ore, or the ore is only reduced with difficulty, this is an indication that nearly the whole of the carbon has been practically eliminated—that is to say, that the carbon contained in the metal has become so low that neither the ore nor the oxygen of the flame has any perceptible burning or taking out action on the carbon that remains; but if the ore disappears after two or three minutes, this is an indication that a little more ore is required, which is accordingly thrown in at intervals till there is no action, as above explained. When this is the case no more ore is required, but a full heat is kept up for fifteen or twenty minutes, so that the oxygen in the flame may possibly burn out a little more of the carbon, and the metal will thereby be considerably heated.

When nearly the whole of the carbon has been eliminated the surface of the bath of metal will show numerous thin and very liquid pools, like water, and on which the heat seems to have greater effect than on the remaining surface. A sample now taken out will be found to contain about .12 per cent. of carbon. When this point is reached wrought-iron ranging from ten to thirty per cent. or higher (of the total charge exclusive of ore) is introduced, according to the carbon required in the finished metal—that is to say, if soft steel containing .10 per cent. of carbon is required, then from twenty-three to thirty per cent. of wrought-iron must be added. If .15 per cent. of carbon is required, 17 per cent. of wrought-iron must be added, and so on, the quantity charged being in proportion to the carbon required. Such wrought-iron should be put in in quantities of five or six cwts. or more at a time on each bank, and allowed to melt in entirely by the heat of the furnace without pushing in any part thereof, except that which is furthest away from the metal, which should be turned into the bath at a white heat. In this way the metal will maintain its full liquidity and heat, and consequently it will not readily solidify when in the ladle or elsewhere after being tapped from the furnace.

In order to dilute or reduce the amount of the carbon contained in the metal soft wrought-iron must only be here used—that is to say, iron containing less than .12 per cent. of carbon, this latter being about the amount already in the metal; therefore, iron containing .11 or .10 per cent. of carbon or lower is used with advantage for this object, and if a soft steel be required with carbon as low as .10 per cent. it is found best to use iron with nearly all the carbon worked out, or containing (say) .05 per cent., or lower, as then a less quantity can be charged than if the iron contained more carbon. The carbon contained in the whole may thus be reduced to .06 or .07 before charging the ferro-manganese, which added to the small amount of carbon put in by the ferro-manganese will bring it to .10 per cent. in the finished metal. If the carbon contained in the metal before charging in the ferro-manganese should have been reduced to .12 or .15 per cent. by the first addition of wrought-iron, and the amount of carbon in the finished material be required to be not less than .17 or .18 per cent., then the amount or a little more of the ferro-manganese can be charged without putting in the second lot of wrought-iron. The quantity of ferro-manganese (say .7 per cent.), as stated below, will be found amply sufficient, provided the quantity of manganese in the pig-iron charged at first be made up.

When the wrought-iron in this last operation has disappeared from the banks, or has become dissolved by the molten metal, a rabble or bar should be frequently inserted and stirred about in order to assist in bringing the same into close contact with the other metal, thereby rendering it perfectly liquid and uniform with the other existing metal. This rabbling should be renewed at intervals during half an hour after the wrought-iron has disappeared from the banks, in order to bring the metal into contact with the full heat of the furnace before tapping it into the ladle. A sample will now be found to contain from .6 or .7 to .11 per cent. of carbon, according to the quantity of wrought-iron charged at the last operation, and according to the proportion of carbon contained in such wrought-iron. A little burnt lime (say) 15 or 20 lbs., is now thrown in to assist in keeping a fluid slag, and after ten minutes 7 per cent. (of the total amount charged exclusive of ore) of powdered ferro-manganese containing not less than 70 per cent. of manganese is now charged into the centre of the molten bath, which ferro-manganese will melt in about two or three minutes, but in order to secure a perfect melting and union from five to ten minutes should be allowed previous to tapping, and the metal should also be well rabbled before running out.

ENGINEERING NOTES.

By Prof. WM. CAIN, A. M., C. E., Prof. of Mathematics and Engineering, Carolina Mil. Inst., Charlotte, N. C.

Written for Van Nostrand's Engineering Magazine.

1. DECOMPOSITION OF RESULTANT ON A CROSS SECTION.

Conceive a slice BDA, Figs. 1 and 2, of width unity; the cross-section BD, perpendicular to the plane of the paper, being a *rectangle.*

Also suppose the resultant R of all outward forces on the cross section BD, to lie in a plane parallel to the paper, through the center of the slice and perpendicular to the supposed cross-section. Let us suppose *first* that the cross section can develop both *tensile and compressive* resistances as in a solid beam, and that R passes through *a without* the cross section.

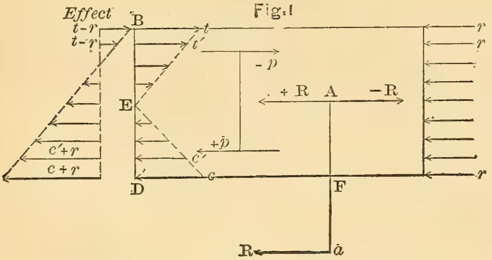


Fig. 1

If at some point A in the medial plane, we conceive two opposed forces +R, —R, parallel and equal to R, the force R with the force —R forms a right handed couple $\overline{RR}$, that can be replaced by the equal couple $\overline{pp}$ or the forces, $t, t' \ldots . c', c$, equal and opposed to the uniformly increasing tensile *resistances* from E to B and the compressive *resistances* from E to D, E lying in the center of gravity of the cross section.

2. In Fig. 2 A lies anywhere, in the medial plane, *within* the cross section (a further limit will presently be indicated); in Fig. 1, A lies in the center of gravity of the cross section, so that +R passes through E.

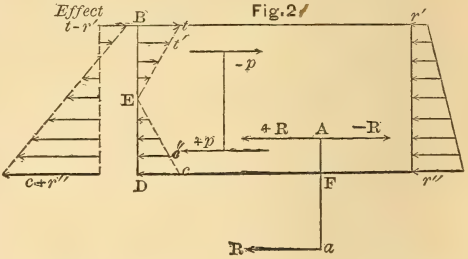


Fig. 2

The force +R at A, Fig. 1, is decomposed into forces, $r, r \ldots .$, *supposed* uniformly distributed. In Fig. 2, +R at A is *supposed* decomposed into $r', \ldots ., r''$, straight lines limiting the arrows representing the forces.

3. The hypothesis that in an elastic beam the forces $r' \ldots r''$, Fig. 2, are proportional to the ordinates of a trapezoid

will be proved at art. 18. This has long been a favorite hypothesis, but I have not met with a demonstration anywhere, simple as it is.

4. In both Figs. 1 and 2, the sum of the moments of the forces $r\ldots$ about A must be zero in order that R may be their resultant.

5. Now the forces $r, \ldots,$ add to some of the forces, $t', t', \ldots, c', c$, and subtract from others, thus leaving as the actual *forces*, opposed by equal tensile or compressive resistances at the cross section, $t-r, t'-r, \ldots, c'+r, c+r$, Fig. 1, or, $t-r', \ldots. c+r''$, Fig. 2, as shown to the left of the figures.

6. We shall now show that the resulting actual forces $t-r\ldots.$ by either mode of decomposition, Figs. 1 or 2 are the same.

For brevity put $BD=h$, $AF=y$ (Fig. 2) or $F=d$; then by the theory of the center of gravity (see Weisbach's Mechanics, Coxe, art. 110), since $+R$ acts at the center of gravity of the trapezoid $r'\ldots r''$ Fig. 2, and can be represented by that trapezoid, h being its height,

$$y=\frac{r''+2r'}{r''+r'}\,\frac{h}{3},\ \text{and}\ R=\frac{r'+r''}{2}h$$

hence, by elimination,

$$r'=\frac{2R}{h}\left(\frac{3y}{h}-1\right)\ldots(1);\ r''=\frac{R}{h^2}(4h-6y)\quad(2)$$

Now calling f the strain t or c, we know from the theory of flexure, that in Fig. 1.

Moment of R at a about $A=R(d+\frac{1}{2}h)=\frac{1}{6}fh^2$, whence

$$f=\frac{6R(d+\frac{1}{2}h)}{h^2}=t=c$$

Uniform compression $r=\frac{R}{h}$,

Hence,

$$(t-r)=\frac{R}{h}\left(\frac{6(d+\frac{1}{2}h)}{h}-1\right)=\frac{R}{h^2}(6d+2h)\quad(3)$$

$$(c+r)=\frac{R}{h}\left(\frac{6(d+\frac{1}{2}h)}{h}+1\right)=\frac{R}{h^2}(6d+4h)\quad(4)$$

In Fig. 2,

$$\text{the moment } R\overline{R}=R(y+d)=\tfrac{1}{6}fh^2$$

$$\therefore f=\frac{6R(y+d)}{h^2}=t=c$$

Therefore, in Fig. 2,

$$(t-r'')=\frac{R}{h}\left(\frac{6(d+y)}{h}-\frac{6y-2h}{h}\right)$$

$$=\frac{R}{h^2}(6d+2h)$$

$$(c+r'')=\frac{R}{h^2}(6y+6d+4h-6y)$$

$$=\frac{R}{h^2}(6d+4h)$$

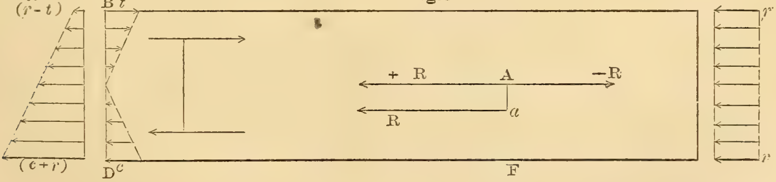
These values for the extreme forces being the same as for the corresponding forces $(t-r)$, $(c+r)$, eqs. (3) and (4), we conclude that either mode of decomposition leads to the same result.

7. From eq. (6), $r'=o$ when $3y=h$, hence if forces $r'\ldots r''$ are all to be compressive, A must not be supposed nearer edge than $\frac{h}{3}$.

8. If R at a is sufficiently near A, $t-r, t'-r, \ldots.$ may all act as compressive forces, thus not requiring any tensile resistances at the cross section BD as at a joint of the voussoir arch.

The effect is the same, as previously shown, if R at a is decomposed at once into the compressive forces $r'\ldots. r''$ of Fig. 2, equivalent to, $r-t, \ldots., c+r$, of Fig. 3, according to the ordinates of a trapezoid.

Fig. 3

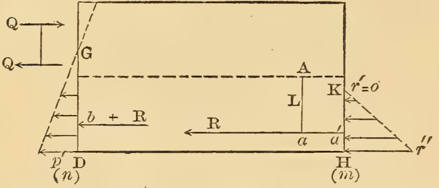


9. From eq. (3), $t-r=o$ when $d=\frac{1}{3}h$ or when $aA=\frac{1}{6}h$. When a is farther from A than $\frac{1}{6}h$, then $(t-r)$ is positive and we assume tensile *resistances* at B to oppose the forces.

But suppose the joint BD can offer no

tensile resistances, then it is wrong to decompose R as before, if by another method of decomposition stability is assumed. Many writers seemed to have overlooked the following method of decomposition entirely:

Fig.4



R at *a*, Fig. 4, is the resultant in position on the joint BD. We may decompose R according to the ordinates of a triangle as at (*m*), if we assume the law of the trapezoid as before, r' is now *o*, hence $y=\frac{k}{3}$, where $k=\text{KH}=$ height of trapezoid or triangle.

Placing

$$\overline{\text{A}a}=\text{L},\ \text{HK}=\text{K}=3\,\overline{\text{H}a'}=3(\tfrac{1}{2}h-\text{L})\ .\ (5)$$

10. Let us now compare this result with the case shown at (*n*), where the beam is supposed to supply tensile resistances along BG. Call the resistances per square unit at B and D, p and p' respectively; *i. e.*, $p=t+r$ and $p'=c+r$.

From similar triangles, calling $\text{DG}=x$,

$$p^1 : x : : p : h-x\ .\cdot.\ x=\frac{p'\,h}{p+p'}$$

Now substituting for p and p' their values, eqs. (3) and (4) and replacing $(d+\frac{1}{2}h)$ by L, we have,

$$x=\text{DG}=\frac{\frac{\text{R}}{h}\left(\frac{6\text{L}}{h}+1\right)h}{\frac{\text{R}}{h}\quad\frac{6\text{L}}{h}}\ \frac{h^2}{12\text{L}}+\frac{h}{2}\ .\ (6)$$

Now from eqs. (5) and (6)

For $\text{L}=\frac{1}{6}h$, $\text{HK}=\ h$, $\text{DG}=\ h$,

$\frac{1}{3}h$, $\frac{1}{2}h$, $\frac{3}{4}h$,

$\frac{5}{12}h$, $\frac{1}{4}h$, $\frac{7}{10}h$,

$\frac{1}{2}h$, 0, $\frac{2}{3}h$,

That is, HK<DG, when L lies between $\frac{1}{6}h$ to $\frac{1}{2}h$; or the point of no strain in the beam lies nearer the edge DH where the joint can oppose no tensile resistances than when we suppose them exerted.

11. Let us now compare the strains p' and r''.

In eq. (2), when KH=K<BD, then we must write K=height of trapezoid, for *h*; we also have $\text{K}=3\overline{\text{H}a'}=3y$, Fig. 4. Again, moment of R at *a* about $\text{A}=\text{R}(\frac{1}{2}h-y)$, &c. See art. 6. Hence eqs. (2) and (4),

$$r''=\frac{\text{R}}{9y^2}(12y-6y)=\tfrac{2}{3}\frac{\text{R}}{y}\quad .\ \ .\ \ (7)$$

$$p'=(c+r)=\frac{\text{R}}{h^2}\,(4h-6y)\ .\ \ .\ \ (8)$$

Thus for any values of y between 0 and $\frac{1}{3}h$ (see table) we find that, $r''>p'$, always.

y	r''	p'
$\frac{1}{12}h$	8	$3\frac{1}{2}$
$\frac{1}{6}h$	4	3
$\frac{1}{4}h$	$\frac{8}{3}$	$\frac{5}{2}$
$\frac{1}{3}h$	2	2
	$\frac{\text{R}}{h}$	$\frac{\text{R}}{h}$

There is therefore greater compression at edge DH when the real forces are as at (*m*), than when the beam can oppose tensile resistances as at (*n*). Also since KH<DG, within the same limits ($y=o$ and $\frac{1}{3}h$), the compression is more uniformly distributed at (*n*) than at (*m*); hence the beam will bend more when, as in the voussoir arch, a joint BD can only oppose compressive resistances, than when, as in a solid beam, tensile resistances can be exerted. As a consequence of this compression at lower edge, there being none (according to our hypothesis—the law of the trapezoid) at K in case (*m*), the joint above K must open, and could in fact be removed without interfering with the distribution of the forces at all. While it is not easy of comprehension, that on an incompressible joint every part should not bear some part of R, yet on real compressible joints it would be hard to conceive of the joint *not* opening above some point K. Experiment sustains this view. Some writers have erroneously asserted that the opening was due to tensile forces. It may be observed, that if, as in the voussoir arch, these tensile forces are unbalanced rupture must ensue.

12. *The decomposition of* R *cannot be the same for an open joint as for a solid*

beam, when L *lies between* $\frac{1}{6}h$ *and* $\frac{1}{2}h$, for if we assume for a solid beam the disposition (*m*), the edge nearest *R* is most compressed, the joint would tend to open above K; but as there are tensile resistances there that prevent it, the disposition (*m*) is not correct for the solid beam, hence some other disposition as (*n*) is correct.

13. In experiments by the writer with wooden voussoir arches—gothic being 14 *in span*, 12.12 *in rise* and weighing 4.16 lbs., the resultants at the joint of rupture approached within $\frac{1}{18}h=\frac{1}{18}$ depth joint from edge without rupture ensuing (Cain's Voussoir Arches, p. 85). They likewise indicate that the nearest approach of "*a*" (Fig. 4), to the edge consistent with stability, called its "limiting position," for a plane joint probably depends upon the magnitude of R (as we should infer, r'' increasing with R, whilst KH is constantly $3.\overline{Ha'}$), and the special compressibility of the material. Of course "*a*" cannot be upon the very edge, since R must act upon a finite surface.

In the experiments, the joints generally opened on the side farthest from the resultant, before the final weight was applied, and remained open, bearing at the edges only, thus slightly distorting the arch. As in the investigations of stability, the arch was assumed non-deformable, the resultant passed nearer the edges than estimated.

14. The spandrels of a stone bridge, by exerting horizontal forces at the haunches, may keep the joints entirely or partially closed. The disposition of forces then may, in a bridge with solidly built spandrels approximate to (*n*), Fig. 4.

15. In the disposition (*n*), Fig. 4, R is transported from *a* to *b* nearer G, thus R at *b* and couple Q Q (shaded forces) being just equivalent to R at *a*. In fact, conceive R at *b* and couple $\overline{QQ}$. Now at *a* conceive +R and −R applied. Now R at *b* with −R at *a* form a couple which must just equal $\overline{QQ}$ leaving +R at *a* as the resultant of R at *b* and $\overline{QQ}$. Also, since $\overline{QQ}$ is right handed, $\overline{RR}$ must be left handed, hence *b* is nearer G than *a*.

The total strain of compression on DG is greater than on HK at (*m*), but since $p' < r''$ and KH<GD, the strain is more equalized on DG than on KH, and (*n*) is the most advantageous disposition of forces.

16. Some writers assert that if A*a* is greater than $\frac{1}{6}h$ and tensile resistances are not supplied at B, the arch must fall. The experiments mentioned have proved finally that if R lies between $\frac{1}{6}h$ from center and its "limiting position," very near edge, that rupture does not ensue; hence there are no tensile forces exerted, hence R is simply decomposed into compressive forces. It may be remarked that the resultant will at the joints of rupture in an arch reach its limiting position, whether the theorist wishes it or not. The *true line* of pressures most likely never keeps within the "middle third" in any actual arch—certainly not for the smaller spans with their joints.

For any other continuous symmetrical form of cross section than the rectangular, we can by parallel planes divide it into strips that are approximately rectangles. As the error of so considering them diminishes indefinitely with the width of the strips, we can assume the previous conclusions for any continuous symmetrical form of cross section, since the error can be made as small as we please, becoming 0 at the limit.

18. As to the assumed law of the trapezoid, we may state generally *for a rectangular form of cross-section, that if* R *is on one side of the neutral axis, the beam will suffer greater compression on that side, hence bending will occur.*

To prove it, we premise that when bending occurs the extensions or compressions of the fibers, and hence the forces *per square unit* acting on them, must necessarily be directly as their distance from the neutral axis. [G in Fig. 4 (*n*), or K in Fig. 4 (*m*)].

Where tensile resistances are exerted at one side and compressive at the other as in Figs. 1, 2, or 4 (*n*) flexure or *bending* must occur.

Now consider the resultant R, in the cross section as at Figs. 3 and 4 (*m*). There are three suppositions:

1*st*. In Fig. 4 (*m*) call $r' \ldots . r''$, the compressive forces *per square unit* and R their resultant. Suppose an *equal shortening of the fibers* due to the force R at a' on $\overline{Ha'}$ and $\overline{a'K}$. This requires a uniform distribution of the component of R ($r' \dots r''$) on KH, whence Ka' $=$Ha'.

But as the fibers give equally on KH, KI must also suffer an equal shortening of the fibers, but R at a' cannot be the resultant of *uniform* forces distributed over HI, hence the first assumption is incorrect.

2*d.* Suppose *the actual shortening of the fibers on* $\overline{Ka'}$ *greater than on* $\overline{Ha'}$.

This can *only* happen by supposing the beam *more compressed at* I *than at* H; thus bending occurs and the forces $r' \dots r''$ must *regularly* decrease from I. But in that case R cannot be their resultant when a' is below the neutral axis. Our 2d. surpposition is thus false.

3*d. and last. Suppose the actual shortening of the fibers greater on* $\overline{Ha'}$. As before, bending occurs and the forces $r' \dots r''$, consequently, regularly increase from K to H according to the law of the trapezoid. In this case R can be and is their resultant. There is no inconsistency here with the supposition.

The first two cases being untrue, the last, the only remaining one, is true.

We have thus proved that bending always occurs when R is not in the center of gravity of the section; and that in a rectangular cross-section, that can oppose no tensile resistances, the resultant R is decomposed according to the ordinates of a trapezoid, whence follows the conclusions of previous articles.

19. The reasoning and conclusions of the last article are easily extended to continuous forms of cross section, symmetrical about the neutral axis, as in art. 17.

20. For any form of cross-section, if we admit that the beam *bends* when R is not in the neutral axis, the extensions and compressions must be as their distance from the neutral axis, *i.e.*, the tensile or compressive force per square unit on a fibre is directly as the distance from the point of no strain.

21. The writer has thought proper to endeavor to put in a clear and simple manner the foregoing, both to meet enquiries as well as misapprehensions. In VAN NOSTRAND'S MAGAZINE for Sept., 1877, p. 257, is an article "On the Strength of Columns," by E. Hatzel, Translated, &c. The following articles on the same subject embody the same steps followed by Mr. Hatzel, with two important exceptions that will be noted further on, thus reaching a formula slightly different from his.

22. STRENGTH OF LONG POSTS.

Let ABCD represent the neutral axis of a pillar bent by the force P at A acting in the direction AD.

Call the abscissa AF$=x$, ordinate BF$=y$CE$=c$,ACD$=$AED$=l,\delta=$ radius of curvature at B, A$=$ cross-section at B when $y=$BF.

I$=$moment of inertia of cross-section about neutral axis,

E$=$modulus of elasticity,

$f=$maximum resistance per square unit of A.

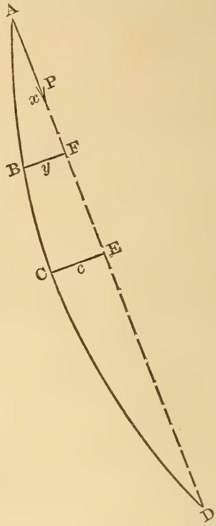


Fig. 5

Let us suppose, as in Fig. 1, two opposed forces at B, each equal and parallel to P; then, as in Fig. 1, the compression per square unit due to $+$P is,

$$r=\frac{\mathrm{P}}{\mathrm{A}} \quad . \quad . \quad . \quad . \quad (1)$$

Now the moment Py can only be resisted by a moment (this seems to have been overlooked by Mr. Hatzel as he takes the moment of inertia about the edge of the cross-section, which gives no resisting *couple* at all): hence calling (Fig. 1) $t=c=f_1=$max. strain per square unit, at a distance from the neutral axis $=g$, we have from the well known theory of flexure,

$$M=Py=\frac{f'I}{g} \quad . \quad . \quad . \quad (2)$$

[It may be observed that from (1) and (2) we may find $r+f^1=c+r$ and $f^1-r=t-r$ of Fig. 1 for any cross-section, M being the moment $R(d+\frac{1}{2}h)$].

From (1) and (2), we have for the stretching per unit of length

$$\frac{r}{e}=\frac{P}{Ae}, \quad \frac{f'}{e}=\frac{g}{EI}\,Py$$

The sum of which represents the greatest admissible shortening, $\frac{f}{E}$, if P represents the crippling weight, and we put $y=c$ (its max. value

$$\therefore \frac{f}{e}=\frac{P}{AE}+\frac{g}{IE}\,Pc$$

$$\therefore fA=P\left(1+\frac{gA}{I}c\right). \quad . \quad (3)$$

The equation of the elastic line ACD (see Weisbach, art. 265), is

$$y=c\ \sin.\left(x\sqrt{\frac{P}{EI}}\right)$$

When $x=\frac{1}{2}l$, $y=c$

$$\therefore 1=\sin.\left(\tfrac{1}{2}l\sqrt{\frac{P}{EI}}\right) \quad \therefore \tfrac{1}{2}\pi=\tfrac{1}{2}l\sqrt{\frac{P}{EI}}$$

$$\therefore \frac{EI}{P}=\frac{l^2}{\pi^2}$$

Now at c,

$$\delta=\frac{EI}{Pc}=\frac{l^2}{\pi^2c} \quad . \quad . \quad . \quad (4)$$

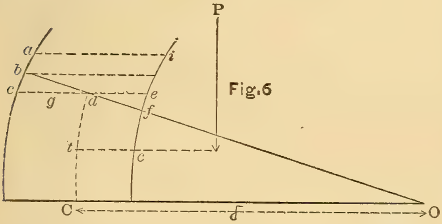


Fig.6

In the accompanying figure $OC=\delta$, $Cd=1$, $dc=g$. The cross section gi parallel to CO is moved to bf. bc=extension of outermost fibre due to moment $Py=k$. Or if compression $ef>bc$ due to same moment, $k=ef$ and $de=g$.

The uniform compression due to P is $ac=ie$. When $ac<bc$ there is no tensile strain.

In any case we have the proportion,

$$\delta : 1 :: g : k \quad \therefore \delta=\frac{g}{k}$$

This gives in eq. (4)

$$\frac{g}{k}=\frac{l^2}{\pi^2c} \quad \therefore c=\frac{kl^2}{g\pi^2}$$

which, substituted in (3), gives

$$P=\frac{fA}{1+\frac{kl^2}{\pi^2}\frac{A}{I}}$$

But $\frac{A}{I}=\frac{1}{r^2}$, where r=radius of gyration of the cross-section about the neutral axis. Also for brevity write, $\frac{k}{\pi^2}=a$, and we have the usual formula,

$$P=\frac{fA}{1+a\left(\frac{l}{r}\right)^2} \quad . \quad . \quad . \quad (5)$$

[Remark. k has been assumed by Mr. Hatzel as identical with "λ=greatest possible shortening which the material can sustain per unit of length," whereas $f\ i$ in last figure$=\lambda$].

23. In this formula, (5), $a=\frac{k}{\pi^2}$ would be known if k were known, but as k depends upon the unknown moment, Pc (c being unknown) it is obviously impossible to determine it except by experiment. "a", by theory, is the same for any cross section. It seems reasonable to suppose that a increases with l, as the bending (which determines $a=\frac{k}{\pi^2}$) is probably greater the longer the pillar.

A comparison of numerous experiments does not support this view.

"a" is usually assumed constant.

In the experiments on pillars made by the Cincinnati Southern Railway, "a" was assumed constant and "f" computed. It was found that for the same shapes and iron f as computed does not vary more than the tensile strengths of iron of the same manufacture. For any special shape it is recommended (see Mr. T. D. Lovett's report to C. S. R. 1875) to find f by experiment, when the above formula is applicable.

"For swelled columns the formulæ are are also applicable, provided the diameter at the end be used." "Riveting at the ends and tail bearings are of great importance."

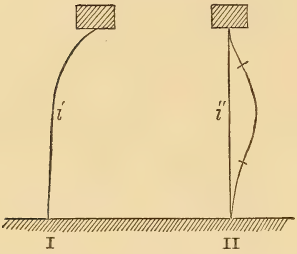
24. In Fig. 6*a* (I), *for pillar fixed at one end and round at the other*, formula (5) applies if we replace l by $2l'$,

$$\therefore P=\frac{fA}{1+4a\left(\frac{l'}{r}\right)^2} \quad . \quad . \quad (6)$$

25. In Fig. 6*a* (II) *column fixed or flat at both ends*, the curvature of Fig. 7 (I) is repeated four times. For l' then in (6) put $\left(\frac{l''}{4}\right)$

$$\therefore P=\frac{fA}{1+\frac{a}{4}\left(\frac{l''}{r}\right)^2} \quad . \quad . \quad (7)$$

Fig. 6a



26. Dr. Rankine says that Hodgkinson verified the relations expressed by eqs. (5) and (7), but "found the strength of a pillar fixed at one end and rounded at the other, to be a mean between the strength of two pillars of the same length and diameter, one fixed at both ends, and the other rounded at both ends."

The constant $\left(\frac{a}{4}\right)$ in eq. (7) is given by Rankine and others at $\frac{1}{36000}$; and for columns *hinged on pins at the ends*, it is recommended by American engineers that we put $\left(\frac{a}{4}\right)$, in eq. $7=\frac{2}{36000}$, f being determined by experiments on hinged joints.

27. It may be observed that when, for rounded ends $a\left(\frac{l}{r}\right)^2=1$, (the value of K corresponding to P being substituted) that some part of the cross-section is in tension, see eqs. (1), (2) and (3). This should be avoided for cast iron. If the pillar cannot exert tensile forces, it will bend, but will not break in two until P is at its "limiting position," very near the edge (see arts. 9 to 14). Compare the magazine article quoted, p. 261 "5th."

28. It is advisable in stone or brick pillars from a practical (not theoretical) point of view, that $a\frac{l^2}{r^2}<1$. For when $a\frac{l^2}{r^2}>1$, if the mortar is cohesive, tensile resistances will be exerted until the cohesion is destroyed, when the joint opens and there being only compressive resistances, the pillar bends more and of course P approaches its limiting position."

It is certainly improper from a practical point of view, to have the joints of a vertical pillar opening. In a voussoir arch you cannot prevent the resultant on the joints of rupture passing as near the edge as its limiting position, though the spandrels may prevent opening of the joints. It is reported that in many bridges the voussoirs bear on the edges only.

29. Mr. Hatzel found from some experiments on brick piers that when $l=12$ diameters, tensile forces might be feared.

30. It is usual to take a certain proportion of the crippling weight for the safe load a pillar can bear. It has been recommended by a Committee of the American Society of Civil Engineers, appointed to propose "Means for Averting Bridge Accidents," (see Van Nostrand's Magazine, Oct. 1875, p. 306), not to exceed for wrought iron in compression the following strains per. square inch in lbs. for square or cylindrical sections,

Diameters.	Pounds per Square Inch.	
	Square ends.	Round Ends.
10	10,000	7,000
10 to 15	9,000	6,500
15 to 20	8,000	6,000
20 to 25	7,500	5,500
25 to 30	6,800	5,000
30 to 35	6,000	4,000
35 to 40	5,000	3,500
40 to 50	3.800	2,500
50 to 60	3,000	2,000

On the Committee were some of the first engineers in this country.

After the Ashtabula accident, the same

limit of strain was proposed to the Ohio Legislature.

31. In this table the safe strains allowed diminish more rapidly with the height than the formula warrants. Hence it has been proposed to tack on a variable factor of safety. Thus, if $d=$ diameter of column in the direction of the bending we may write for *flat ends*, the safe strain per sq. in.

$$P=\frac{1}{4+\frac{\frac{l''}{d}-15}{10}}\cdot\frac{fA}{1+\frac{a}{4}\left(\frac{l''}{r}\right)^2}\quad . \quad (8)$$

If we put $f=38500$, $A=1$ and $\left(\frac{a}{4}\right)=\frac{1}{36000}$, we find for $\frac{l''}{d}=10$, $P=10761$; $\frac{l''}{d}=27$, $P=6371$; $\frac{l''}{d}=45$, $P=3793$, which agrees tolerably well with the table.

The above recommendations are to be amended from time to time as experience may suggest.

32. In these formulæ as in most specifications, the safe strain allowed for tension or compression is usually some constant fraction of the breaking weight, irrespective of the extremes of load to which the number may be subjected.

Since the experiments of Wohler and Spangenberger and the publication of the treatise of Weyrauch ("Constructions of Iron and Steel") it may pertinently be asked, even if we cannot accept Weyrauch's formulae for *bridge members* whether our own empirical formulae cannot be very much improved upon in the light of the experiments mentioned.

Wohler's law is a fixed fact and should not be ignored.

The suggestions thrown out on this subject are of course open to much criticism, as no experiments can be appealed to for numerical values, and they can only be defended as plausible and systematic.

In order that bridge engineers may compare with their own practice, I have given several illustrations after the assumed formulae.

SAFE STRAINS FOR THE DIFFERENT MEMBERS OF A BRIDGE.

33. Weyrauch (see "Constructions of Iron and Steel," Chap. XIII, or this Magazine for June 1877, p. 519) deduces from Wohler's experiments, in connection with Launhardt's formula, the following value for the safe strain in kilograms per square centimeter$=b$, to which wrought iron should be subjected in tension,

$$b=\frac{2100}{n}(1+\tfrac{1}{2}\theta)\quad . \ . \ . \quad (9)$$

where $n=$factor of safety, $\theta=\frac{\text{min. B}}{\text{max. B}}=$

$$\frac{\text{Minimum strain that piece ever bears}}{\text{Maximum strain that piece ever bears}}$$

In the experiments, *impact*, such as a train of cars passing over a bridge causes, was not considered, hence its effect is not included in the above formula. There are no experiments to determine its effect, hence if we estimate it at all, it must be empirically. It is of course most convenient to embrace it in the formula in place of considering it separately.

34. Now it is reasonable to suppose that the effect of impact diminishes as the weight of the member increases. The weight of the web members of a bridge increases pretty regularly from the center to the abutments in a framed truss. θ increases from the center outwards, most rapidly at first, as an example will show. If now we assume the effect of impact to vary directly as θ, this hypothesis may be as near the truth as the first one. The unknown term now is the coefficient of θ, which call 1, and write for *the safe strain on ties in lb. per square inch.*

$$b=7500\,(1+\theta)\quad . \ . \ . \quad (10)$$

Also write empirically for *the safe strain on wrought iron columns in lbs. per square inch,*

$$b=\frac{1}{4+\frac{1}{10}\left(\frac{l}{d}\right)}\ \frac{38500}{1+c\left(\frac{l}{r}\right)^2}(1+\theta)\quad . \quad (11)$$

where $c=\frac{1}{36000}$ for pillars *flat at both ends* and $c=\frac{2}{36000}$ for *both ends hinged*, and $c=\frac{1}{24000}$ for *one flat and one hinged end.* As before $l=$length of pillar, $d=$diameter in the direction of bending, $r=$radius of gyration of cross section about neutral axis; f has been taken here as a mean, 38500. It should be determined by experiments for any particular form of cross section.

35. These formulæ are empirical, and can only be compared with the practice of leading engineers, there being no experiments to check them by. The experiments given by Stoney (Strains, &c., arts. 472, 475) rather give a color to, than disprove the formulæ. They certainly embrace the deduction of Launhardt that b varies as θ, and are supposed to include impact by diminishing b more than Weyrauch's formula towards the center of the truss.

The applications will now enable the engineer to see if the formulæ are any improvement over usual rules of thumb, and hence whether they may be adopted in whole or in part, or neglected altogether.

36. Assume that the chord strains are to be determined by supposing the load on bridge (say *two locomotives and tenders weighiny* 2,800 *lbs. per foot, followed by cars weighing* 2,240 *lbs. per foot*), uniformly distributed and that the weights of bridges of 100, 200, 300, and 400 feet spans are 900, 1,500, 2,400 and 4,000 lbs. per. foot. We have, eq. (10),

For	lbs.
100 ft. span, $b=7500\ (1+\frac{900}{3700})=$	9,325
200 ft. span, $b=7500\ (1+\frac{1500}{4000})=$	10,300
300 ft. span, $b=7500\ (1+\frac{2400}{4800})=$	11,250
400 ft. span, $b=7500\ (1+\frac{4000}{6400})=$	12,190

For bridges of small weights $b=7500$ nearly. At the middle of all the bridges $\theta=0$ for ties and counter ties and $b=7500$; hence b varies from 7500 at center of bridges to 12190 for chords for a 400 feet span, or to 10300 for a 200 ft. span, &c. The excessive vibration of very small spans may have to be allowed for separately.

In formula (11) for posts, for $\left(\frac{l}{d}\right)=10$ and cylindrical columns, we get very nearly the same values as above, which appears at least reasonable, if not proper.

37. In the computation of $\theta=\frac{\text{min. B}}{\text{max. B}}$ for the web members, using the method of apex loads, we observe that a web tie is strained *most* when the load (engines in front) extends from farthest abutment to foot of tie, *least* when it extends from nearest abutment to top of tie. Thus, in an ordinary Pratt Truss 200′ span, 16 panels and weighing 272 lbs., we find from a comparison of shearing forces from eq. (10)

1st panel from end, $b=7500\times1{,}337$ $=10{,}027$

2d panel from end, $b=7500\times1{,}327$ $=9{,}942$

3d panel from end, $b=7500\times1{,}304$ $=9{,}780$

4th panel from end, $b=7500\times1{,}261$ $=9{,}458$

5th panel from end, $b=7500\times1{,}186$ $=8{,}895$

6th panel from end, $b=7500\times1{,}060$ $=7{,}950$

7th and 8th panel from end, $b=7500$.

For the counters we, of course, have $b=7500$. Nearly the same figures apply to posts when $\frac{l}{d}=10$.

38. It will be observed that for 200′ span we have $b=10000$ lbs. about, for chords and end ties. The above values of b for end ties and middle ties will give about the same dimensions as the rule "add 50 per cent. to live load for counters and middle bars," which is in use by some engineers.

Observe that for small spans, 10 to 30 feet, say, as θ is small, $b=7500$ nearly for all bridge members. (It may be best to take it less for such small spans). By the 50 per cent. rule, whilst the chord and end members are supposed loaded with the actual load, the middle ties are supposed loaded to nearly 50 per cent. over actual load. That b is nearly constant for small spans seems the more reasonable conclusion.

In the Louisville bridge 400 feet span, b for tension, was varied from 7000 to 12500 nearly, as by form (10).

In Europe, Gerber *used* nearly the same values as we derive from eq., (10), in his "Mainz Bridge" (see Weyrauch, Chap., XXX.)

If we write $b=646(1+\theta)$ kils. per sq. cent., the results agree very closely with Gerber's last formulae except for $\theta=\frac{2}{3}$ to 1 say.

In formula (10) above when $\theta=1$, we suppose impact null, $b=15000$ lbs., per square inch. It seems that this is sufficiently great—by Gerber's formulae it would be much greater.

39. Formula (10) then agrees somewhat with the practice of engineers, though b should diminish in each panel towards the center according to Wohler's law, which is established beyond question, and this is often neglected. It seems that the same reasoning should apply to posts.

40. Let us now compare eq., (11) with the table art. Thus for end braces of a 200 feet span, $\theta=\frac{1}{3}$, for hollow cylindrical posts $r=\frac{d}{4}\sqrt{2}$, we have,

$$b=\frac{1}{4+\frac{1}{10}\frac{l}{d}}\;\frac{385000\times1\frac{1}{3}}{1+\frac{2}{9000}\frac{l^2}{d^2}}.$$

$$\because\ \frac{l}{d}=10,\ b=10{,}050$$
$$20,\ b=7860$$
$$30,\ b=6110$$
$$40,\ b=4730$$
$$50,\ b=3666$$

These figures agree very well with those recommended by the Committee of the Am. Soc. Engs. Formula (11) now causes h to diminish towards the center of the span, which *should be*, according to Wohler's law.

The above formulae are of course to be amended from time to time as experience may suggest. It is hardly probable that experiments can ever estimate the exact influence of impact as it occurs on bridges of all spans.

May we not as well estimate for it empirically now as to wait years to begin our guessing?

41. If it is preferred we may use larger values than 7500 in eq., (10) or 38500 in eq., (11), thus giving greater values for b than 1000 pounds per square inch for the chords of a 200 feet span.

It is the decrease of b for web members towards the center that should be the object of formulae, and that is effected by eqs., (10) and (11) in a simple manner. It is true as has been suggested by a friend that this is building faster than we have foundations laid, but as the bridge builder must estimate b by some rule of thumb, may not the above rules be defended on the plea of necessity, as well as other rules?

It must be distinctly understood that this is all that is claimed.

If a constant factor of safety is used in eq., (11), [the variable factor, $\left\{\frac{1}{4+\frac{1}{10}\frac{l}{d}}\right\}$ is only used out of abundant caution], it becomes simpler. This is a matter for practical bridge builders to decide.

COMPARISON OF TWO BRIDGE TRUSSES.

42. Two trusses are represented in Figs. 7 and 8 (the latter being a design of my own, though I am informed that a similar truss, only with vertical interior posts, has been used on the Pa. R.R.) The dotted lines represent suspenders to hold up the roadway.

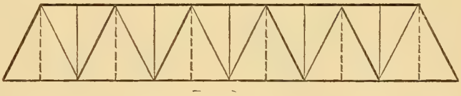


Fig. 7.

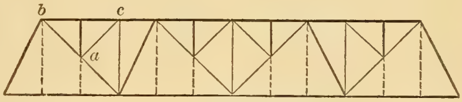


Fig. 8

The Fink elements *abc* (Fig. 8), support the upper chord, thus giving the same chord lengths as in Fig. 7. Weight of bridge was assumed at 272,000 lbs., span 200 feet, 12 panels of $16\frac{2}{3}$ feet each, height, Fig. 7=28 feet. Live load for *web* 2,800 lbs. per. foot on 100 feet, followed by 2,240 lbs. per foot for rest of span. For chords (although this does not give maximum strains), uniform live load of (280000+224000)÷200 per foot. The load $\frac{1}{2}$ panel either side of an apex, supposed concentrated at that apex.

43. We have then for chords, $\left(\frac{l}{d}\right)=10$, since $\theta=\frac{272}{776}=.35$, $b=7500\ (1+.35)=$ 10130 for tension and nearly the same for compression.

From the shearing forces (which are a maximum when load extends from farthest abutment, a minimum for the same) that can be found by formula, or by tabulating for separate weights, we get,

Panel.	Shearing Forces.		θ
	Max. B.	Min. B.	
1	184,700	62,330	.333
2	153,960	49,040	.320
3	124,750	33,800	.272
4	97,100	16,600	.17
5	71,000	− 2,540	0
6	46,450	−23,650	0

From which we find b from eqs. (10) and (11) for ties and braces, b thus varies for ties from 7500, suspenders and middle ties, to about 10000 for end ties and chords. For Fink tie $\theta=\frac{9}{43}$ about $\therefore b=9100$. For vertical post θ was taken $=0$. The least section of post taken three square inch for Fink post and 4.5 square inch for vertical post of Fig. 7. The end upper chord panels regarded as "fixed at one end and hinged at the other," the other panel lengths as fixed at both ends; the chord and posts of wrought iron hollow cylindrical; diameter assumed for upper chord and end braces, $13\frac{1}{3}$ inches. We thus find the weights in lbs. (on the computed strains only) on $\frac{1}{2}$ of one truss.

	Fig. 7.	Fig. 8.
Upper Chord......	8,200	9,500
Braces and Posts...	14,000	10,500
Lower Chord......	7,700	6,800
Ties...............	5,800	7,300
	35,700	34,100

44. Thus giving a saving of 4×1600 lbs. of wrought iron on 2 trusses. If the same thickness of metal is taken for upper chord and braces (not considering the extra cost of mouldings, pins, &c.) the saving is found to be 13,000 lbs. The true saving lies then between 6400 and 13000 lbs.

If the engines and cars are distributed so as to give the maximum strain on chords, the saving is slightly less.

45. If we take the height of the truss, Fig. 8, as 31 feet, the diameters of compression members being the same as for Fig. 7, we find the saving on 2 trusses, 8,000 lbs.

46. In this Magazine for January, 1877, is an article by Emil Adler, C. E., in which a very general discussion of the "Most Economical Depth of Straight Girders and Trusses" is given.

Extending his method by including form (11) for posts, various inclinations of ties and braces, &c. I find from the formula deduced, about the most economical depth for the simple triangular of 12 panels (Fig. 7) to be 28 feet, for the truss, Fig. 8, about 31 feet. For a greater number of panels the depth is less.

The simple triangular, being one of our cheapest forms—theoretical—the saving in stress, Fig. 8, may be of interest to bridge engineers. The interior posts of Fig. 8 may be vertical, as square joints possess a decided mechanical advantage over hinged joints, being besides more economical.

47. In Fig. 8 the long tie at middle, that becomes at times a counter, may be rested on the bottom of the Fink post, at its center, thus saving in the weight of this latticed brace.

48. The different inclinations of ties and braces was suggested by the following demonstration:

Let P=shearing force on tie BC and brace AC, the panel length being AB=l. Put BC=l_2.

AC=l_1, AD=x, DC=h.

W=weight of 1 cu. in. of tie in lbs.

W′=weight of 1 cu. in. of post in lbs.

C=cost per lb. of tie in cents.

C′=cost per lb. of post in cents.

T=safe strain per sq. in. for tie.

S=safe strain per sq. in. for post.

We have

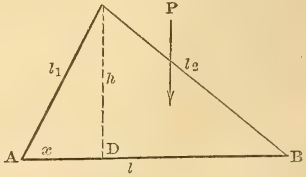


Fig. 9

Strain on CB=$P\frac{l_2}{h}$; Cost CB=$\frac{Pl_2}{hT}12l_2wc$.

Strain on CA=$P\frac{l_1}{h}$; Cost CA=$\frac{Pl_1}{hS}12l_1w'c'$

Total cost of tie CB and post CA=B

$$y=\frac{12P}{h}\left(\frac{l_2^2}{T}wc+\frac{l_1^2}{S}w'c'\right)$$

$$=\frac{12P}{h}\left(\frac{h^2+(l-x)^2}{T}wc+\frac{h^2+x^2}{S}w'c'\right)$$

Now if we desire this to be a minimum, T and S remaining constant and x varying, we put its first differential coefficient$=0$.

$$\therefore \frac{-2lwc}{T}+\frac{2xwc}{T}+\frac{2xw'c'}{S}=0.$$

$$\therefore x=\frac{Swcl}{Swc+Tw'c'}$$

Thus, for $wc=w'c'$,

$$T=11000,\ S=5000,\ x=.31,$$
$$S=6000,\ x=.35,$$
&c., &c.

We see that for $wc=w'c'$ as long as S and T are different, the ties and braces should have different inclinations, and for proportions above $x=\frac{1}{3}$ about.

49. In a Pratt Truss of 200 span if ties and braces are so inclined in them that $x=\frac{1}{3}$ there is a saving in them alone of a few thousand pounds. The weight of the counters is of course increased, owing to their increased length.

The economy of the square joint may, however eliminate all saving. If we suppose the post AC of wood, we shall find x to be very nearly equal to l, hence in the Howe bridge the braces should be of wood. The Howe form is generally applied to wooden bridges. For the post AC of cast iron, $x=\frac{1}{2}$ about.

50. If a weight is supposed at top of post, its effect is to require that the post be more nearly vertical. Its influence is easily included in the formula.

51. The inclination of the Fink ties, $\overline{ab}$, $\overline{ac}$ of Fig. 8, has been taken the same as that of the main ties. This is best for appearance sake.

To find the inclination that secures the greatest economy in the ties, post and chord of a Fink element (neglecting the bracing between the two trusses of a bridge) we may proceed as follows :

Let T=safe strain on ties AC, BC Fig. 10, and S and S′ the safe strains per square inch for the post $\overline{PC}$ and the chord AB, P being the load, $\overline{PB}=l$, BC P$=\theta$. The strain on BC or AC$=\frac{1}{2}$P sec. θ.

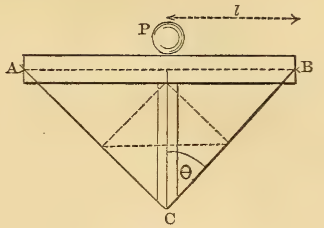


Fig.10

$$\therefore \text{Volume of } AC+BC=\frac{P\sec.\theta}{T}l\operatorname{cosec}\theta$$

$$\text{Volume of Post } \overline{PC}=\frac{P}{S}\,l\cot.\theta$$

$$\text{Strain on } AB=\tfrac{1}{2}P\tan.\theta$$

$$\therefore \text{Volume of } AB=\frac{P\,l\tan.\theta}{S'}$$

Total volume$=Pl$

$$\left(\frac{\sec.\theta\operatorname{cosec}\theta}{T}+\frac{\cot.\theta}{S}+\frac{\tan.\theta}{S'}\right)$$

to be a minimum. Whence,

$$\frac{1}{T\cos.^2\theta}-\frac{1}{T\sin.^2\theta}-\frac{1}{S\sin.^2\theta}+\frac{1}{S'\cos.^2\theta}=o$$

$$\therefore \frac{S'+T}{TS'\cos.^2\theta}-\frac{S+T}{TS\sin.^2\theta}=o$$

$$\therefore \tan.\theta=\sqrt{\frac{(S+T)\,S'}{(S'+T)\,S}}$$

52. Some numerical values will best illustrate the changes of θ, for the greater economy. Thus let S′=10000 lbs.,=10 (thousand lbs. being understood).

$$\frac{T}{S}=\frac{10}{10},\quad \theta=45^\circ$$
$$\frac{T}{S}=\frac{10}{7},\quad \theta=47^\circ 47'$$
$$\frac{T}{S}=\frac{10}{6},\quad \theta=49^\circ 3'$$
$$\frac{T}{S}=\frac{10}{5},\quad \theta=50^\circ 47'$$
$$\frac{T}{S}=\frac{10}{4},\quad \theta=52^\circ 55$$

The formula may be made to include cost and specific gravities of ties and compression members if desired.

A considerable saving may be effected in the Fink bridge by using the above formula. For complete accuracy the transverse bracing should be considered also. For slight changes of θ, it does not vary, though, as rapidly as the vol. of ties and columns.

53. With regard to some of the foregoing subjects the writer invites criticism, so it be just and its aim—the Truth, for in finding the last he is as much interested as any one; but an intemperate attack too often degenerates into personality, which "though it make the unskillful laugh, cannot but make the judicious grieve."

ANCIENT ROMAN WORKS.*

From "The Builder."

No relics of the period which preceded the Gothic invasion affords us such an insight into the rough yet civilized manners and customs of the citizens of Rome, and of the vast multitudes who bowed to the imperial rule, as do the stately amphitheatres whose ruins still grace so many Italian towns, though earthquakes have levelled some, and tempests are gradually, but not less surely, destroying those which yet remain.

It is proposed to arrange the following remarks in three divisions:—First, briefly dwelling on the origin and history of the Roman amphitheatres; secondly, on the plan and general arrangement; and thirdly, on the construction and materials employed in their erection.

It can hardly be doubted that the Romans obtained their ideas of amphitheatres from the Etruscans, and the remains at Sutri are a strong argument in favor of this belief, where we see a natural amphitheatre formed on the sloping sides of a hill, with the seats hewn out of the solid tufa rock. The Greek amphitheatres were also constructed in the same manner; sometimes Nature herself formed the amphitheatre almost without the labor of man, and at other times an ancient stone quarry was converted by small trouble into an amphitheatre accommodating many thousands of spectators. Such amphitheatres as these can scarcely be said to have been erected, since they were chiefly formed by excavation, and it remained for the Romans under C. Scribonius Curio to erect the first building of this kind, in Rome itself; though history gives such strange accounts of this structure that they sound almost fabulous. It is supposed to have consisted of two wooden theatres placed back to back and made to revolve on pivots, so that by means of windlasses and machinery, detailed by Pliny, the *visorium* of each could be brought round face to face and united on the line of its diameter, or it could be used as two separate theatres. Pliny narrates that it was formed "of two large theatres of wood mortised together in a singular manner, and suspended so as to turn freely, on both sides of which were exhibited the afternoon shows of plays."

The name "amphitheatrum" was first given to the building in the Campus Martius, erected soon after this by Julius Cæsar, and which was also of wood; but both these were probably destroyed by fire, and Statilius Taurus, in B. C. 29, erected the first stone amphitheatre at the command of Augustus. This was also situated in the Campus Martius, but the seats and the whole of the interior being of wood, it met the same fate as its predecessors. The Romans at this period had grown enamored of gladiatorial contests, the excitement of which became almost as necessary to them as food; and as the Imperial city was now left without any building of this class, Vespasian resolved to erect an amphitheatre which should eclipse all former ones, and the Colosseum, or the Flavian amphitheatre, was commenced. Situated between the Esquiline and the Cœlian hills, on the site of the great artificial lake which washed the walls of the Golden House of Nero, it stood almost centrally in ancient Rome. This enormous structure took only ten years and nine months in building, during which time 15,000 men are said to have been employed on it. Nevertheless, Ves-

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* By Mr. W. Hilton Nash. Read before the Architectural Association.

pasian did not live to see it completed, and Titus dedicated it in A. D. 80, although it was not fully completed till the reign of Domitian. The history of this building is perhaps more varied than that of any other, it having been used during the eleventh and twelfth centuries as a castle by the Frangipani family, in 1450 for a hospital, and in the seventeenth century for the representation of the Passion plays so much in vogue during the Middle Ages, and finally in the year 1700 a saltpetre manufactory was carried on within it. Wherever the Romans came with their conquering legions, we find traces of their amphitheatres; so that not only in Italy, as at Rome, Verona, Polo, Pozzuoli, Pompeii, and Capua, but also in France, at Nismes and Arles, and in England, at Cirencester, Silchester, Dorchester, and Old Sarum; and in many other countries have these interesting remains been discovered. But when we reflect on the gross butchery which these ancient buildings have witnessed, when the seats were filled, tier upon tier, with thousands of spectators crying out in the common language of the amphitheatre, "Kill him, tear him, burn him! Why is he so fearful of death?" our thoughts are sobered, and we cease almost to regret that the buildings are in ruins.

Let us now consider the plan and arrangement of the Roman amphitheatre. There are three main features in all structures of this class:—the arena; the *gradus spectatorum*, or seats for the spectators, forming altogether what was termed the *visorium;* and the colonnades and vomitories, or staircases, by which access to the seats was obtained. The arena itself varies in size and proportion, but is usually about one-third of the shorter axis of the building; it was usually covered with sand, as its name denotes, but was sometimes boarded. A wall about fifteen feet high, termed the podium, surrounded the arena, for the purpose of protecting the spectators from the enraged lions or the bounding panther; and on this wall were rollers which turned in sockets, so that the animals could not climb it, the summit being frequently protected by net work, as in the Colosseum, where Nero, in a fit of extravagance, is said to have fastened the nets with amber-beads. In addition to this, ditches or *euripi* sometimes surrounded the arena as a further safeguard against any accident. On the summit of the podium in the Colosseum were the seats of distinction for the senators and the emperor himself; above this were the seats for the equestrian order, or, as we should say, the upper classes; above these, divided by a *præcinctio*, or landing, were the seats for the *popularia*, or middle classes; and the upper seats of all were occupied by the *pullati*, or lower orders. In the Colosseum a colonnade runs round above this, not usually met with in other buildings, which was appropriated to the women, and on the flat roof of this colonnade were stationed the sailors who were engaged in the management of the velarium. Each story was divided from the other by a præcinctio, and intersected at intervals by the passages leading to the various staircases. The oblong form of all amphitheatres, except the double theatre of Curio and Cæsar's wooden theatre, which were circular, is a feature which we may do well to consider. These buildings were frequently used, not only for combats between man and beast, but between small bodies of armed men, and by the elliptical form of the arena these gladiators were better able to extend their line, and appeared to more advantage than if they had been placed in the centre of a circular platform with little scope for diversity of movements. The Pompeian frescoes afford illustrations of this, for there we seldom see the combatants massed, but in a long extended line. The scorching rays of an Italian sun made it necessary to have some covering over the amphitheatre during the games, since the people sometimes sat for six or eight hours together, and often took their places over night to secure a good seat; on this account a large awning called the *velarium*, made of woollen cloth or silk, extended over the whole of the visorium. The difficulty of raising and maintaining in its position this vast awning must have been great, when we remember that the least diameter of the Colosseum is nearly equal to the length of the Menai Bridge, and, in fact, in boisterous weather, the awning could not be hoisted, and when this was the case the Romans came to the show in broad hats and with a kind of parasol called *umbella.* Fontana, in his interesting work, gives a curious illustration of the velarium, but no description. Gwilt, however, supposes it to have been arranged in the following manner:—A cable being placed round the edge, and following the curve of the podium, strong ropes were attached to it in the direction of the radiating walls on the plan, and passing through pulleys in the heads of the masts ranged round the building and resting on corbels; there were 240 of these masts in the Colosseum, each one passing through a hole in the cornice and socketing into the corbels below; these ropes were then brought through the heads of the masts and down to the ground, where numerous windlasses were fixed to receive them, and at a given signal every windlass commenced its work, and the immense tent moved slowly upwards till it arrived at the level of the upper cornice, and when fixed in its position, sloped gracefully towards the centre; the central part, viz., that over the arena, being left open to the sky. The cloth itself might have been moved on rings attached to the main ropes, and worked by persons stationed on the parapet. The hoisting of the velarium was in Rome always entrusted to sailors, many hundreds being employed, and encamping during the gladiatorial shows in the neighborhood.

We now come to the third division of the subject, viz., the construction and materials employed. The construction of almost all amphitheatres was a succession of cross walls converging towards the centres of the different arcs, intersected by other walls of gradually increasing diameter, pierced by arches on which were bedded the seats for the spectators, and four principal gateways were placed at the extremities of the major and minor axes of the ellipse. The Colosseum at Rome, though not so skilfully or carefully built as some other examples, is, as it were, an epitome of all other amphitheatres. This vast structure had four corridors running entirely round it on the ground level, the two largest being on the exterior, and forming a sort of covered portico or ambulatory, each corridor being roofed by a continuous semicircular vault supported on square piers. Between the second and third corridors is a large space occupied by staircases leading to the various seats; the entrances to the staircases being from the second and third corridors; from the latter also are other entrances into staircases which are in another space between the third and fourth corridors. The third corridor, which must have been very dark, received a small amount of light by holes in the vault. We are apt to affirm that the ancients never built hurriedly, but in this vast structure, which some maintain only took ten years and nine months to build, we see striking evidences both in the work of Vespasian and Titus of the rapid way in which the works were pushed on. Carelessly-dressed stones—many of the arcades being unequally spaced out—no mouldings preserving the same level throughout the building, and the profiles of the mouldings themselves in some cases unfinished, all tend to strengthen this belief; and yet, in spite of all this loose detail, there is perhaps no building in the world which has called forth such universal admiration as the Colosseum. The general effect of the Colosseum would have been far finer without the upper or fourth story, which is so heavy and unmeaning that some have even doubted whether it was part of the original structure, and I believe this is the only amphitheatre which has this addition. The amphitheatre at Verona has only three stories, and the exterior elevation has consequently a more æsthetic proportion. Mr. Ferguson conjectures, in his "History of Architecture," that this upper story may have been added for the purpose of working the velarium, and this seems more probable, since to cover so vast a surface with an awning would be next to impossible without some space on which the sailors who worked the velarium could carry on their operations; and this would be provided by the flat roofing covering the colonnade. Mr. Parker is of opinion that this story is of the third century, and that there was originally one of wood of a somewhat similar design. The system of Roman vaulting has been so ably described by Mr. Phené Spiers in his paper on the "Roman Baths" that I can only briefly dwell upon it; for, indeed, the vaulting which exists in the ruins of amphitheatres is of a most elementary character, its purpose being usually to roof the passages leading to the seats, and for supporting the sub-structures. The principal materials employed in the Colosseum are stone, brick, concrete, and pumice stone. Tiburtine stone, or, as it is now called, Travertine (though the former is the more correct name, as the original quarries were on the banks of the Tiber, near Tivoli,) forms the facing of the outer walls, also the piers of the two outer corridors, the centres of the arches, and some bands in the inner walls. The stone is largely used at the present day in Rome, and forms an excellent facing stone, almost like marble. The stonework in many cases is not bonded with the brickwork, and in some instances has entirely separated from it, leaving a straight vertical joint. Large blocks of tufa are also used for the filling in, but the Roman architects did not consider the tufa walls, or even the tufa and brick combined, capable of sustaining the compression which the numerous rows of seats and the vast concourse of spectators who occupied them would necessarily subject them to: so at frequent intervals they cut away the brickwork and inserted piers of travertine to assist in carrying the weight. The stonework of the Colosseum was originally held together by iron cramps; these have rusted, and eventually split the stone, and have fallen out or been carried away, perhaps to be used again to cause more similar destruction. I know of no example of the *opus reticulatum* in the Colosseum; nevertheless, it was commonly used in contemporary buildings. It consisted of small pyramidal blocks of tufa, flat on one side, and about 2½ inches by 2¾ inches square on the face, and pressed into a bed of concrete while wet, and forming, as its name implies, an appearance like network. The main body of the work is of brick, and some of the finest quality. The bricks are tile-shaped, about one foot by one foot six inches, and were called by the Romans *lateras,* and formed, when bedded, the famous *opus lateritium.* The first century produced the finest brickwork the world has ever seen, and in Nero's time we have ten courses to the foot of these tile-like bricks, laid with an almost imperceptible mortar joint; but after the reign of Trajan, the bricks were made thicker and the mortar-joints also increased, being often one inch thick, till, in the fourth century, and during the reign of Constantine, we find the brickwork measuring four courses to the foot, and by knowledge of these facts it is easy to ascertain the date of any Roman brickwork with tolerable accuracy. The Colosseum possesses good specimens of first-century brickwork, and also the Amphitheatrum Castrense in Rome, and also some contemporary brickwork from the Palace of Nero, date A.D. 60. Here we see the tile-work called *opus lateritium* bedded in mortar composed of puzzolana and lime in proportions of about three of the former to one of the latter.

The earliest instance of the use of concrete (*fartura*) at Rome is at the ruins of the Emporium, B.C. 195. A vast amount of concrete was used in the Colosseum, chiefly for filling in the vaulting and internal walls; but finding this material too heavy, pumice-stone was employed for the vaults, as it could easily be obtained from the neighborhood, and is found at the present day mixed with the Roman tufa in beds a few inches thick. All concrete is liable to fracture by the shrinkage of the material, and in walls of any great length the fractures occur at equal distances apart (as in the Roman walls of Richborough Castle). Whether the Romans were aware of this fact or not is uncertain, but they frequently introduced layers of thin bricks at intervals to tie the whole mass together, and perhaps more with the idea of bonding the work than of counteracting its shrinkage. This tile-bonding may be seen in almost all Roman concrete walls in Europe, and in the Temple of Minerva Medica at Rome.

Though not strictly within the province of an architectural paper, it may be well to touch briefly on the sports and combats which took place on the arena. The gladiatorial encounters were either between themselves or with wild beasts, and special schools were instituted where the gladiators underwent rigorous training. Gladiators were first exhibited in Rome in B.C. 488, by M. and D. Brutus; and they were all bound by a solemn oath—"We swear, after the dictation of Eumolpus, to suffer death by fire, bonds, stripes, and the sword, and whatever else Eumolpus may command, as true gladiators we bind ourselves body and soul to our master's service."—(Petronius.) In the early days of Rome, before the public taste became degraded, the office of gladiator was confined to the lowest classes; but the thirst for popular applause induced men of good family, and even senators, to enter the lists; and hence we find an edict forbidding persons of exalted rank from entering the arena. Previously to the performance of any spectacle the walls were placarded, and notices (*libelli*) issued by the editor or showman, stating the nature of the exhibition and the names of the combatants, and these notices were frequently illustrated, after the manner of our nineteenth-century play-bills. On the appointed day, those who had not previously obtained tickets took up their position at daybreak, and sometimes over night. The *equites*, the *popularia* and *pullati* being seated, were followed by the senators, who had previously sent their *biselli*, or chairs of state, to be placed on the podium. Then a flourish of trumpets and a grand procession announced the arrival of the emperor, who, sitting at the *suggestus*, or royal box, gave the order for the sports to commence. At first the combats are comparatively tame, but as soon as the fiery blood of the gladiators is roused, they throw away the wooden staves with which they have been fighting, and take to the sword and buckler, and the *retiarius*, with his net and trident, seeks to entangle and despatch the *secutor*, who follows him round and round the arena with his deadly knife. The victor looks up at the vast sea of heads, and awaits the final signal of the up or down turned thumbs of the spectators before taking up his sword to dispatch his victim.

The death-signal is given, the sword descends, and the corpse is dragged off the arena by a large hook to the *spoliarium*, through the Porta Libitinensis, or Gate of Death. And now the great trapdoors on the surface of the arena are seen to open, and vast wooden cages appear, containing the wild beasts, which "leap out of the earth," as an old writer has it. The cages open spontaneously, then sink into the earth again, and the ferocious animals let loose on the arena are slaughtered by the bloodthirsty gladiators. The apparatus for bringing the wild beasts on to the arena in this manner only existed in some amphitheatres, but not at Pompeii or Verona. The recent excavations at the Colosseum have brought to light traces of the grooves in which these lifts or *pegmata* worked, and also the chase for the counterpoises, and the sockets in which turned the pivots of the capstans used for working the apparatus. These lifts may also have been used for bringing up the ships which were to take part in the *naumachia* or naval engagement, and in the Colosseum a vast gulf separated the arena into two parts, so that when the boards which covered it were removed any large stage properties could be lifted on to the arena. These sub-arena buildings or sub-structures were, perhaps, the most interesting portion of the amphitheatres, and, as it is the ambition of many persons to see behind the scenes of a modern theatre, so it must have been the desire of many a Roman citizen to see below the arena and get an insight into the mysteries which caused wild beasts to spring out of the earth and ships to float on the surface of the arena; for Seneca writes,—"The eyes of the silly people are astonished at all these sudden movements, the causes of which they do not understand." This species of amusement gave its name to the buildings which were specially devoted to them, and those at Rome were very similar to excavated amphitheatres. The *naumachia* of Augustus was 1,800 feet long by 1,200 feet broad; and at the sea-fight held there during the reign of Julius Cæsar 4,000 seamen and 1,000 marines were engaged. These *naumachia* were constructed of stone, as we read that that of Domitian was pulled down to repair the Circus Maximus. No amphitheatre was capable of being used as a *naumachia* unless provided with vast underground works and drainage on a large scale; the amphitheatres of Verona and Pompeii, for this reason, could never have been used for this purpose, but in those of Rome, Capua, Pozzuoli, and Pola, these entertainments were undoubtedly provided. Representations of the chase were not wanting among the amusements of the arena, and trees torn up by the roots were transplanted on its surface, and wild boar and stag hunts took place in these temporary forests, as may be seen depicted in the frescoes at Pompeii; and Calpurius describes the visit of a country lad to Rome, who relates all the wonderful things he saw there. "We saw the amphitheatre, with interwoven beams rising to heaven. . . . . . I saw all kinds of wild beasts, ... not only those carnivorous monsters of the forests, but sea monsters, together with fighting bears." Sixty-two amphitheatres are enumerated by Clerisseau in his "Antiquities of France," as still existing; but it will hardly be necessary to mention all of these, as the remains of some are scarcely traceable; however, we may consider a few of the more important ones. The great amphitheatre of Capua, which rivalled the Colosseum in size, is said to have held 87,000 spectators, but only two of the seventy-four arcades that formed the lower story exist. It had three stories, of the Doric order, and more pure in detail than the Colosseum. The sub-arena structures are more perfect, although the general arrangement of the plan is nearly identical. We see the same dens for the wild beasts, the sockets and grooves for working the *pegmata* or lifts by which they were hoisted on to the arena; but the arena, instead of being a boarded floor, is carried on vaulting, and pierced by openings for the lifts, and probably each of these trap-doors had a water-tight covering, to enable the whole of the arena to be flooded. If such was the intention, the surface would not be covered with more than 2 feet or 3 feet of water; while the boats or galleys for the naval engagements would float in the deep canals, three in number, which run parallel with the major axis of the ellipse, the other water being merely for creating an impression on the spectators that the whole surface was flooded to an equal depth. The *opus reticulatum*, surrounded with a framework of brick, which is found in this structure, proves it to have been partly built in the time of the Emperor Hadrian, and it is probable that a greater part was executed in the second century.

It stands on the site of the ancient city of Capua, now called Santa Maria di Capua Vetere, and three miles beyond the modern town of that name. It is not positively known by whom the amphitheater of Verona was erected, yet some writers attribute it to the time of Augustus, and the good preservation of the interior is owing to its having been used for plays during the middle ages; and this practice is still continued, for when I visited this picturesque city in 1875, a stage was erected in the amphitheater, and a play was acted one Sunday afternoon. Of the outer wall which supported the portico surrounding the building there remain only seven piers. At the two extremes are the principal entrances, the sides of which are parallel. I think the entrances at each end give great grandeur to the design, as they are more marked and architecturally treated than in most examples. On the interior over the two entrances are galleries inclosed on their front and sides by a balustrade, and the seat of distinction was situated here. It is later in date, says Fergusson, than the Colosseum, Capua, or Nismes, on account of its rustication; this, I think, is doubtful, especially as this rustication is said to have been unintentional, and simply caused by their not being time to complete the work as intended. The idea of rusticating piers and columns is said to have originated from this building. The amphitheater at Pola, in Istria, is about the same age as that at Verona, but the arena is almost entirely destroyed, and only the outer walls remain. The interior may possibly have been of wood, and has either decayed or been burned; the dimensions are nearly the same as Nismes, viz., 436 feet by 346 feet, and 97 feet high. The two lower stories have pilasters, but the upper one has none, and was probably built so as to allow of the masts which carried the ropes of the velarium to come down and socket into the cornice of the second story; and there is an open kind of battlement terminating the attic story, which it is conjectured had some connection with the working of the awning. There are here the usual canals for conducting water for the purposes of the *naumachia*, lined with water cement, but the substructures only go under part of the fabric, since the labor of excavating under the whole surface would have been enormous, as the building is situated on the slope of a rocky hill. Four rectangular towers are attached, which were probably provided with wooden stairs, as no remains of stone ones are discernible, but the use of these towers has puzzled the most learned antiquaries, and no satisfactory solution of the problem has yet been arrived at. The Amphitheater of Puteoli or Pozzuoli, near Naples, has substructures as fine as any known amphitheater, though in size it is inferior to the Colosseum or the Capuan amphitheater. It is probable that it was used as a *naumachia*, since we find a large central channel deep enough to float a Roman galley, and constructed after the same manner as that at Capua, with small square holes all round the arena, which have evidently been fitted with water-tight coverings to prevent the escape of water when the whole arena was flooded; this would have been quite practicable here, but in the Colosseum it could not have been done without some modifications of its existing plan. This building was chiefly erected in the time of Hadrian, and exhibits beautiful specimens of the construction of his time. The amphitheater of Pompeii accommodated about 10,000 or 11,000 spectators, and was constructed chiefly of rough masonry, called *opus incertum*, with quoins of squared stone, partly filled in with rubble, the whole originally having been covered with ashlar. The entrances are at each end of the ellipse. The podium was found to be richly ornamented with frescoes when first excavated, but this rapidly peeled off on exposure to the atmosphere. The amphitheater at Ostricoli is small, measuring only 312 feet on its major, and 230 feet on its minor axis, and is two stories in height. It is in a very ruined condition, though one of the most modern works of this class; nevertheless its simplicity of design makes it a valuable specimen of Roman art.

In France there are two amphitheatres of considerable size, one at Nismes in Aquitaine and the other at Arles, the former capable of containing about 17,000 and the latter 20,000 spectators. At Nismes, a wooden floor with trap-doors in it still exists, though Mr. Parker is of opinion that this is not the ancient floor, but replaced one of the same character. The substructures, which differ from those of the large Italian amphitheatres, appear to have had no stone staircase for access. At Arles there were originally some subterranean arches, two stories of sixty arches each and an attic, well constructed of carefully-fitted stones of large size, the orders employed on the exterior being the Doric and Corinthian. The artistic effect of these buildings is mainly due to their mass, combined with an elegance of curve which is always found in buildings of an elliptical form; the circle does not give the same variety or gracefulness of curve which is found in an ellipse. Some buildings strike us by their beauty of detail, but it is evident that this is not the first impression produced by the Colosseum or any other amphitheatre which astonishes us by its vastness. The solid tufa walls of the sub-structures, the numerous tiers of vaults upon vaults rising with their rugged outline against the blue background of the clear Italian sky, inspire us with a sense of reverence towards the builders under the Imperial rule, conquerors of the world, and masters of the arts and sciences, which we, even in these latter ages, seem hardly to have equaled. It does one good to contemplate a block of tufa, such as we see in the sub-structures of the Colosseum, discarding the bondage of mortar and sufficiently secure by reason of its enormous size, and bedded with that care which the early builders knew so well the value of. These solid structures make our mean brick walls, which are measured by inches, look like a sheet of paper compared with an armor-plate.

Some modern structures have been erected which in many points are analogous to the Roman amphitheatres, but a more difficult problem than that which the Roman architects had to solve has been successfully mastered by our modern constructors. In the Roman amphitheatres, all that was required was that every one should be able to see the doings in the arena, but we have to provide buildings in which an assembly can see and hear also ; it is not sufficient for them to be spectators, they must be an audience. This difficulty has been met in a fairly satisfactory manner in the Albert Hall. No wild beasts are let loose on this arena, so no podium is required, and no English tars fight on its surface in ships as in the ancient *naumachia*, therefore no substructures or great drainage works are necessary; but there are features in this building which carry out the Roman principles, though perhaps the staircases, owing to modern ideas, are not planned on the same simple and masterly manner; nevertheless, I should advise any one who wishes to obtain a faint idea of the grandeur of a Roman amphitheatre, and has not an opportunity of seeing the works themselves, to study this building. The shell of the Albert Hall or the "Kensington Amphitheatre" consists of two concentric walls, between which are contained the staircases, corridors, and general arrangements for the service of the central hall, and all the necessary offices, etc. The arrangement of the *cunei*, or wedges of seats, is amphitheatrical, and they are entered from spacious corridors and dormitories; but the simple and stately form of the inverted cone is broken by three tiers of boxes running round the building.

In considering all Roman works, it is well for us to understand as much as possible the sentiments which actuated the Romans in the construction of their edifices. Wealthy and proud, they delighted in showing their wealth in costliness of materials rather than in delicacy of form and thought—to strike wonder into strangers' minds, not by their refinement of thought as expressed in their buildings, but by massiveness and apparent and frequently real solidity which has seldom been surpassed. The power of mass is the first thing which strikes an ordinary beholder, and the Romans, as has justly been observed, were an arch-building but not an architectural nation. The arch and the vault, grafted on the Greek models, soon subsidised the vertical and horizontal lines which were the ruling features of Greek design. The Corinthian order supplanted the manly Doric, and I believe the decline of Rome may be dated from the time when the simple Doric and the graceful Ionic were almost entirely discarded, and the meretricious Corinthian became the universal order in all buildings. This is noted in M. Viollet-le-Duc's "Lectures on Architecture," so admirably translated by Mr. Bucknall, where we read, in reference to the degeneracy of art and the looseness of detail in Roman times in comparison with the Greek epoch, that Roman architects "had not leisure to study purity of line, the stone-dressers had not the time for these refinements; it is less trouble to describe a quarter circle with a compass than to find an indescribable curve," such as the Greek torus. In one important item the Romans far excelled the Greeks; they had the faculty of planning, and for my own part I never came away from studying a Roman plan, whether it be for a temple, a palace, a public bath, or an amphitheatre, without feeling braced and refreshed in mind, and strongly impressed with the skill of the Roman architects in this respect. We in the present day, and especially in this great capital, are in a position very similar to that of the Roman citizens, surrounded by wealth and comfort which borders on luxury; we live in an age as great in its way for building, if not for architectural work, as any era of ancient Rome; and we architects in this busy nineteenth century are pushed and hurried on in our work like the Roman architects of the first century; we cannot afford to give a whole day to the finessing of a curve or the rounding of a torus. Art is too long, and life is too short; or, we might put it, the work is too great, and the percentage too small, for architects to work in this fashion; and an architect should not be obliged to look out anxiously for another building to occupy his attention as soon as one is completed, but should be able to give his heart and soul to his work, and be free from all anxiety, which is incompatible with that tranquillity of mind from which emanate all noble designs.

In conclusion, I must express my regret that the scope of this paper has not allowed me to treat of the great drainage works which supplied these vast structures with water, and afterwards conducted it to the Tiber; for the aqueducts which conveyed the water from the distant hills over the dreary tracks of the Campagna, were among the clearest evidences of the grandeur of the empire. It was in this class of work, where the mechanical powers of mind and matter were brought into operation, that the Romans were *facile princeps*. The great drain of the Colosseum itself was a work of no small magnitude, and the small drains which ramify the soil of Rome like the veins and arteries of the human body, make it impossible to dig down more than fifteen feet without coming upon some supply of running water. And now, as we draw near to the close, not only of these remarks but also of our session, I hope that some few of our members may become imbued with that enthusiasm which should accompany the contemplation of these noble works; for architecture must be studied with love or not at all; and when we separate, some to study the beauty of Gothic detail and revel in the grandeur of the grey and solemn minster, and others to seek the shores of sunny Italy, there to ponder on the past glories of the Eternal City, where, as the poet says:

> "The very dust we tread stirs as with life,
> And not a breath but from the ground sends up
> Something of human grandeur;"

let us look reverently on these structures, which from various causes have happily escaped the cruel hand of the restorer, and still go crumbling on in untouched solemnity, so that, as we traverse the arcaded Gothic cloister, or the deep, shady, vaulted corridors of the Colosseum, which we admire "as we admire the beautiful in death," our minds may haply be awakened to the spirit which actuated these early builders, who produced works which, as time rolls on, accumulate increased interest and veneration.

REPORTS OF ENGINEERING SOCIETIES.

Liverpool Engineering Society.—This society held its first meeting of the coming session in the rooms of the Royal Institution. Mr. Wilkinson Squire brought before the members some interesting samples of mortar. A piece from Sandown Castle, Kent, built by Henry VIII., was tested by Mr. Squire, proved to stand a tensile strain of 263 lbs. per square inch, the section tested being 1.8 square inches in area. A sample of mortar lately brought by Mr. Alfred Holt, member, from Nicopolis, a city built B.C. 40, to commemorate the battle of Actium, appeared to be as hard as stone. The President thought that, on account of its rough and honeycombed appearance it bore out his theory that the goodness of ancient mortar was due to the quality of the materials rather than to any particular care then bestowed upon its mixing. Mr. R. F. Pitt, member, read a valuable paper, entitled "Piecework *versus* Daywork." He endeavored to show that piecework is advantageous under most circumstances, the principals obtaining more work in a given time, and turning over their capital more quickly, at the same time the men make higher wages than they otherwise could do. He mentioned several instances where men earned by piecework double the current rate of wages, whilst men working beside them under similar conditions could hardly earn an ordinary day's wages. These men, by "daywork," or club rules, would have earned exactly the same; such methods of payment, therefore, tend to lower the skill of the artisan besides increasing the cost of the work.

Society of Engineers.—A party of about thirty of the members, associates, and friends of this society, on August 15th, paid a visit to the iron shipbuilding yard of Messrs. Samuda Brothers, at Poplar, and there inspected several first-class vessels now in course of construction. The party, headed by Mr. Clement Barnard, vice-president, and Mr. Perry F. Nursey, secretary, were received at the yard by Mr. J. T. Field, one of the managers of the establishment, and by him conducted to the objects of our visit. Proceeding through the worksheds, now in moderately active operation, the engineers were shown four new life-boats, two of which are screw launches, and a third, a row-boat, and were then taken to the hydraulic press, one of the largest in the world, and capable, when used in bending armor plates, of giving a pressure of 4,000 tons. Continuing the journey over piles of heavy timber and scraps of iron, the party arrived at the *Burgirafer*, an armor-plated ship with twin screws, now in course of construction for the Turkish Government. A sister ship, the *Payki Sherriff*, not quite finished, also for the service of the Ottoman Porte, was visited. She is 245 feet in length, 3,800 horse-power, with 4,700 tons displacement, and carries four 25-ton guns. The battery is within a strongly-armored turret, where enough space is obtained and gear laid down to vary the direction of the fire very considerably. Each ship carries ten inches of armor-plating. The unfortunate *Independencia*, whose back was broken in an unsuccessful launch at Messrs. Dudgeon's, some time ago, lies alongside, and is being rapidly repaired for the Brazilian Government. A beautiful ship the *Foo So*, designed by Mr. E. J. Reed for the Japanese navy, next received the attention of the engineers. She is 200 feet in length, of 3,500 horse-power, with 6,700 tons displacement, and is remarkable for spacious deck room and ample space for working her guns. She carries four 18-ton and two 10-ton steel guns, and is protected by armor-plating of 9 inches thickness. The *Foo So* is built in thirty-two separate compartments, in order to localize the effect of torpedoes, and such is her engine-power that a speed of thirteen knots an hour is expected to be obtained. The Turkish ironclads are detained in the Thames pending the present war.

IRON AND STEEL NOTES.

A singular fact with reference to the production of heat is described by M. Oliver (*Comptes Rendus*). A square bar of steel 15 mm. in width and 70 to 80 ctm. long is seized with the two hands, placed, one at one end, the other in the middle of the bar. The other end is pressed against an emery grind-stone rotating rapidly. In a few minutes the rubbed end is considerably heated. The band at the middle has no sensation of heat, but that at the extremity is strongly heated, so that it has to be taken from the bar. Thus, in certain cases, heat appears not to be propagated in metals from one part to that next it.

FUEL USED TO SMELT A TON OF IRON.—In January, 1876, the Cedar Point Furnace at Port Henry, N.Y., made iron with a consumption of 1.26 tons of anthracite coal to a ton of pig iron. The Crown Point Furnace at Port Henry, N.Y., uses 1.64 tons of anthracite coal to a ton of pig iron ; the Bay State Iron Company, at the same place, uses 1.33 tons of anthracite coal to a ton of pig iron, and the Fletcher Furnace at Buffalo, uses 1.399 tons of anthracite and .028 ton of coke, or 1.427 tons of the mixture to a ton of pig iron. In 1871, at the Glendon Ironworks in the Lehigh Valley, Pa., an open-top furnace used 1.19 tons of anthracite coal to a ton of pig iron ; in 1872 the same furnace with a small cone top used 1.325 tons ; and in 1873 the same furnace with a double cone top used 1.205 tons. The Thomas Iron Company, in the Lehigh Valley, Pa., in the last six months of 1875, use an average of 1.75 tons of anthracite coal (which at $3.41 a ton, cost $5.96) to a ton of pig iron; in the five years embraced in the period from 1869 to 1873 their average consumption was 1.978 tons of anthracite coal to a ton of pig iron. The Stewart Furnaces, in the Shenango Valley, Pa., use 1.891 tons of raw bituminous coal and .229 ton of coke to make a ton of pig iron, a total weight of 2.12 tons. The Brazil Furnace, Indiana, used, in 1872, 2¼ net tons of raw coal to a gross ton of pig iron. The coal then cost from $1.23 to $1.75 per ton at the mine. In 1872 the Fletcherville Furnace in Essex County, N.Y., used 0.95 ton of charcoal to smelt a ton of pig iron. The Deer Lake Furnace, No. 1, in the Lake Superior region, uses 1.073 tons of charcoal to a ton of pig iron ; Deer Lake Furnace, No. 2, uses 1.097 tons of charcoal ; Morgan Furnace, in the same district, uses 0.853 tons of charcoal; Bay Furnace uses 0.858 ton of charcoal ; Fagette Furnace uses 1.040 tons of charcoal ; and Elk Rapids Furnace uses 0.884 ton of charcoal. One of the furnaces of the Clarence Ironworks, England, uses 1.125 tons of coke to a ton of pig iron, and a furnace at Pouzin, France, uses 1.2 tons of coke to a ton of pig iron. Mr. William E. S. Baker, Secretary of the Eastern Ironmasters' Association, has published a table of the cost of the materials used in making a ton of pig iron each year from 1850 to 1875, in which he gives 2.859 tons of anthracite coal as the average consumption of fuel to a ton of pig iron at a furnace on the Susquehanna river in Pennsylvania ; in 1875 the coal used to smelt a ton of pig iron cost $7.21. In 1875 Mr. John Fulton, then of Saxton, Pa., in the Broad Top coal field, estimated that to smelt a ton of pig iron would require 1.75 tons of Broad Top coke, 1.75 tons of Connellsville coke, or two tons of anthracite coal. At that time he stated the cost of each fuel at Harrisburg to be as follows :—Broad Top coke, $3.78 per ton ; Connellsville coke, $4.25 per ton ; anthracite coal, $4.50 per ton. The cost of the fuel used to smelt a ton of pig iron at Harrisburg was then as follows:—Broad Top coke $6.61½ ; Connellsville coke, $7.43¾ ; anthracite coal, $9.—*The Bulletin of the Iron and Steel Association.*

METALLURGY IN JAPAN.—The following is quoted by *Engineering* from a thesis prepared by a Japanese student, graduated at a United States college : Iron ores are very abundant in the Japanese Islands. The chief ores of Japanese iron industry are magnetic iron ore, specular iron ore, and brown hematite. The first is found in two varieties, one of iron-grey colour and the other black. Masses of this ore in the state of magnetic polarity, generally called lodestones, are found in the eastern part of Nipon, Sendai, and Nambu. They are very highly esteemed for the steel manufacture, for swords and compass needles. Japanese furnaces are small in size and simple in structure, although the principle is the same as that of the blast furnace used here and in Europe. The walls of the Japanese furnace are built with fire-proof clay, and sometimes with a few stones. The shape of the furnace is round at the bottom, having at one side an opening which is closed with a clay stopper. On the opposite side of the furnace wall, a little above the bottom, there are two openings through which a continuous stream of air is passed in the furnace by means of a Chinese bellows worked by men. Before the ores are put into the furnace they are piled up in heaps with coal and calcined, or roasted, so that the water, carbonic acid, and sulphur may be expelled. The Japanese do not know the theory of the puddling process used in the Western countries, but the principle is exactly the same. The cast iron mixed with some sand and some iron scales is melted with charcoal heat in a furnace similar to that already described, and kept in this melted state for several days until the whole mass assumes a fluid appearance. The Japanese method of steel making is entirely different from those usually employed in Western countries. It is done in this way. They mix a certain quantity of pig iron, which contains too great a quantity of carbon, with a certain quantity of bar iron, which has too little carbon, and cover the mass with borax and smelt in a small crucible of fire-proof clay for more than a week. The borax is used to dissolve any impurities in the slag. When the metal is separated from the slag floating on the surface, it is taken out and hammered hard, and alternately cooled in water and oil many times. After the steel has been cast in that method, it is cemented and tempered. The method of cementing consists in covering thickly the hammered steel with a liquid mixture of clay, loam ashes, and charcoal powder. When this layer is dried the whole is heated red hot and then cooled very slowly in warm water. The steel is now ground on a whetstone. The steel thus made is not very elastic, but is very hard. The explanation is that either the Japanese do not understand the tempering process, or they are unable to remove entirely the impurities from the steel. I have often heard Japanese blacksmiths say that watch springs can never be made in Japan, for Japanese steel is not elastic. The Japanese take great care and time in steel manufacture for swords. For instance, for ordinary knives forging and cooling are to be done only four times, but for swords fifteen times. Copper is and will be the most important metal of Japan. It is found in almost every province. For roasting they have a furnace covered with a shed, provided near the bottom with several openings for the draught of the air. Five alternate layers of ore and wood are placed in the furnace and burned.—*Journal of Society of Arts.*

RAILWAY NOTES.

On the Advantages of Brake Blocks of Cast Steel.—By Dr. Rohrig. The Ober-Schleswig railway has for some years used brake blocks of cast steel, or rather of an alloy of cast iron and cast steel, invented and manufactured by Glockner Brothers, of Tschirndorf, near Halbau. The advantages of this material, given as the result of a four years' experience, were as follows:

(1) A saving over wood in cost of maintenance amounting to 31 per cent. in wagons, and 39 per cent. in brake vans.

(2) The tires do not have flat places worn on them, which lessens the wear of the rails, as well as of the tires themselves.

(3) The damage done to the axle-boxes, &c., by these flat places is also saved.

(4) There is less danger of heating the tires, and the blocks cannot catch fire.

(5) The mechanism of the brake can be simplified, as the levers require less frequent adjustment.

Further experience has fully confirmed these results. The brake vans on the above railway fitted with these brake blocks have been found to run from 184,000 to 230,000 miles before the blocks need changing, whilst with wooden blocks the distance only averages 120,000 miles. The railway has now seven hundred and fifty wagons fitted with these blocks, and they have been largely adopted by other companies, especially the Uetliberg Railway Company, who report most favorably on their advantages for steep gradients. They are accustomed to send wagons down the incline without an engine at the rate of twelve miles an hour, relying on these brakes only, and with perfect security; whilst there is an absence of the jarring which is so unpleasant with ordinary brakes.

The only disadvantage is that the co-efficient of friction is somewhat less than with wood; but this can be got over by giving increased power to the gearing used for putting on the brakes.—*Abstracts from Journal of Institute of Civil Engineers.*

We learn from our Indian exchanges that the construction of the Indus Valley Railway is making rapid strides towards completion. The chief engineer hopes to have the line open from Mooltan to Sukkur by the end of July. The line is now laid from Mooltan to Chowdree, in the Bhawulpore State, 113 miles, and by the end of this month it is hoped that it wlll be completed as far as Khanpore, 140 miles from Mooltan. The temporary bridge at Adamwahan still stands well, and arrangements are being made for a steam ferry against its giving way in the floods. On the Sukkur side, equally satisfactory progress is being made, Mr. Mackenzie, contractor, undertaking to complete sixty miles to Gobla within two months, being at the rate of a mile per day. Midway between Sukkur and Mooltan too, in the Khanpore section, work is being pushed forward, the material being sent by camel to the scene of operations there. Beyond Sukkur the work is not neglected, although the present effort is to open only as far as Sukkur. The line is quite complete from Kotree to Schwan, thus leaving only the short break to Sukkur from Schwan to be completed after the first effort has been successful. If the river behaves, there is every reason to hope that the line will be open right through within eighteen months from this date.

ENGINEERING STRUCTURES.

Objection to the Algerian Sea.—In a letter to M. Daubrée, M. Naudin states his fears that if the shallow Algerian *chotts* are filled with water, they will form an immense pestilential breeding place, worse than the Tuscan Maremma or the Pontine Marshes. The greatest depth in the centre of the basin would not exceed twenty-five metres, and over most of the surface the water would be very shallow. All the conditions would be favorable to an immense multiplication of organic life, decay and miasm.—*Comptes Rendus.*

The Canal which the Government has Constructed to overcome the obstruction to navigation caused by the Des Moines Rapids in the Mississippi, at Keokuk, Iowa, was formally opened with suitable ceremonies last Wednesday. The canal extends along the Iowa shore from Keokuk to Nashville, a distance of seven and six-tenths miles, is 300 feet wide in embankment and 250 wide in excavation; minimum depth of water, five feet; maximum depth, eight feet, which is sufficient to float the largest steamers that ply the waters of the upper Mississippi. The embankment enclosing the canal is ten feet in width on top, with a rip-rap covering two feet thick and carried two feet above extreme high water mark. The fall in the entire distance which the canal extends is 1,875 feet. There are two lift-locks and one guard-lock, each 350 feet long and eighty feet wide on top. These are built of solid cut-stone masonry, and are pronounced by experts who have examined them to be very substantial and highly creditable specimens of engineering skill. Sluices of sufficient capacity to control the surplus water carried into the canal by the numerous streams emptying therein during their frequent floods, are built around the locks. These were not included in the original estimate and have materially increased the cost of the work. The machinery for operating the lock-gates and wickets is made from an original design of Major Amos Stickney, the officer in local charge of the improvement. This plan consists of a system of pulleys, chains, and wire ropes, operated by means of a pump forcing the water into hydraulic cylinders sunk behind the walls back of each gate, connected by means of iron pipes, with an engine situated near the head of the lock, so that one man at the engine can handle the massive gates and wickets with ease and precision. The original estimate for this improvement was $3,390,000. It has cost so far $4,281,000. Work was begun on it in October, 1867. It is estimated that $100,000 will yet be required to finish the work, and Congress will be asked to make an appropriation of this amount at the next session. The improvement is of incalculable importance to the navigation of the Mississippi river, as it removes the only obstruction now remaining between New Orleans and St. Paul."—*Engineering News,* iv. 228.

Fire-Resisting Compound Arched Floors and Roofs.—We have recently had an opportunity of inspecting a floor constructed on a principle which has been patented by Mr. George Northcroft, architect, of 5 Castle Street, Liverpool. The following description will enable our readers to understand the mode of construction, and appreciate the advantages of the system. When the walls of the building to which the system is to be applied are brought up to the floor level, flat centers are set, filling the entire space between the walls. On the side walls cast-iron wall plates are bedded, having cast seatings for expansion rollers, on which are set rolled iron girders, placed at such distances from each other, as is necessary, according to the span and load to carry. On the end walls other cast-iron wall plates are set, the four being secured at the angles, thus forming a continuous bond round the building. On the cast-iron wall plate along the end walls a fireclay skew-back, the full depth of the floor, is bedded. To each rolled girder is bedded in asphalte composite closely fitting fireclay skewbacks with radiated joints, which completely surround and protect the girder from the fire. Thus, divided into bays, the floor may be continued for an indefinite length. Fine mortar or cement is used in setting the brickwork filling the bays. The key of each bay, the full depth and width of the floor, three inches wider at the crown than at the soffit, is set first. The cross joints of this key are radiated from the center each way, hence what may be termed the keystone of this key is three inches thinner in the soffit and six inches shorter than at the crown. Between the keys running parallel to the skewbacks surrounding the girders, the space is filled in with two courses of 6 inch fire bricks, grooved and tongued into each other, one course kept uniform with the floor level and the other uniform with the ceiling, the space between being determined by the depth of the floor. These two upper and lower courses of bricks are also united in the middle of the floor by keys the full depth of it, which act at right angles to the keys of the bays. From these keys the joints of the 6-inch bricks radiate, thus producing a complete counterpoise, constituting the floor a series of compound arches. If great strength is required and the bearing long, the girders used are proportionately strong and deep, and set together. The patentee claims originality in his design, in the provision made for the complete protection, as well as the expansion and contraction, of the iron employed in construction; in the combination of indestructible material, whereby great widths can be spanned without any intermediate supports, and the floors made to sustain unusually heavy loads.—*Architect.*

ORDNANCE AND NAVAL.

Small Arms for Field Artillery.—The Martini-Henry carbine recently issued on a small scale to the Royal Artillery has proved itself, as far as early trials demonstrate, a most efficient weapon. It is highly satisfactory that this branch of the Service should be in possession of such an excellent small arm, if such a weapon is necessary for gunners at all. The attempt to convert a well-trained garrison artilleryman into a very indifferent infantry soldier, solely for considerations of "sentry go" and martial display at field-days and reviews, is such a time-honored custom that it fails to evoke any surprise or remonstrance from the most ardent of "true reformers." When we consider field artillery in connection with small-arms for their own defence, we cannot so readily dismiss the question; the course pursued in their case seems so full of vacillation and uncertainty. For many years the complement of carbines to a battery of six guns was twenty-four, four being thus at the disposal of each gun detachment. If we omit the mounted non-commissioned officers in charge, this was in the proportion of one to every two men. This number might, under exceptional circumstances, have proved sufficient for an emergency; but without the total abolition of the weapon its most ardent opponent would scarcely demand a reduction in the issue. Modifications in modern tactics, based mainly on the experience of the last great European war, have led to the gradual emancipation of the field artillery from the "escorts" which hampered its movements, and, where composed of infantry, almost wholly deprived it of its mobility. Field artillery, according to the latest theory and pratice, is to show itself practically unescorted in the forefront of the fight. In this position, should it escape annihilation from the oppsing small-arm fire, it is extremely liable to surprise or to the capture of its guns by a rush of infantry under cover, or by a cavalry charge. Such considerations point to the necessity for some last resource, some *pis aller* in the moment of need. The escorts being abolished, surely the gun detachments should be more than ever capable of self-defence; and yet the proportion of carbines has been reduced one-half, two only being alloted to a gun detachment by the latest regulation. The French arm every gunner of a detachment of field artillery with a carbine, nor can we detect any marked slowness or awkwardness in the service of their guns. If it be held that a carbine slung on the body as carried by the French field gunner is liable to hamper a man in his actions, the introduction of a pistol for gunners might prove desirable. When it was the custom to afford guns constant protection in the shape of the escorts now abandoned, it was essential that gunners should devote their exclusive attention to their own distinctive arm. As, however, they are now liable to be called upon to defend themselves in a *melee*, the question naturally suggests itself—are two carbines tightly strapped to a limber, with every regard to neatness, adequate provision for the defence of eight otherwise unarmed men?—*The Broad Arrow.*

On the Difference of the Steering of Steamers with the Screw Reversed when under Full Way, and when Moving Slowly. By Prof. Osborne Reynolds. The author said the fact that the results which had been established by the Committe were so little known to pilots and seamen, besides being likely to excite surprise, would tend to cast a certain amount of discredit, if not on the truth of the results themselves, at least on their importance. It seemed as if nautical men must have formed their opinions from experience, and such was the faith of the English people in the practical that it was very difficult indeed for them to believe that a few landsmen, calling themselves scientific, could teach sailors how to steer ships. So strong was this feeling that it was to collisions they must look in the hope of preventing collisions. This sounded like a bull, but it was perfectly true, for nothing but disasters would awake our rulers to the idea that something was wrong. Fortunately, or unfortunately, such disasters were not wanting. There were the cases of the Ville du Havre and the Loch Earn, in which the collisions were undoubtedly due to the steamers having turned in the opposite direction to that intended. These and other disasters furnish evidence enough of the mistakes which had been perpetrated, and of the importance as well as the truth of the results the Committee had established. He fancied that the ignorance which existed was due to the fact that few seamen had turned a ship under full way with the screw reversed, and contented themselves by arguing as to what must happen in such a case from their experience in manœuvring their ships when moving slowly. Of such manœuvres they had had abundance, but as soon as they had got beyond their experience, they adopted the seemingly obvious, but entirely erroneous opinion that the way of the ship would cause the rudder to act as if she was going ahead in spite of the screw being reversed. He felt strongly that in speaking thus in a town like Plymouth he ran the risk of being looked upon as impertinent. If he were wrong he was impertinent, and no one would feel it more than he should. It was not a pleasant task to point out imperfections, however accidental they might be. Even if one saw the wheel coming off an omnibus, all the thanks he was likely to get for pointing it out to the conductor was to be asked if he could not tell him something he didn't know. Of course they must learn as they went on, and all he, with deference, asked of seamen was to try the experiments for themselves, and then aid the Committee in bringing facts under the notice of the Legislature. Their own interests demanded this, for as things now were, great injustice might be done to the captain, who, in a case of emergency, adopted the very best course to save his ship.

BOOK NOTICES.

Tables for the Determination of Minerals by their Physical Properties. By Persifor Frazer, Jr., A. M. Philadelphia: J. B. Lippincott & Co. For sale by D. Van Nostrand. Price $2.00.

This is a new edition of a work already well known to mineralogical students. The tables are constructed with reference to detecting minerals by aid of such simple apparatus as the worker can carry into the field.

The minerals are first classified into groups depending upon lustre or color. Then against each mineral name are given other properties, as follows, viz., Lustre; Color; Streak; Hardness, Tenacity; Crystal System; Clearage; Chemical Formula; to which is also a *Remark* giving Specific Gravity, and generally a Blowpipe Reaction.

No other set of similar tables contains so much in so small a space.

Woodward's Ornamental and Fancy Alphabets. By George E. Woodward. New York: W. H. Stelle & Co. For sale by D. Van Nostrand. Price 50 cts. per number.

This work is issued in twenty parts, each being a quarto pamphlet.

The merit of the designs we take to be, that degree of distinctness or simplicity which admits of easy and prompt reading. This applies to the complex monograms which are too often offered as ingenious puzzles. The designs in the six numbers before us are elegant and beautifully printed.

WOODWARD'S NATIONAL ARCHITECT

in twenty parts contains working plans of residences for city and country, and afford excellent study for young architects. The pamphlets are uniform in size with the alphabets mentioned above. Price 50 cts. each part.

A Treatise on the Use of Belting for the Transmission of Power. By John H. Cooper. Philadelphia: Claxton, Remsen & Haffelfinger. For sale by D. Van Nostrand. Price $3.50.

There is no subject relating to Mechanism upon which the information has been so vague and unsatisfactory as this one of Belting. The need for such a book as this has long been manifest. That the subject has been treated by competent hands in the work before us is abundantly indicated by the papers previously published in a contemporary journal, and by the appreciative comments upon them by practical men.

The contents of the present volume are: Rules and Data for Belting; Methods of Belt Transmission; Cements, Adhesive and Fastenings; Varieties of Belting; Strength of Belting

Leather; Experiments of Briggs and Towne on Leather Belts; Experiments on the Tension of Belts by Morin; Rope Transmission of Power; Frictional Gearing.

The work is to a large extent a compilation from various authorities, selected with judgment, and arranged with much care. Many of the sections are essays from scientific journals. Quite as many are practical formulas and suggestions contributed by mechanical engineers of wide experience.

The illustrations, eighty-three in number, are exceptionally good.

The book deserves a wide circulation.

MANUAL OF SURVEYING, FOR INDIA. Compiled by COL. H. L. THUILLIER, and LIEUT-COL. R. SMYTH. Calcutta: Thasker, Spink & Co. For sale by D. Van Nostrand. Price $21.00.

This treatise, of over eight hundred pages, contains much that is of use only to a beginner, and, unless that beginner is located in India, he may get all that this book affords him in a smaller and less expensive work.

A portion of the work treating of Geodesic Surveying, contains valuable information; but it is only a small fraction of the book.

The volume might be profitably divided into separate treatises, three at least, one of which would be of use or interest only to surveyors in India. The other two would be serviceable to surveyors of other portions of the globe, although unnecessarily voluminous for the purpose.

The maps, typography and illustrations are all excellent.

THE THEORY AND ACTION OF THE STEAM ENGINE. By W. H. NORTHCOTT, C. E. London: Cassell, Petter & Galpin. For sale by D Van Nostrand. Price $3.50.

The book is written for practical men, and contains the rules and formulas necessary for the successful working of steam motors. For the theory of Thermodynamics the reader is referred to special treatises on that subject.

The author presents very fairly and clearly so much of the theory of the generation and use of steam, as is in constant request by practical engineers.

It may be read with profit by those students desirous of acquiring a knowledge of this theory who have not the time for such works as Rankine's.

The book is neatly printed, and has a fair supply of illustrations, mostly, however, of indicator diagrams.

PRACTICAL HINTS ON THE SELECTION AND USE OF THE MICROSCOPE. By JOHN PHIN. Second Edition. New York: Industrial Publication Co. For sale by D. Van Nostrand. Price 75 cts.

Such a work as this is needed by every beginner at the microscope, whether he is intending to work or is seeking only amusement. The suggestions in regard to collecting and preparing objects are of a thoroughly practical character, and so plainly given that the merest tyro may follow them.

The sections of the book treating of the construction and use of the various grades of instruments, and their comparative merits, may be read with profit by experienced workers in Microscopy.

The illustrations are numerous and are fairly well executed.

MINUTES OF THE PROCEEDINGS OF THE INSTITUTION OF CIVIL ENGINEERS. London: William Clowes & Sons.

We have received the following papers from the Secretary of the Institution, Mr. James Forrest.

The Whitson Pumping Station. By Thomas Sullock Storke, A. I. C. E.

Tunnel Construction. By Arthur Earnest Baldwin, A. K. C.

History of the Modern Development of Water-Pressure Machinery. By Sir William George Armstrong, F. R. S.

Street Tramways. By Robinson Souttar.

Economical Method of Manufacturing Charcoal for Gunpowder. By George Haycraft, A. I. C. E.

RAILWAYS OF NEW SOUTH WALES. Report on their Construction and Working during 1876. By JOHN RAE, A. M., Railway Commissioner. Sydney: Chas. Potter. For sale by Van Nostrand. Price $4.00.

This is a voluminous Government report, abounding in statistical information of much value. It is a model of systematic arrangement, and is, therefore, a valuable addition to the literature of railway systems and their working.

The economical results are compared with the estimates of railways in the other Australian Colonies, as well as with those of the United Kingdom, and of this country.

In addition to the details common to such reports, profiles of the lines are given, together with a map of the Colony exhibiting the entire railway system.

We will present an abstract of this valuable report in our next number.

ASSOCIATION DU CHEMIN DE FER SOUS-MARIN ENTRE LA FRANCE ET L'ANGLETERRE. Rapports présentés aux membres de l'Association. Paris.

This is a collection of reports by different members of this "Association," presented to the Assemblée Génerale on the sixth of June last.

M. Michel Chevalier, President of the Permanent Committee, is also acting President of the Association, and it is to the politeness of this gentleman that we are indebted for an early copy of the Rapports.

The publication includes:

1st. A Report on the Actual Condition of the Enterprise.

2d. A Financial Report on the work of 1876.

3d. Report of M. A. Lavalley, on the Results of Surveys, Soundings, etc.

4th. Report of M. Larousse, Hydrographic Engineer.

5th. Report on the Submarine Geological Explorations, by M. M. Potier and Lapparent, Engineers of Mines.

6th. Report on a Boring at Sangatte, by M. M. Potier and Lapparent.

The reports are abundantly illustrated with plans and profiles of the finest description. In a future issue we will present abstracts of 1st, 3d, 4th, and 5th Reports.

A MANUAL OF RULES, TABLES AND DATA FOR MECHANICAL ENGINEERS. By DANIEL KINNEAR CLARK. New York: D. Van Nostrand. Price $7.50

Books, like nearly all other things, may be divided into three classes: good, bad and indifferent. Mr. Clark's new manual may unhesitatingly be ranked in the first class. In general character it resembles such "pocket books" as Haswell's and Trautwine's, but instead of being a pocket book it is a large volume of 984 pages which measure 6 x 9 inches. It is printed on heavy tinted paper, and therefore it is a thick book, not at all resembling a "pocket book," In its general character it resembles Trautwine's book more than any other; but that, as most of our readers know, is a pocket book for civil engineers, whereas Mr. Clark's manual is intended for mechanical engineers. The two books resemble each other in the fact that instead of containing only rules, tables and memoranda, they each have what may be called dissertations on the subjects treated. Trautwine's book is, however, printed in very small type and on thin paper, so that it is comparatively small, though somewhat larger than the usual pocket book. Mr. Clark's manual refers to a very different range of subjects, as it treats of mechanical and not civil engineering, although, of course, some subjects are common to both. Perhaps no better idea can be given of the character of Mr. Clark's work than to quote the titles of the chapters. These are: "Geometrical Problems," "Mathematical Tables," "Weights and Measures," "Money—British and Foreign," "Weight and Specific Gravity," "Weight of Iron and Other Metals," "Fundamental Mechanical principles," "Heat," "Steam," "Mixture of Gasses and Vapors," "Combustion," "Fuels," "Application or Heat," "Strength of Materials," "Strength of Elementary Constructions," "Work or Labor," "Friction of Solid Bodies," "Mill Gearing," "Evaporation Performances of Steam Boilers," "Steam Engine," "Flow of Air and Other Gasses," "Work of Dry Air or Other Gas Compressed or Expanded," "Air Machinery," "Flow of Water," "Water Wheels," "Machines for Raising Water," "Hydraulic Motors," "Frictional Resistances."

The two chapters on "Geometrical Problems" and "Mathematical Tables" might not inaptly be called mathematics for practical men. They give, in fact, a summary of practical geometry, mensuration and trigonometry in such a concise form as every man actively engaged in engineering has often occasion to use and refer to. What he then wants is a book in which can be found in the shortest possible time exactly the fact or the theory he wants to use. For such persons these chapters are admirably adapted.

The chapters on "Weights and Measures" and on "Money" show the confusion which exists the world over regarding these matters. The advocates of the metric system have here a text from which they could preach a very forcible sermon. The portion referring to wire gauges is almost comic in its diversity. There are six different English wire gauges given. Besides these there are, we believe, several which originated in this country. So great is the confusion on this subject that, to express a dimension as such a number wire gauge would not mean anything definite, unless it was stated what special gauge was meant.

The tables of weights and specific gravity are unusually full. Of course, any careful examination of these tables is quite out of the question in a review of this kind, but from an examination of the tables and data it would seem as if almost any information which might be desired could be found under its appropriate head.

A similar remark might be made of the chapter on "Fundamental Mechanical Principles" that was used to describe the chapter on "Geometrical Problems." If the one is mathematics for practical men, the other may be called mechanics for the same class. The explanation of such matters as the "centre of gyration," "work accumulated in moving bodies," and "work done by percussive force," are remarkably clear and concise.

In the chapters on "Heat" and "Steam" will be found a condensation of all the latest information relating to these interesting subjects. Our remarks would assume the character of a table of contents if we undertook to give an idea of the scope of these chapters. They give in a very lucid and condensed form the substance of the information contained in larger treatises on these subjects, so that information relating to them is easily accessible with an expenditure of a very little time. The same thing is true of the chapter on "Combustion."

The subjects of "Fuels" is discussed very fully, and information is given regarding coal and other fuel from all parts of the world. To our shame be it said that no other information, worthy of the name, was procurable concerning American coals excepting that contained in Prof. Johnson's report made in 1843–44. The experiments which were the basis of his report were very complete, but since then large coal fields have been opened, and many qualities which were then unknown are now extensively used.

It is, in fact, difficult to describe the book without becoming monotonous in reiterating the same commendation of every part. Mr. Clark's plan seemed to be to take up the subjects treated and then seek all the attainable information relating to them and put it into as condensed a form as possible. The chapter on the "Applications of Heat" illustrates this, as all the others do. The transmission of heat through solid bodies is fully treated, then the heating and evaporation of liquids through metallic surfaces, the cooling of hot water in pipes, the condensation of steam in pipes, warming and ventilation, evaporation in the open air, desiccation and heating of solids.

The discussion of the "Strength of Materials" occupies 132 pages, derived chiefly from English sources, and is undoubtedly very valuable, but is deficient in not giving the results of German investigations, like those of Woehler, and others made in this country by Prof. Thurston. By representing graphically the action of material under strain, and not only its ultimate strength is determined, but other qualities, such as elasticity, ductility, etc., are clearly shown. The "autographic" method by which the material is made to draw an outline of its own character, promises, or rather has established, a new era in the investigation of the quality of materials.

The discussion of the "Strength of Elementary Constructions" contains the results of experiments made to determine the strength of rivet joints, columns, beams, railway rails, steel springs, ropes, chains, bolts and nuts, stay bolts, cylinders, tubes, pistons and boilers. There is also a brief discussion of the strains of frame work, such as cranes, girders, roofs, etc.

The treatment of the "Friction of Solid Bodies" is comparatively brief, and anyone in search of any complete information on this subject will look in vain here, as he will everywhere else, for it, so that the reader will probably renew his hope that the coming investigator who is to make this subject clear will soon begin his labors.

"Mill Gearing" has 40 pages devoted to it, which forms a very useful compendium of that art. Besides discussing the subjects usually found in the treatises on this subject, much information of comparatively late date, such as frictional wheel-gearing, rope gearing, etc., are discussed.

Probably no portion of the book will have more interest for railroad mechanical engineers than that which refers to the evaporative performance of steam boilers. The information which is condensed here is spread over a very wide field, and to the general reader is therefore inaccessible. Among other things it contains a report of the experiments of Woods and Dewrance in 1842 to determine the evaporative efficiency of locomotive boiler tubes. These same experiments were repeated by M. Paul Havrez in 1874. In both cases it was found that the first six inches of the tubes next the fire-box did more work than the remaining 60 inches of tube. The record is, however, provokingly silent with reference to the important point whether the experiments were made with a forced blast, which is the condition under which a locomotive boiler is always worked, or whether they were made with the draft of an ordinary chimney.

The following conclusion, stated by the author in this chapter, every master-mechanic should lay to heart and hang up in his office in the form of an illuminated text. It is that "*There can never be too much heating surface, as regards economical evaporation, but there may be too little; and that, on the contrary, there may be too much grate-area for economical evaporation, but there cannot be too little, so long as the required rate of combustion per square foot does not exceed the limits imposed by physical conditions.*"

The limits of this notice will not permit any careful examination of the chapters on the "Steam Engine" and on the "Expansive Working of Steam" "Flow of Air," "Air Machinery," "Water Wheels" and "Frictional Resistances." The much-disputed subject of the economy of the compound engine is discussed quite fully in the chapter on the steam engine, but to examine it critically would require more time and room than can now be given to it.

Altogether Mr. Clark's book may and should be commended very highly. It is vastly the best book of the kind ever yet published. It contains no mathematics with the exception of a little simple algebra and can be comprehended by the ordinary reader who is without the accomplishments of a knowledge of the higher mathematics. The book, it is quite safe to predict, will become the office companion of nearly every mechanical engineer who takes any pride in his profession.—*R. R. Gazette.*

L'AIR COMPRIME ET DES APPLICATIONS, PRODUCTION, DISTRIBUTION, ET CONDITIONS D'EMPLOI. Par M. A. PERNOLET, Ingenieur. Paris: Dunod, 1876. For sale by D. Van Nostrand. Price $8.00.

Scarcely used about thirty years ago, compressed air is to-day rightly regarded as a power transmitter and storer of the very first importance. The oldest application of compressed air is to the diving bell, mentioned by Aristotle in his "Problems." The diving dress was only invented about fifty years ago. Sinking bridge caissons into sandy bottoms, sinking wells by the use of compressed air, are other modern statical applications. As examples of what may be termed dynamical applications may be cited winding engines for working shafts and inclines; drills; pumping engines; and coal-cutters in mines and collieries. The cutting of the long tunnels of the Mont Cenis and St. Gothard, and of the Hoosac Tunnel in the United States, would have been next to impossible without the use of compressed air. The first-named work no doubt gave the impetus to this branch of engineering. In America compressed air is sometimes used for driving machinery in confined workshops. In the St. Gothard the locomotives for taking out the rubbish, when in the tunnel, are worked by compressed air. Inventors in Scotland and in France are now working at tramway locomotives driven by compressed air, and the Whitehead torpedo constitutes a sufficiently noteworthy invention. In ventilating large confined spaces, for instance, jets of compressed air are used to induce subsidiary currents by means of some form of injector. In London, Paris, Berlin, and Vienna compressed air is applied in the pneumatic despatch tubes; and air is the motive power of several forms of brakes now competing for the favor of the railway companies. Its purely physical properties, such as its elasticity, are employed in signaling, and its capacities for taking up heat and doing work are made use of in cooling and ice-making.

The treatise is divided into six parts. The introduction treats of the history, applications, advantages, and disadvantages of compressed air. The first division examines the general theoretical conditions of its production, distribution, and use; the second relates to the production more especially, with descriptions of the construction and duty of most of the compressors actually in use, comparing them together at the same time. We then come to an examination of the modes of distributing compressed air, the piping, reservoirs, and accessory apparatus, to its actual practical applications; while the fifth and last division treats on the economical conditions involved in its employment. At the end of the description of each machine the excellent plan is adopted of giving in a short recapitular table the principal dimensions and numerical data of each machine and its engine.

He divides compressors into four principal classes (1) low, (2) medium, (3) high, and (4) very high pressure, according to the pressures, (1) less than one atmosphere, (2) from 1 to 3 atmospheres (3) from 3 to 7, and (4) above seven atmospheres, at which they work. This is quite correct, as it is the working pressure which most influences the physical conditions of working and the suitable mode of construction. Compressors working at moderate pressure supply air for diving, and to the caissons of bridges; Bessemer blowing engines, propelling air at about 20 lb. above the atmosphere, the pressure required to counterbalance the ferrostatic column, come under the same category. In the description of the compressors built by Cail and Co., for the contractors of the Kehl bridge over the Rhine, and set up in order to inject compressed air to equilibrate the head of water below which the men were working in the caissons, it is stated that these compressors offer the first instance of an injection of water. This is a mistake, and we could point to more than one British specification, several years anterior to that date, embodying this process; and not merely, as in this case, injecting it into the air-chest, but directly into the compressor.

The title of this work does not do full justice to its contents. In the first place, information is given not merely relating to the compression, but also, as should be, to the expansion, in order to do work, of air; and, in the second, not merely the use of air is treated on, but also of other gases, such as lighting gas for portable use, and of carbonic acid gas in the continental beet-root sugar refineries. Without much increasing his volume, the author could well have brought the expansion of air more within the range of his work; or, in other words, have given the few instances extant where rational attempts have been made to re-expand the air, returning it the heat it requires for expansion. Then, not sufficient is said about the means or apparatus adopted for separating the water from the air, when direct inter-contact has been employed to keep down the temperature generated by the compression. This possibly results because there is no recognition whatever by the author of the important physical fact that water absorbs very considerable volumes of air—volumes dependent on the pressure of the air and the amount of surface of water exposed to the fluid contact, time being also no doubt an important factor. What this book—excellent as it is—lacks is perhaps a more detailed criticism based on actual examination of the machinery while at work. No doubt such information is difficult and costly to get; but several of the machines herein described, and even favorably noticed, are within our own knowledge working most execrably. Nevertheless, we repeat that it is the most important and complete work on the subject, affording an excellent point of departure for further investigation and improvement. What is at present required in the use of compressed air is a considerable diminution in the first cost of obtaining the compressed air by really improving the compressor, and a practical means of working the air at a high rate of expansion without the present attendant losses. It is certainly not too much to say that the air compressor of the future has yet to be invented. —*Engineer.*

MISCELLANEOUS.

PROF. MORITZ has published an account of a thermometer he invented about thirteen years ago, which he now thinks might be of utility in meteorology. It is a nearly circular band of silver and platinum, fastened at one end, and at the other moving a mirror, the reading being taken from a scale on the mirror, as in a magnetometer.

AT a recent meeting of the Dumfries and Maxwelltown Water Commission, the petition of the inhabitants of Lochrutton, complaining of their sewage running into the loch for lack of drainage, and polluting their water supply as well as that of Dumfries, was considered; and it was resolved to intimate to the local authorities of the parish that unless they rectify the evil, proceedings will be taken against them.

THE provisional order of the Local Government Board for the formation of a united drainage district for Birmingham and the Tame and Rea district has been read a first time in the House of Lords, and copies have been supplied to the various authorities embraced in the scheme. It is understood that after the second reading a week's notice has to be given before it is considered by the committee to whom it will be referred, in order to give time for opposition to the measure.

A GLASS has lately been made with phosphate of lime, by M. Sidot. He states that it is perfectly transparent and very refringent (its index of refraction is 1.523, that of common glass being 1.525); and it can be worked like ordinary glass. It does not, like ordinary glass, dissolve all metallic oxides, but it dissolves very well oxides of cobalt and chromium. It is attacked by boiling acids, as also by potash; it is not attacked by hydrofluoric acid; and this property may render it valuable in employment of telescope glasses, for workmen who are exposed to these vapors, and who have to work in the art of engraving on glass.

VAN NOSTRAND'S
ECLECTIC
ENGINEERING MAGAZINE.

NO. CVIII.—DECEMBER, 1877.—VOL. XVII.

A NEW HYDRO-DYNAMOMETER.

By M. DE PERRODIL.

Translated from "Annales des Ponts et Chausées" for Van Nostrand's Magazine.

This instrument which we have termed a hydro-dynamometer is employed to measure the velocity at any point of a liquid current. It determines directly the pressure exerted by the liquid against an obstacle to the current. This pressure is equilibrated by the torsion of a metallic rod. A graduated circle is used to measure the amplitude of the torsion.

The instrument consists of a frame which is to be placed vertically in the stream, and having a height therefore equal to the depth of the current. The two standards of the frame furnished with cross rods AB, ST, UV, XY and CD are united at the top in a circle in a vertical plane, and support a graduated horizontal circle EF. The frame is movable about its axis; to insure this it is hung upon a pivot at G which is attached to an arm GI extending from the tube KL. This tube is held by a clamp screw to the staff MN. Other horizontal arms parallel to GI are fixed to the tube and serve as guides to the axis GR.

This rod at the axis is the torsion spring of the instrument. It is a cylindrical rod Rz of small diameter, set at R upon the lower cross bar and passing freely through the upper bars XY, UV, etc., of the frame as well as the graduated circle EF, and terminates at z above the circle by a horizontal needle whose extremity passes over the divisions of the circle when the rod is twisted by torsion. The supporting arm G which holds on its upper face the point of the pivot upon which the frame turns, is furnished on the lower side with a little hollow cylinder, in which the upright torsion rod terminates. This little cylinder is furnished with a clamp screw with which to regulate the bearing of the rod RZ.

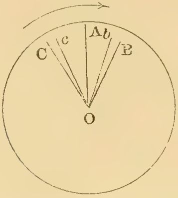
The force to be measured is received by a little disk *a*, Fig. 3, which is fixed in the plane of the frame.

In order to observe the velocities at different depths it is necessary to bring the disk to each point. To accomplish this the staff MN must have sufficient length to permit the tube to traverse a distance equal to the depth. To observe therefore surface velocities conveniently, the disk and arm may be attached to a higher part of the rod.

To use the instrument, set the staff in the bottom of the stream to be measured, and bring the tube to proper height. Bring the arms which support the rod parallel with the current and on the upstream side of the tube, (see Figs. 2, 3 and 4).

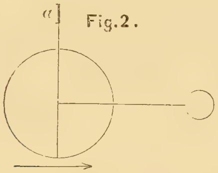
The disk presents its edge to the stream, and the rod experiences no torsion. The needle points to 0 of the graduated scale.

Fig. 1.



It is necessary to adjust the instrument by hand, so as to bring the plane of the frame and disk to be perpendicular to the direction of the current. This is done only by calling into action the torsion spring. The amplitude of the torsion is then measured by the scale.

Fig. 2.



Observation proves that the velocity of the water at any point is not constant. It varies between limits more or less widely removed. These variations probably follow the law of a periodic function of the time. It is furthermore noticed that these limits are wider as the velocity is greater. The result is, the needle oscillates continually. It is difficult to measure the limits of these oscillations with the eye. To remedy this difficulty, a screw *b* is fixed to the upper end of the rod, and the plane of the frame is brought to its position with reference to the current. The needle is thus held in a fixed position, while the frame oscillates about its axis. The hands of the observer are left free and the motions of the graduated arc can be observed with a reading glass, and the extreme limits of vibration noted. The mean of the two extreme angles is taken as the one measuring the torsion due to the current.

The mean position of the needle thus observed is free from errors arising from friction. For suppose OA, (Fig. 1) to be this position, and OB the position of the needle when the velocity of the water is a maximum, then the pressure of the water is greater than the tension of the spring by a quantity equal to the friction. As the velocity decreases the needle recedes from OB to OC, and in this latter position the tension of the spring is greater than the pressure of the current by a quantity equal to the position.

Fig. 3.

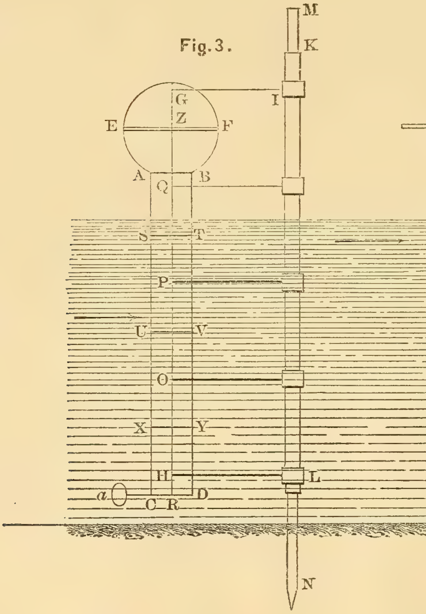
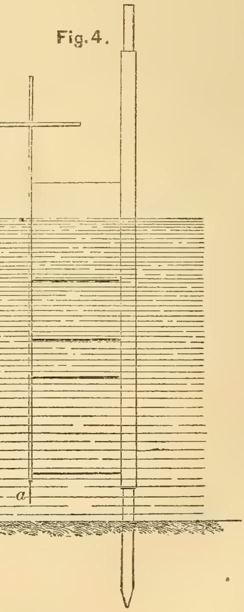


Fig. 4.



The mean angle therefore corresponding to the position OA will give a measure of the torsion free from friction.

Another source of error is eliminated by the mean reading, for instance, if equilibrium is established between the pressure of the current and the tension of the spring, augmented by friction, when the needle is in the position OB; then by virtue of the acquired velocity it should move beyond this limit. If OB is the extreme limit of the oscillation, then equilibrium between these forces really exists for some other point nearer OA or O*b*. On the other side for similar reason the equilibrium exists for some point as O*c*. In the positions O*b* and O*c*, the velocities and consequently the living forces may be supposed equal by reason of the probable symmetry of the oscillations; so the angles CO*c* and BO*b* are equal, and the mean of OB and OC is also the mean of O*b* and O*c*.

The theory of the instrument is given as follows:

Let r be the radius of the rod which serves as a torsion spring, l the length of this rod from its extremity at the lower cross-bar to the center of the needle at z (Fig. 3); a the angle of torsion of this rod, that is to say the angle of rotation of its section at the level of the needle, relative to its lower fixed section.

The molecular forces developed in the metal by this torsion form a couple, of which the moment M is given by the following equation:

$$\frac{a}{l}=\frac{M}{GI} \tag{1}$$

in which G is the coefficient of elasticity of torsion and I is the moment of inertia (polar) of the section of the rod. This moment referred to an axis perpendicular to the plane of the section at its center, the section being circular is

$$I=\frac{\omega r^4}{2}.$$

Let R be the radius of the disk, b its lever arm, measured from its center to the center of the rod.

The pressure of the water which is supposed proportional to the area of the disc and to the square of the velocity of the water is

$$F=K\frac{1.000}{2g}\omega R^2v^2.$$

K is a constant coefficient. This force being equated with M gives

$$M=Fb \qquad \text{whence}$$

$$K=\frac{1.000}{2g}\omega R^2v^3l=GI\frac{a}{l}. \tag{2}$$

The only variables are a and v. The relation between them is expressed

$$y \quad v=c\sqrt{a}.$$

The coefficient c is determined by experiment. Such values are to be compared with the theoretic value deduced from eq. 3. This is

$$C=\sqrt{\frac{gG\omega r^4}{1.000Kl\omega r^2b}}=\frac{r^2}{R}\sqrt{\frac{gG}{1.000Klb}} \tag{3}$$

In the first instrument made r was made$=0.0015$, $l=2$ meters. The spring was of brass. The value of b was known for copper only. Claudel (L'Aide Memoire) gives for this metal $G=4.3666+10^9$.

For a value of $a=1^\circ$ equation 1 gives $M=0.000305$ kilogrammeter. By using monogrammeter for a unit of measure we have $M=0.305$ grammeters.

By bringing the instrument to a horizontal position, and placing known weights on the extremity of the arm, it has been found that a torsion of one degree corresponds to a moment of 0.264 grammeters. But the angle of torsion as observed above must be proportional to the moment of the couple producing it. It appears therefore that for the brass torsion rod of this Hydro-dynamometer $G=3.77\times10^9$.

Introducing this value in equation (3), making

$$r=0.0015,\ g=9.81,\ l=2.00$$

we shall have $c=\dfrac{0.097}{\sqrt{KbR^2}}$.

The instrument is furnished with three discs of different sizes and with different lever arms; as follows:

$R=0.010 \qquad b=0.05$
$R=0.032 \qquad b=0.10$
$R=0.064 \qquad b=0.20$

These give for bR^2 the values respectively

0.0000128, 0.0001024, 0.000819.

The three corresponding values of c are:

$$C_0=\frac{2.7}{\sqrt{K}}\quad C_1=\frac{0.96}{\sqrt{K}}\quad C_2=\frac{0.34}{\sqrt{K}}$$

With the smallest disc we get

$$v_0=\frac{2.7}{\sqrt{K}}=\sqrt{a}$$

Taking a equal to the arc of one degree=0.0174 we have

$$v_0=\frac{0.354}{\sqrt{K}}$$

With the second disc we should have for the same torsion angle.

$$v_1=\frac{0.121}{\sqrt{K}}$$

and with the third disc

$$v_2=\frac{0.442}{\sqrt{K}}$$

The coefficient K for a thin circular plate is found to be equal to 1.12. The values of velocity then become, for a torsion of one degree

$$v_0=0.335\quad v_1=0.115\quad v_2=0.042$$

For an angle of torsion of a degree these values above must be multiplied by $\sqrt{a^\circ}$.

This angle of torsion ought not to exceed 90°, the limit of elasticity of the metal about 0.00135; the greatest relative displacement of the molecules is at the surface of the cylindrical rod. This is equal to $\frac{ra}{l}$ at all points of this surface and this may be, at most only equal to 0.00135. Then $a=180$.

This length of arc corresponds to about 104°. We may then safely employ the instrument with a torsion of 90°. The velocity of current measured, therefore, should not be greater than 0.333 $\sqrt{90}$ or about three meters with the smaller disc, 1.09 meters with the second and 0.40 with the larger one.

In a series of observations made on the surface current of a canal, whose velocity had been determined by floats, it was found that the smaller disc gave a result higher than that of the formula in the ratio of 1.12 to 1. The coefficient K taken equal to 1.12 should be made equal to $1.12\times\overline{1.12}^2=1.40$.

Experiments upon the velocities in a canal at Hers are in progress to determine more exactly the value of K for each of the discs.

The instrument doubtless permits the accurate measure of the pressure exerted by a current of water at a moment that it has a given velocity, and solves therefore the problem of the measure of the velocity of curents, either by direct application of the formula $v=m\sqrt{a}$ or by aid of an empirical table if m proves not to be a constant quantity.

THE HISTORY AND THEORETICAL LAWS OF CENTRIFUGAL PUMPS, AS SUPPORTED BY EXPERIMENT, AND THEIR APPLICATION TO THEIR DESIGN.

By the Hon. R. CLERE PARSONS, B.A., B.C.E.

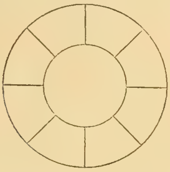
From Proceedings of the Institution of Civil Engineers.

Centrifugal pumps are by no means a modern invention; the crude idea of which probably dates as far back as the middle of the last century, when the mathematician Euler brought out a primitive form of centrifugal pump, an account of which he published in the Proceedings of the Academy of Berlin for 1754. This pump is referred to by M. Combes, the eminent French engineer, and also its theory, as deduced by Euler; but, as it has never come into practical use, it has probably failed, like most of the mechanical inventions of that great mathematician. From that period many rotatory and centrifugal pumps were invented, principally by French engineers, but none of them seems to have yielded even a reasonably good efficiency. The first mention of a centrifugal pump at all to be compared to those of the present day is in the year

1830, when one was erected by Mr. McCarty in the Naval Yard at New York, and some improvements upon this machine were patented by him in the following year. No experiments upon this pump seem to have been published, consequently it is uncertain whether it yielded a good efficiency. The next epoch in the history of the centrifugal pump is the Exhibition of the year 1851, when the late Mr. Appold achieved the great success with his pump of trebling the efficiency obtained by any other exhibitor. Mr. Appold, by making numerous experiments, at length determined that the efficiency mainly depended upon the form of the blades in the fan, and, further, that the best form was a curved blade pointing in the opposite direction to that in which the fan revolved (Fig. 1). Two other forms of fan which he tried were, one with straight radial blades, and another with straight blades inclined at an angle of 45° to the circumference (Figs. 2, 3). Both these fans yielded efficiencies far inferior to the one with curved blades.

Fig. 1.

Fig. 2.



Another detail which effects the efficiency, but not to the same extent as the form of the blades of the fan, is the form of the casing surrounding the fan. The old theory which Morin, Appold, and many others held, and which is still advocated in some recent books, viz., that as long as the casing outside the fan is large enough it is immaterial of what shape it is, can be proved to be false both by theory and experiment. In January 1875 Mr. Anderson, M. Inst. C. E., of the firm of Messrs. Eastons and Anderson, deputed the Author, in company with Mr. Hesketh, Stud. Inst. C. E., to make some experiments upon centrifugal pumps, both with a view to determine the laws which regulate their discharge, and at same time, if possible, to improve their efficiency. The first experiments were made upon a pump whose suction and discharge pipes were ten inches in diameter. The fan was fourteen inches in diameter, and the casing surrounding the fan was circular, as is shown in Figs. 4, 5.

Fig. 3.

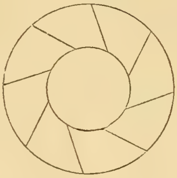


Fig. 4.

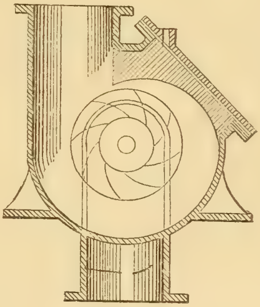
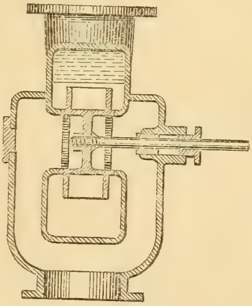


Fig. 5.



This pump was driven by a single-acting engine working directly on to the shaft of the pump. The water was raised by the pump from a large cast-iron tank, and discharged back into it through a measuring tank. In the bottom of the measuring tank was a hole in a thin sheet-iron plate, and by the head of water maintained over this hole the discharge was calculated by the formula

$$Q=0.62\times A\times\sqrt{2gh}\text{ cubic ft. per second;}$$

where A=area of hole in square feet,

h=head of water in ft. over hole.

The revolutions per minute of the fan were measured by a counter attached to the shaft, and the lift was determined by means of a staff fastened to a float in the lower tank. The staff was graduated in feet and decimals of a foot, and was applied to a water-gauge glass connected with the discharge-pipe. In order to take account of the residual velocity of the water in the discharge-pipe in estimating the lift, the tube with which the water-gauge was connected had a bend whose extremity met the water flowing in the discharge-pipe; consequently the water in the gauge-glass stood at the same height as the water issuing from the discharge-pipe. The engine was supplied with steam from a portable boiler, into which the feed-water was injected by a hand-pump. During an experiment the speed of the engine was regulated by an observer, so as to maintain a constant head of water in the measuring tank, and consequently insuring a uniform discharge from the pump. An experiment was continued until thirty gallons of water were evaporated in the boiler, and the time required to accomplish this was accurately noted. Then the number of lbs. of water raised by the pump per minute, divided by the number of lbs. of water evaporated in the boiler per minute, is proportional to the efficiency of the pump. These experiments were repeated for different discharges, and tabulated in the following form:

TABLE 1.

Gallons discharged per Minute.	Lift in Feet.	Foot-lbs. raised per Minute.	Foot-lbs. raised per lb. of Water evaporated.	Revolutions per Minute.	Remarks.
..	5.667	..	..	305	Appold's fan in circular casing.
745	6.000	44,886	7,779	363	
879	6.250	54,937	8,244	380	
989	6.666	65,936	11,883	393	
1,153	7.000	80,710	11,385	416	

The casing round the fan was next altered from the circular to the spiral form, as is indicated by the dotted lines in Fig. 4, by fitting wooden blocks inside; and a fresh series of experiments was now made under circumstances resembling the former set as much as possible. The following are the results, tabulated in a similar form:

TABLE 2.

Gallons discharged per Minute.	Lift in Feet.	Foot-lbs. raised per Minute.	Foot-lbs. raised per lb. of Water evaporated.	Revolutions per Minute.	Remarks.
..	5.750	..	..	320	Rankine's fan in spiral casing.
577	6.500	37,505	8,960	346	
746	6.925	51,600	10,809	363	
878	6.750	59,265	11,264	368	
999	7.085	70,029	12,088	387	
1,150	7.750	89,125	13,248	403	
1,288	8.333	107,329	15,996	423	

Thus, by comparing the first and second tables, it will be noticed how greatly the spiral casing has improved the efficiency, and also increased the discharge, the boiler pressure remaining the same. With the same casing, but another fan, designed on the principles laid down by Rankine in his "Applied Mechanics," and also advocated by Glynn in his treatise on Water Power, some experiments were made similar to the preceding ones.

The following tables show the results of these experiments:

TABLE 3.

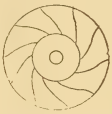
Gallons discharged per Minute.	Lift in Feet.	Foot-lbs. raised per Minute.	Foot-lbs. raised per lb. of Water evaporated.	Revolutions per Minute.	Remarks.
..	5.500	..	..	300	Rankine's fan in circular casing.
578	5.925	34,200	7,203	316	
741	6.167	45,695	8,556	335	
880	6.333	55,733	8,377	348	
993	6.583	65,372	10,748	355	

TABLE 4.

Gallons discharged per Minute.	Lift in Feet.	Foot-lbs. raised per Minute.	Foot-lbs. raised per lb. of Water evaporated.	Revolutions per Minute.	Remarks.
..	5.416	..	..	300	Rankine's fan in spiral casing.
580	6.333	36,731	9,675	324	
743	6.667	49,528	10,857	334	
879	7.000	61,530	11,692	343	
996	7.333	73,036	12,954	353	

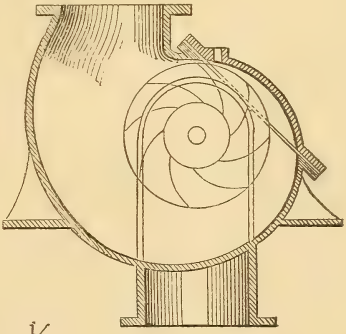
These results prove that Rankine's fan is far inferior to that of Appold. The blades of this fan were for half their length, from the center outwards, similar to those of Appold; but for the remaining half of their length they curved forwards in the direction in which the fan revolves, and ended in radial tips (Fig. 6). This method of estimating the efficiency of a pump is by no means an accurate one; but in altering a pump it is a convenient way of determining whether the alteration has proved a success or the reverse.

Fig.6.



A new casing was now designed, of a spiral form (Fig. 7), and a fresh series of experiments undertaken. These experiments were of a much more elaborate nature than those just described, and as it may make the deductions from them more easily understood, their general arrangement will be described. In order, as far as possible, to insure a constant lift, the pump was supported on the side of a large barge, and the engine for the purpose of driving it was placed in the bottom. The power transmitted to the pump was estimated by the dynamometer used by Messrs. Eastons and Anderson at the Royal Agricultural Society's shows. The method of measuring the amount of water raised by the pump described in the former experiments was repeated in this instance; the water

Fig.7.



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being raised from the Thames, in which the barge was floating, and discharged back into it through the measuring tank. The lift was measured by taking the difference of the levels of the water in the river and that in the discharge-pipe, as shown by two gauge-glasses. The revolutions of the pump were indicated by a counter attached to the pump-shaft. A number of experiments were made upon this pump, and the results tabulated as follows:

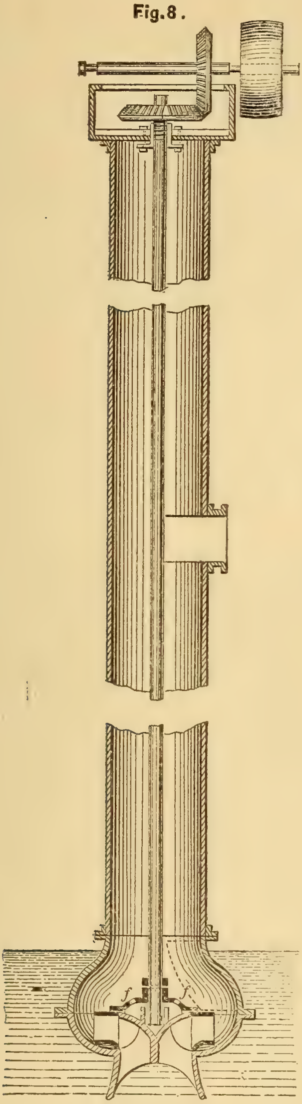
Table 5.

No. of Experiments.	Gallons of Water discharged per Minute.	Lift in Feet.	Foot-lbs. raised per Minute.	Foot-lbs. indicated per Minute.	Revolutions per Minute.	Efficiency per Cent.	Corrected Efficiency per Cent.
1	1,012	14.67	148,461	298,438	392	49.74	58.57
2	1,108	14.70	162,875	317,158	394	51.35	59.99
3	1,197	14.65	175,364	332,136	395	52.80	61.08
4	1,280	14.70	188,160	343,754	398	54.74	62.99
5	1,350	14.75	199,128	357,194	399	55.75	63.78
6	1,431	14.75	211,073	374,954	400	56.20	63.95
7	1,501	14.70	220,650	388,897	402	56.69	64.16
8	1,568	14.75	231,280	404,737	403	57.01	64.29
9	1,630	14.80	241,240	409,612	404	58.90	66.19
10	1,695	14.75	251,987	419,790	405	60.17	67.18
11	1,753	14.80	259,450	435,630	406	59.42	66.39
12	1,012	17.40	176,088	370,458	424	47.53	54.06
13	1,108	17.20	190,576	388,316	425	48.97	55.56
14	1,197	17.20	205,884	404,156	427	51.09	57.33
15	1,280	17.30	221,440	417,214	428	53.08	59.51
16	1,350	17.30	233,550	433,054	429	53.93	60.19
17	1,431	17.40	248,994	447,552	431	53.63	61.86
18	1,501	17.40	261,174	460,512	432	56.71	62.47
19	1,568	17.40	272,832	471,552	433	57.86	63.97
20	1,630	17.60	286,880	479,810	434	59.79	65.98
21	1,695	17.60	298,310	486,050	435	61.37	67.64
22	1,753	17.60	308,528	494,210	436	62.43	68.68
23	1,012	11.81	119,517	238,603	..	50.09	..
24	1,197	11.80	141,246	268,970	..	52.51	..
25	1,355	11.83	159,681	297,829	..	53.61	..
26	1,501	11.83	177,568	321,918	..	55.16	..
27	1,630	11.92	194,196	341,127	..	56.93	..
28	1,753	12.00	210,360	357,007	..	58.92	..
29	2,029	12.33	250,175	416,957	..	60.00	..
30	2,301	12.12	278,881	463,268	..	60.13	..
31	2,544	12.17	309,604	503,759	..	61.46	..
32	2,765	12.17	336,500	553,346	..	60.80	..
33	2,933	12.75	373,867	583,870	..	64.04	..

Some experiments upon a vertical-spindle centrifugal pump will now be described.

The shaft or spindle in these pumps is vertical, as is shown in Fig. 8, and the fan revolves in a horizontal plane. Outside the fan is a circular casing attached to the bottom of a wrought-iron tube. The water enters the fan on one side only—in this case below—and is discharged into the casing; from thence it is carried in the wrought-iron tube to the height required, and is discharged through the orifice in the side. The wrought-iron tube both performs the office of a discharge-pipe, and also serves as a support for the bearings of the vertical shaft. Above the fan, and attached to the casing, are six guide-blades, shown in section in Fig. 10 and dotted lines in Fig. 8, which receive the water escaping from the fan with a high tangential velocity, and conduct it up the vertical tube. In order to render the explanation of this pump more lucid the working parts of the fan are shown in black when they occur in section, whilst the stationary parts of the case are in-

Fig. 8.



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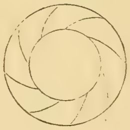
dicated in the ordinary way. In this form of pump the vertical pressure upon the fan arising from the column of water over it is entirely removed: *f f* are two holes, one inch in diameter, in the top of the fan, which admit the water into the space between the support of the center bearing and the fan, and consequently the pressures on both sides of the fan are equalised. The Appold fan, used in the experiments upon this pump, is shown in Fig. 9, and the one made upon Rankine's principle was similar to that in Fig. 6. The pump was placed on the side of the barge in the same position as the pump last described, and the experiments were carried out in an exactly similar manner, and the results obtained are tabulated as follows:

(*See Table on following page.*)

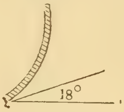
Fig. 9.



These experiments also show the great superiority of Appold's fan over that proposed by Rankine.

The dynamical forces, produced in the centrifugal pump when it is in motion, will now be considered; and afterwards it will be shown how the theoretical investigations are supported by the experiments.

Fig. 10.



There are two totally different conditions in which a centrifugal pump may be situated while it is rotating. One in which it is revolving just fast enough to raise the water up to the discharge-pipe, and no further; and another in which it is revolving slightly faster, and discharging water out of this pipe. In the first case there is only centrifugal force, which is produced by the water in the fan rotating, that maintains the column of water in the discharge-pipe. In the second case this force is still produced, but in addition to it another, which may be called the force of impact, or in other words, the force with which the blades of the fan impinge against the water discharged by the pump.

The centrifugal force in the first case may be calculated as follows:—Assuming that the fan is a cylinder of water; every particle of this water as it rotates exerts a force outwards from the center; consequently the force exerted at the circumference, or that which maintains the head in the discharge-pipe, is the sum of the forces of all the particles from the center of the fan to the circumference.

TABLE 6.

No. of Experiment.	Gallons discharged per Minute.	Lift in Feet.	Foot-lbs. raised per Minute.	Foot-lbs. indicated per Minute.	Revolutions per Minute.	Efficiency per Cent.	Remarks.
1	3,079	8.1	249,399	559,600	206	44.3	Appold's fan.
2	3,343	"	270,783	576,460	208	46.9	
3	3,578	"	289,818	617,850	210	47.0	
4	3,811	"	309,501	674,600	214	49.9	
5	4,252	"	344,412	729,900	217	47.2	
6	4,437	"	359,397	766,830	218	46.6	
7	4,634	"	375,354	745,830	220	50.3	
8	4,810	"	389,610	795,700	220	48.9	
9	5,074	7.1	360,254	777,050	215	45.3	
10	3,079	6.2	190,898	503,000	163	37.9	Rankine's fan.
11	3,343	"	207,266	554,500	165	37.3	
12	3,578	"	221,836	578,000	165	38.3	
13	3,821	"	236,902	610,750	166	38.7	
14	4,042	"	250,604	656,750	167	36.6	
15	4,252	"	263,624	680,600	168	39.6	

This force is given in lbs. per square inch by the formula

$$F=\int_{o}^{R}\frac{pxdx}{g}\omega^2 \quad . \quad . \quad . \quad (2)$$

Integrating this expression

$$F=\frac{pR^2\omega^2}{2g} \quad . \quad . \quad . \quad (3)$$

p=weight in lbs. of a column of water one square inch in section and 1 ft. long.

R=radius of fan in feet.

ω=angular velocity of fan.

g=dynamical force of gravity.

Now since $R\omega=v$, where v=velocity of circumference of fan.

By replacing $R^2\omega^2$ by v^2 in equation 3 it becomes

$$F=\frac{pv^2}{2g} \quad . \quad . \quad . \quad . \quad (4)$$

Now supposing that the head supported by the fan, while it is rotating with a tangential velocity v, be h, then the pressure at the base of the column is ph, but by the ordinary formula of dynamics

$$h=\frac{v^2}{2g};$$

therefore

$$ph=\frac{pv^2}{2g} \quad . \quad . \quad . \quad . \quad (5)$$

Thus by equations 4 and 5,

$$F=ph.$$

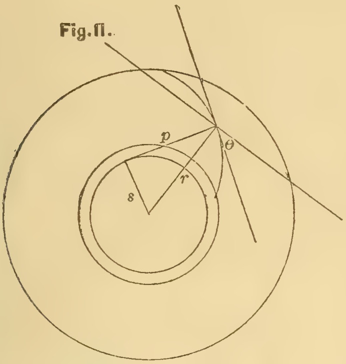
Therefore a fan, when rotating, will support a column of water the velocity due to whose height is equal to the tangential velocity of the circumference of the fan. This conclusion is fully borne out by the experiments, when corrections are made for the axle of the pump displacing a small quantity of water, and thus reducing to a slight extent the centrifugal force. In the case of the vertical-spindle pump (Fig. 8), a large correction has to be made in consequence of there being fixed ribs to support the lower bearing of the vertical shaft, and these, preventing the water contained between them from rotating, reduce the centrifugal force to a great extent.

In the second case, in which the pump is supposed to be discharging water, the calculation of the centrifugal force is a much more complicated process than in the previous instance. Assuming that the blades of the fan are involutes of a circle whose radius is s (Fig 1), and ρ be the radius of curvature of the involute at any point whose distance from the center of the fan is r. Assuming also that a particle of water moves in a curve similar to that of the blades—which is evidently the case. Then calling v the velocity of the water through the fan relatively to it, and ω its angular velocity, the linear velocity of a particle of water is given by the formula

$$V=r\omega-v\cos.\theta \quad . \quad . \quad (6)$$

θ being the angle indicated on the Fig., or $\cos.\ \theta=\frac{\rho}{r}$.

Fig. 11.



Substituting for $\cos.\ \theta$ its value, then

$$V=r\omega-v\frac{\rho}{r}\quad . \quad . \quad . \quad (7)$$

Now the centrifugal force of a particle at the distance r from the center, and moving with the velocity V, is

$$F=\frac{pV\omega}{g}dr\text{*}\quad . \quad . \quad . \quad (8)$$

Putting in for V its value in equation 6,

$$F=\frac{p}{g}\omega\left(r\omega-v\frac{\rho}{r}\right)dr\quad . \quad . \quad (9)$$

Now calling R and R′ the external and internal radii of the fan, and integrating expression 9 between these limits,

$$F=\frac{p}{g}\int_{R'}^{R}\left(r\omega^2-v\omega\frac{\rho}{r}\right)dr\quad . \quad (10)$$

but $\rho=\sqrt{r^2-s^2}$;
substituting this in the last expression,

$$F=\frac{p}{g}\int_{R'}^{R}\left(r\omega^2-v\omega\sqrt{1-\frac{s^2}{r^2}}\right)dr\ . \quad (11)$$

Expanding the second term,

$$F=\frac{\rho}{g}\int\left\{r\omega^2-v\omega\left(1-\frac{1}{2}\frac{s^2}{r^2}-\frac{1}{8}\frac{s^4}{r^4}\right)\right\}dr\quad (12)$$

$$F=\frac{p}{g}\left(\frac{R^2-R'^2}{2}\omega^2-v\omega\left\{R-R'+\frac{1}{2}s^2\left(\frac{1}{R}-\frac{1}{R'}\right)+\frac{1}{24}s^4\left(\frac{1}{R^3}-\frac{1}{R'^3}\right)\right\}\right)\quad . \quad (13)$$

which is the whole centrifugal force exerted at the circumference of the fan in lbs. per square inch. The second force exerted in this case is the force of impact. It is estimated by the maximum tangential velocity generated in the water passing through the fan, which takes place just as it is escaping at the circumference. The reason advanced for the assertion that this force can be estimated by the tangential velocity produced, is that no other force can produce this velocity. The centrifugal force just calculated can only produce a radial force or a radial velocity, but can in no case produce a tangential force or a tangential velocity. This latter force can only be made use of by gradually reducing the velocity of the water issuing from the fan, and this condition is effected by the spiral casing and conical discharge-pipe, which will be referred to presently, and can be easily calculated by multiplying v by cosine θ', the angle made by the blade of the fan at its outer extremity, with the tangent to the fan; and subtracting this from V, the velocity of the circumference, the absolute tangential velocity of the water leaving the fan is obtained, viz:

$$v'=V'-v\cos.\ \theta'\quad . \quad . \quad (14)$$

The head then due to this velocity is given by the formula

$$H_2=\frac{v'^2}{2g}\quad . \quad . \quad . \quad (15)$$

This, in other words, is the height that the water would rise, supposing that there was no friction to impede it. Now the centrifugal force has been estimated in lbs. per square inch; but by dividing it by 0.434 it is reduced to feet head of water. Then by adding these two heads together the theoretical height to which the pump lifts the water is obtained, *i.e.*,

$$H+H_2=\frac{F}{.434}+\frac{v'^2}{2g}\quad . \quad . \quad (16)$$

This theoretical lift is always greater than that deduced by experiment, and it is only in a perfect pump that these two lifts would coincide. Consequently, if the practical lift be divided by the theoretical lift, and the result multiplied by 100, the percentage efficiency of the pump is obtained.

These calculations are worked out for the last series of experiments upon the spiral-cased Appold pump, and the results tabulated as follows:

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[*] This expression is not strictly correct, but is a near approximation.

TABLE 7.

No. of Experiment.	Angular Velocity. ω.	ω^2.	$0.218\ \omega^2$.	Velocity through Fan, v.	$0.316\ v\ \omega$.	$0.218\ \omega^2 - 0.316\ s\ v\ \omega$.	Centrifugal force in Feet Head, H.	Velocity due to Impact, $R\omega - v\cos.\theta'$.	Head due to Impact, H_2.	Head by Experiment, H_1.	$H + H_2$.	Efficiency, $\frac{H_1}{H+H_2}$.
1	41.04	1,684	368.1	2.699	35.05	333.05	10.32	28.573	13.50	14.67	23.82	61.12
2	41.24	1,701	370.8	2.955	38.56	332.24	10.30	28.469	12.66	14.70	22.96	64.02
3	41.35	1,710	372.8	3.192	41.76	331.04	10.26	28.316	12.53	14.65	22.79	64.32
4	41.67	1,736	378.4	3.413	45.00	333.40	10.33	28.339	12.55	14.70	22.88	64.24
5	41.77	1,745	380.4	3.600	47.58	332.82	10.32	28.229	12.45	14.75	22.77	64.78
6	41.88	1,744	382.4	3.816	51.01	331.39	10.27	28.196	12.42	14.75	22.69	65.00
7	42.08	1,771	386.1	4.000	53.27	332.83	10.32	28.065	12.30	14.70	22.62	65.25
8	42.19	1,780	388.0	4.182	55.82	332.18	10.30	27.882	12.14	14.75	22.44	65.73
9	42.30	1,789	390.0	4.349	58.21	331.79	10.29	27.882	12.14	14.80	22.43	65.91
10	42.40	1,798	392.0	4.520	60.38	331.62	10.28	27.789	12.07	14.75	22.35	66.22
11	42.50	1,806	393.7	4.673	62.86	330.84	10.26	27.712	11.99	14.80	22.25	66.51
12	44.39	1,970	429.5	2.699	37.92	391.58	12.33	31.126	15.14	17.40	27.47	63.30
13	44.49	1,979	431.4	2.955	41.61	389.79	12.28	31.046	15.06	17.20	27.34	63.36
14	44.70	1,998	435.6	3.192	45.16	389.44	12.27	30.869	14.89	17.20	27.16	63.33
15	44.81	2,008	437.7	3.413	48.40	389.30	12.26	30.732	14.75	17.30	27.01	63.68
16	44.91	2,017	439.7	3.600	51.16	388.54	12.24	30.621	14.65	17.30	26.89	64.34
17	45.12	2,036	443.8	3.816	54.49	389.31	12.26	30.465	14.49	17.40	26.75	65.04
18	45.23	2,046	446.0	4.000	57.26	388.74	12.24	30.465	14.49	17.40	26.73	65.09
19	45.33	2,055	448.0	4.182	59.99	388.01	12.22	30.359	14.40	17.40	26.62	65.32
20	45.44	2,065	450.2	4.349	62.54	387.66	12.21	30.276	14.32	17.60	26.53	66.33
21	45.54	2,074	452.1	4.520	65.06	387.04	12.19	30.181	14.18	17.60	26.37	66.74
22	45.64	2,083	454.1	4.673	67.50	386.60	12.18	30.104	14.15	17.60	26.33	66.84

By comparing this with Table 5, it will be noticed that the theoretical efficiencies are considerably higher than those deduced by experiment. This is owing to the friction of the bearings of the pump and of the strap by which it is driven; a small amount is also due to the outer sides and edges of the fan revolving in the water, not having been subtracted from the power indicated by the dynamometer. Some experiments were afterwards made in order to determine this friction, and it was found to amount to about 4,500 foot-lbs. per minute. The second column of efficiencies in Table 5 is obtained by making the correction.

The slight discrepancies that are now found to exist between the theoretical and experimental methods of determining the efficiencies are within the limits of observation, or the experiments to determine the loss by friction were not made at the same time, or with the same pump as those tabulated above.

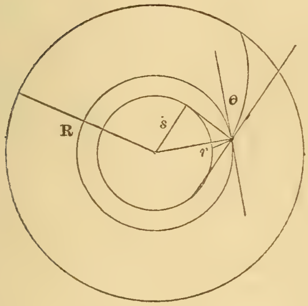
Some of the practical advantages of being able to calculate the efficiency of a centrifugal pump theoretically may here be mentioned. Since the efficiency is the lift as measured by experiment divided by that deduced by calculation, the more the latter can be reduced relatively to the former the greater is the efficiency of the pump.

The first principle to be attended to in effecting this condition is to avoid giving sudden shocks to the water, a principle which is ably discussed by M. Combes. This is done in the fan by designing the blades so as to enter the water in a direction tangential to their surface at their inner edges; and at the same time having their outer edges so as to leave the water moving in a direction as nearly as possible tangential to the circumference of the fan.

The internal angles vary both with the lift and discharge with which the pump is intended to work, and their value can be found in the following way: Assuming, as before, that their form is an involute of a circle, which is the best curve in consequence of its property of allowing a constant area of passage between the successive blades, and thus admitting of no change of velocity in the water whilst traversing the fan:

Let R=external radius of fan;
r=internal radius of fan;
ω=angular velocity of fan;
s=radius of generating circle of involute blades;
v=velocity of water through the passages of fan in feet per second;
v'=radial velocity of water entering the blades;
$V'=r\omega$=tangential velocity of the internal circumference of fan in feet per second;
θ=angle made by the blades with the internal circumference of the fan.

Fig. 12.



Then referring to Fig. 12.

$$\sin = \theta \frac{s}{r} \quad . \; . \; . \; . \; . \; . \; . \; (17)$$

and the condition that the blades should enter the water tangentically is evidently

$$\tan \theta = \frac{v'}{V'} \quad . \; . \; . \; . \; . \; (18)$$

Eliminating θ between equations 17 and 18, an equation is obtained for determining s, viz.,

$$s = \frac{v'r}{\sqrt{V'^2 + v'^2}} \quad . \; . \; . \; (19)$$

If then the blades of the fan be described with a generating circle, having a radius equal to that given by the preceding formula, they will cut the water in a tangential direction. This formula involves, first, the radical velocity of the water entering the fan, which is easily calculated from the required discharge. The best velocity for the water to flow through the passages of a fan is from six to eight feet per second. Next, the internal radius, which is generally made half the external. And, lastly, the velocity of the interral periphery of the fan. This is given by the following formulæ:

$$V = r\omega \quad . \; . \; . \; . \; . \; . \; . \; (20)$$

$$\omega = 9\frac{\sqrt{h}}{R} . g p . \quad . \; . \; . \; (21)$$

A general equation which unites all the variables in a centrifugal pump will now be considered. Assuming at present that the pump has a spiral case, and that its mean theoretical efficiency be 64 per cent., as it was found to be by experiment, then the lift under which the pump is to work must be this percentage of the theoretical lift. Putting this condition into symbols, in the general equation

$$\text{Lift} = 0.64 \left\{ \frac{p}{g + .434} \left\{ \frac{R^2 - r^2}{2} \omega^2 - v\,\omega \left(R - r + \frac{1}{2} s^2 \left(\frac{1}{R} - \frac{1}{r} \right) \right) \right\} + \frac{(V - v)^2}{2g} \right\} \quad . \; . \; (22)$$

But $V = R\,\omega$.

Substituting this in equation 22, an equation is found involving R, ω, v, and lift, any three of which being known, the fourth can be determined. This equation is of great practical value; for if, for instance, it is required to be known how fast a pump of given dimensions ought to be driven, so as to deliver a given quantity of water at a given lift, it is only necessary to put in the given dimensions, the discharge required, and the lift, and to solve the quadratic equation for ω, which gives the angular velocity at once. The general equation is applicable to all centrifugal pumps, but the theoretical efficiency of each kind must be determined before it is introduced into this equation, if great accuracy is required; but for all practical purposes the co-efficients 0.60 or 0.64 are sufficiently accurate.

The next thing to be borne in mind is to proportion the passages throughout the pump, so as to have a gradually increasing velocity in the water until it arrives at the circumference of the fan, and then to have a gradually decreasing velocity until it issues from the discharge-pipe. This condition is effected by having a conical end to the suction-pipe; and what is much more important is to have a spiral casing surrounding the fan. The importance of the last detail is shown most conclusively by the first series of experiments with the circular and spiral cases.

The form of the casing should be an Archimedian spiral, which has the property that the water flowing round the case moves with the same velocity as that issuing from the fan. The casing should then gradually open out into the discharge-pipe, and, if practicable, a conical adjutage should be added, similar to those described by Rettinger.

The same conditions are as nearly as possible fulfilled in the vertical-spindle pump previously described. Below the fan is a conical opening which produces a gradually increasing velocity in the water entering the fan. The water escaping from the fan is gradually turned into an upward direction by the guide-blades over the fan, and is at the same time checked in its velocity in consequence of the apertures between the guide-blades being smaller near the fan than above.

The vertical-spindle pump was the first tried without any guide-blades over the fan, and the maximum discharge obtained with an 8 HP. engine was 1,500 gallons per minute. Six guide-blades, similar to those in Figs. 8 and 10, were then put in, and the maximum discharge, with the same engine and same boiler pressure, amounted to 5,000 gallons per minute. This fact, is sufficient to show how important it is to make use of the velocity of impact, which by Table 7 produces a force greater than that of the centrifugal force.

Professor Rankine in discussing centrifugal pumps states that guide-blades are unnecessary, and even useless; but it seems that the results arrived at by experiment, if not those deduced by theory, clearly prove how important guide-blades are for the attainment of high efficiencies with the vertical-spindle centrifugal pump.

By referring to the column of calculated centrifugal forces in Table 7, it will be observed that the faster the fan rotates—the lift remaining constant—the smaller is the centrifugal force. This seems to be a paradox at first sight, but the reason is evident. As the discharge increases, the velocity of the water in the casing more nearly approaches that of the water leaving the fan; consequently the efficiency of the pump improves, and the theoretical lift diminishes, and with it the centrifugal force.

A remarkable property of centrifugal pumps may be mentioned, which is illustrated very clearly by the preceding experiments, viz., that a small increase in the number of revolutions of the pump, when it has begun to discharge, produces a very large increase in the delivery.

Thus in Table 5 the difference in discharge between experiments 1 and 11 is 741 gallons per minute, and a small increase of only fourteen revolutions; or in other words, while the discharge is nearly doubled the revolutions are only increased by $\frac{1}{28}$. This property has been made use of by Professor James Thompson as a speed-indicator, and has proved successful.

In conclusion, the whole aim of this Paper has been to show that the calculated efficiency is identically the same as that deduced by experiment. Most people have tried to calculate the friction in the pipes and passages in these pumps to effect this, but they have been unable to do so. The Author, however, has proceeded entirely on hydrodynamical principles, and has merely taken the velocities generated in the water, by which means the friction through the passages of the pump and the pipes has been eliminated from the calculations, in consequence of the velocities through them being directly affected by the friction, and these were determined by experiment.

The only friction it was necessary to measure was that of the strap required to drive the pump, and the friction in the bearings, and of the outside of the fan revolving in the water. This latter friction was ascertained by driving the pump at a speed of about 410 revolutions per minute, which corresponded nearly with a mean of the revolutions run during the previous experiments, and, as has been before stated, amounted to about 8 per cent. This is only an approximation; but owing to the great difficulty in maintaining a constant speed in a pump when it is not discharging, a more accurate series of experiments was not made.

SUPPLEMENTARY NOTE ON THE THEORY OF VENTILATION.

BY FRANCIS S. B. FRANCOIS DE CHAUMONT, M. D.

•Proceedings of the Royal Society.

In a previous paper I endeavored to establish a basis for calculating the amount of fresh air necessary to keep an air-space sufficiently pure for health, taking the carbonic acid as the measure. The results showed that the mean amount of carbonic acid as respiratory impurity in air undistinguishable by the sense of smell from fresh external air was under 0.2000 per 1000 volumes.* My object in the present note is to call attention to the relative effects of temperature and humidity upon the condition of air, as calculated from the same observations.

If we adopt the figures of Class No. 1 (that is "fresh," or not differing sensibly from the external air) we find the following:

Temperature.	Humidity.	Carbonic acid.
63° F.	73 per cent.	0.1943 per 1000 vols.

If, now, we arrange the observations according as they differ from the above standard of temperature and humidity, and note the record of sensation attached to each, we may ascertain how far the said record departs (if at all) from what it ought to have been as calculated from the actual CO_2. To do this we may employ the numerical values of the different classes, taking No. 1 (fresh) as unity, thus:

Class.	Sensation.	Value.
No. 1.	Fresh............	1.00
2.	Rather close......	2.13
3.	Close............	3.46
4.	Extremely close...	4.66

Taking each observation and dividing the CO_2 found by the mean quantity of No. 1, viz. 0.1943, we get a number which will give the *theoretical* value of its effect upon the senses; and by comparing this with the *actual* value of the *recorded* sensation, we can note whether the difference is *plus* or *minus*, if any. All observed quantities of CO_2 below 0.1943 are considered equal to that number, and all quantities above 0.9054 as equal to it, as the sense of smell does not seem capable of differentiating quantities except between those limits.

Out of 458 fully recorded cases, 186 gave a recorded sensation *in excess* of the theoretical value—that is, the air seemed less pure than would have been expected from its CO_2. In these the average temperature and humidity were both above Class 1.

152 cases gave a recorded sensation *below* the theoretical value—that is, the air seemed purer than would have been expected from its CO_2. In those cases the average temperature was above, but the average humidity below the mean of Class 1.

120 cases gave a recorded sensation that exactly corresponded with the theoretical value. In those cases the average temperature was above and the average humidity below the mean of Class 1.

Arranging these results and putting F for the temperature in degrees of Fahrenheit, and H for the humidity per cent., we have:

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* In the former paper the amount was given at 0.1830 per 1000; but on revising the calculations, a previously unobserved error was found in one of the constants employed, the correction for which would have the effect of altering the figures a little, the changes being as follows:

		Respiratory impurity as CO_2.	
Classes.		*Original figures.*	*Corrected figures.*
No. 1.	Fresh...........	0.1830	0.1943
2.	Rather close....	0.3894	0.4132
3.	Close...	0.6322	0.6708
4.	Extremely close.	0.8533	0.9054

Except for the sake of rigid accuracy the difference is immaterial, as I adopted 0.2000 as the limit of respiratory impurity in an air-space well ventilated, and the corrected number 0.1943 is still below that.

$+58^{\circ}.6\,F+86\,H=+197.70$ ....[1] Aggregate difference of the recorded and the theoretical value of sensation.

$+230.8\,F-82\,H=-117.37$ ....[2] Do.

$+244.0\,F-91\,H=0$ [3] Do.

Adding the two last equations, we have:

$+474^{\circ}.8\,F-173\,H=-117.37$ ....[4] Do.

From [1] and [4] we can determine the respective values of F and H, which are as follow:

$$F = 0.4730 \qquad H = 1.9765$$

Or, stated in terms of CO_2, by multiplying by 0.1943:

$$F = 0.0919 \qquad H = 0.3833 \text{ per } 1000$$

Taking F as unity, we have:

$$F : H :: 1.0000 : 4.1789 \text{ vols.}$$

Or an increase of one per cent. of humidity has as much influence on the condition of an air-space (as judged of by the sense of smell) as a rise of 4°.18 of temperature in Fahrenheit's scale, equal to 2°.32 Centigrade, or 1°.86 Réaumur.

This may be taken as a proof of the powerful influence exercised by a *damp* atmosphere, corroborating the conclusions arrived at by ordinary experience; and it follows that as much care ought to be taken to ensure proper hygrometric conditions as to maintain a sufficiently high temperature. This is especially the case in the wards or chambers of the sick, in which regular observations with the wet and dry-bulb thermometers ought to be made; these would probably give a valuable indication of the condition of the ventilation, either along with or in the absence of other more detailed investigations. Thus a room at the temperature of 60° F. and with 88 per cent. of humidity contains 5.1 grains of vapor per cubic foot: suppose the external air to be at 50° F. with the same humidity, 88 per cent.; this would give 3.6 grains of vapor per cubic foot; to reduce the humidity in the room to 73 per cent., or 4.2 grains per cubic foot, we must add the following amount of external air,

$$\frac{5.1 - 4.2}{4.2 - 3.6} = 1.5,$$

or once and a half the volume of air in the room. If the inmates have each 1000 cubic feet of space, it follows that either their supply of fresh air is short by 1500 cubic feet per head per hour, or else that there are sources of excessive humidity within the air-space which demand immediate removal.

In the return of the annual accounts of the several manufacturing establishments under the War Office just presented to Parliament, there are some interesting particulars as to the cost of the heavy muzzle loading rifled ordnance of the present day. Thus the 80 ton gun, prepared with a calibre of 14½ inches, cost £6491 for material, £2093 for labor, and £1091 for indirect expenditure, making a total of £9675, subject to further augmentation, until the ultimate cost appears as £10,913. Under the head of the Royal Carriage Department, is an entry of a "truck for proof of heavy guns," made for the gun factories, and costing in all £1593 This, in all probability, is the large 12 wheeled proof carriage on which the 80 ton gun has hitherto been fired. Proceeding with the account of the guns, we find that during the year 1875-76, as many as twenty-one 12½ in. 38 ton guns were made, costing altogether £78,109, or £3719 each. Two 11 inch 25 ton guns cost £4718, or £2350 each; thirteen 10 inch 18 ton guns cost £18,688, or £1437 each. These last throw a projectile of 400 lbs., the 11 inch 25 ton guns a projectile of 535 lbs., and the 38 ton guns a projectile of 800 lbs., while the monster 80 ton gun has a projectile weighing 1700 lbs., or fully three-quarters of a ton. Taking the guns of all sizes, down to a 7 pounder of 150 lbs., the entire number made in the year was 527. The expenditure for material was £100,637, and for labor £38,500, with an indirect expenditure of £19,794, making a total of £158,931. Balance sheet No. 2 raises this amount still further, so as to make up a sum of £179,264. The increased expense consequent on the manufacture of larger guns is shown by the price per ton, which is £80 for a gun of 18 tons, £94 for a gun of 25 tons; £98 for a gun of 38 tons, and £136 for a gun of 80 tons.

—*The Engineer.*

MOMENTUM AND VIS VIVA.

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Contributed to VAN NOSTRAND'S MAGAZINE.

V.

THE MEASUREMENT OF CONTINUOUS FORCES, CONCLUDED.

I have proposed to use the word *toil*, to mean *the exertion of force during time*, whether producing motion of masses or not. The unit of *work* is the exertion of a pound pressure through a distance of one foot. And we also regard the exertion of a half pound pressure through a distance of two feet as equal to a foot-pound of work; or if any number of pounds constant pressure, multiplied by the number of feet through which it is exerted, equals unity, the work done is equal to a foot-pound. If the pressure is variable, a foot-pound of work will have been performed, when the sum of all the products obtained by multiplying the elementary values of the pressures by the respective distances through which these elementary pressures are exerted, is equal to unity. In a similar way, in measuring *toil*, we may regard a half-pound pressure exerted for *two seconds* as equivalent to a one pound pressure exerted for one second; or if any number of pounds constant pressure, multiplied by the number of seconds during which it is exerted equals unity, the *toil* will be regarded as equal to a *second-pound*. And if a pressure is variable, whether producing mass motion or not, the pressure will have performed a second-pound of toil, when the sum of all the products obtained by multiplying the elementary values of the pressure, by the respective times during which these elementary pressures act, is equal to unity.

So long as toil is prevented by circumstances from imparting velocity to the body to which it is applied, we can of course make no use of change of velocity to measure the toil; but, whether any *work* is performed or not, if we know the time during which a constant pressure is exerted, as shown by the continued distortion of any elastic body, *e.g.*, a spring balance, we may still reckon the amount of *toil* of the force maintaining the distortion to be the product of the pressure by the time.

A unit of toil would not in general be equivalent to a unit of work; because the work done by any force in a second depends on the velocity of motion of its point of application; but if a unit of toil be performed on a given body, free to move, and having any *given* initial velocity, the same change of velocity will be produced, and the same amount of work be done, whether the toil be that of one pound pressure for one second, or of ten pounds pressure for one tenth of a second, or of any pressure, constant or variable, such that the sum of the elementary products of pressure and time equals unity. Hence, where changes of velocity of masses are the result of toil, the amount of toil necessary to produce them can be inferred from these changes just as readily as we can infer from them the amount of work employed.

Under this view, we should define the *momentum* of a moving body to be its power of performing *toil*, or briefly, its *toiling* power. The amount of toil which a given body could perform in virtue of a given velocity, could always be represented by the product of any constant force F, by a suitable number of seconds, T, that is, by a product, FT, which product would thus represent the *momentum* of the body. But if the foot and the second be the units of distance and time, and if the pound pressure be the unit of force, and the matt be the unit of mass; or if the pound of matter be the unit of mass, and the tend be the unit of force; we can easily show that in producing velocity the following equation would be true numerically, viz:

$$MV=FT; \qquad (6)$$

so that, although the product MV would not, correctly speaking, *be* what we define as the *momentum* of a body, yet the number of units in it would be the same as the number of units in the momentum, and the product MV could therefore be taken as the *measure* of the momentum, or toiling power, of a body.[*] In a similar way I suppose we ought to regard the product, $\frac{1}{2}MV^2$, rather as a *measure* of the vis viva or working power of a body, than as *being* the vis viva itself; for if a constant force F had imparted the velocity V in the distance D, the equation

$$\tfrac{1}{2}MV^2=FD, \qquad (7)$$

should be regarded simply as expressing *numerical* equality.

In Prof. Tait's Glasgow lecture on *force*, (*Nature*, Sept. 21, 1876), he attempts to show that *force* is nothing whatever but a mere *name* for the *rate of change of momentum.* I cannot agree with this view; and a sufficient reason for rejecting it seems to me to be found in the fact that the intensities of forces can be compared, or *measured*, without having anything to do with momentum, simply by observing the points at which they will hold the index of a spring balance, while not doing any measurable *work*, or in any way changing momentum. I admit that the intensity, in pounds or in tends, of a constant force acting to change the velocity of a body, can be *known* from the observed rate of change of the momentum of the body. For if a constant force act for a time T, to impart the velocity V to the mass M, we may write equation (6) in the form

$$F=\frac{MV}{T}, \qquad (8)$$

and the *number of units* in this ratio, or *rate*, $\frac{MV}{T}$, will thus be the same as the *number of units* in the acting force. But merely because we have cunningly assumed the arbitrary *units* of force, mass, velocity and time so as purposely to bring about the numerical equality given by (6) or (8), it does not therefore follow that force *is* nothing but a ratio, or rate of change. We might just as well say that because the mass of a body may be known from its weight, therefore the mass *is* the weight. For in the latitude of London, the *mass* of a body, in pounds, is numerically equal to its *weight.*

Further, if force *is* the rate of change of momentum per unit of time, we have just as good a right to assert that it *is* the rate of change of vis viva per unit of distance. For equation (7) may be written

$$F=\frac{\tfrac{1}{2}MV^2}{D}; \qquad (9)$$

But a simple relation of numerical equality like this, arising from our choice of units alluded to above, does not seem to me sufficient reason for asserting that *force is* nothing more than a ratio, like 3 per cent.

Can it be that equation (9) represents any part of what Prof. Tait alludes to when he says that "a simple mathematical operation shows us that it is precisely the same thing to say:

"*The horse-power or amount of work done by an agent in each second is the product of the force into the average velocity of the agent*, and to say:

"*Force is the rate at which an agent does work per unit of length.*"

What his simple mathematical operation would be, I do not know; for I never saw it stated elsewhere that *the horse-power done by an agent in each second is the product of the force into the average velocity of the agent*, or that *the horse-power in each second* is such a product, or even that *the horse-power* is that product.

Prof. Tait says that momentum and force cannot lawfully be equated to one another (*sic*) under any circumstances whatever. I quite agree that momentum is not the same thing as force; but it is evident that if we define momentum as the toiling power of a body, and then require this toil to be performed against a constant resistance *in one second*, the intensity of the force (pressure or tension) which the body must exert will be *numerically* equal to its momentum.

The Professor might have found much

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[*] The first three sections of this series of articles were written more than seven years ago, and were published here with scarcely any change from the form then given to them. During this publication of them my interest in the subject revived, and I am led to suspect that in the zeal of my antagonism to the expression *quantity of motion* of a body, I may perhaps at first have underrated the importance of the product MV. I made the unit of momentum of a body a pound pressure, under the condition that the body were to be brought to rest by a constant force in one second. This unit might have been understood as a second-pound; but I neglected to bring out as clearly as might have been done, that the number of second-pounds of what I have now called *toil*, which must be employed in giving a stated velocity to a body, or which the moving body can perform in virtue of that velocity, is the same, in whatever time or distance the velocity is imparted, or the body brought to rest from that velocity, whether the impelling force or the resistance be constant or variable. That I did not entirely overlook this result, may, however, be seen by reference to column 2, p. 130, of this Magazine.

better arguments for not confounding force and momentum than the one he presents; for that seems to me to be of no validity. His supposed proof of the different nature of force and momentum consists in showing that if you define *unit* momentum as the momentum of a unit of matter moving with a unit of velocity, and define *unit* force as that force which, acting for a unit of time, produces in a unit of mass a unit of velocity, then a change of the time unit would change the numerical expression for a given quantity of momentum in a certain ratio, whereas it would change the numerical expression for a given force in the square of that ratio. But this result depends on the mere form of words in which he chooses, not necessarily, to define the *unit* of force. If the *unit* of *velocity* be defined as that of a body moving through a unit of distance in a unit of time, if you double the real value of the unit of time, and then stick to the form of words by which the *unit* of *force* is above defined, it results that you require the new unit of force to produce only one half the previous actual velocity, and that you also give it twice the time to work in. But by Tait's own words (near top of p. 461, *loc. cit.*) "to compare forces, which is the essence of the process of measuring them, we must give them equal times to act."

Suppose I define *unit* of momentum as the momentum of a pound of matter moving with a velocity of one foot per second, and *unit* of force as that force which by acting on the standard pound of matter during the hundred-thousandth part of a stellar day, should give the mass a velocity of one foot per second. If I were then to change my definitions of the units of momentum and force, by substituting *minute* for *second* in both definitions, the new unit of force would have precisely the same ratio to the former unit as would the new unit of momentum to the former one. If V represent velocity in feet per second, and V' velocity in feet per minute, a quantity of momentum in the old units equal numerically to MV would, in terms of the new units, be measured by $60\ MV'$; and if F' stand for a number of the new units of force, and F for the force in my first units, which could have produced the given momentum in a given time, then a force of the same intensity as F will be expressed in the new system by $60\ F'$. So that I fail to see any *force* in the Professor's argument on this point.

Again, Prof. Tait says that the mathematician expresses the distinction between force and momentum, by saying that "momentum is the *time integral* of force, because force is the rate of change of momentum," both of which propositions seem to me objectionable. I have already given some reasons for rejecting the idea that force is a mere *rate of change;* and how can momentum be properly said to be the *time integral* of force, when *any amount* of momentum can be produced in *any time* by a force of the proper intensity? It appears to me that the *integral* of any quantity ought to be equal to the sum of all its increments from zero. But a *constant* force can have no increment, and therefore no proper *integral* dependent on time. And, to take an example of a variable force, we may suppose the following case: Let a force equal to one pound act for one second on the mass M, free to move from rest. At the beginning of each following second let an increment of two pounds be given to the force, supposing it to remain constant for a second at a time. The *integral* of the force, at the end of say five seconds, will be the sum of its increments from zero, or

$$1+2+2+2+2=9 \text{ pounds.}$$

To be sure, since the force in the successive seconds is 1, 3, 5, 7 and 9 pounds, the total increment of the *toil* performed by the force in the supposed time, is

$$1+3+5+7+9=25 \text{ second-pounds;}$$

and the product of the mass and final velocity of the body will be 25; but this amount of toil might have been performed, and the same velocity imparted, in *any time*; so that a momentum of 25 units can of itself tell us neither the time nor the intensity of the force which has produced it. In speaking of a particular force, there might perhaps be no objection to calling the momentum which it imparted to a body the *time-effect* of this force; but this is not the same thing as saying generally that momentum is the time *integral* of force.

The true relation of *time* to the action of a force I conceive to be simply this, that if we know the *intensity* of a constant force which has produced a given momentum, the *time* can be found from equation (6); or if we know the *time*, we can get the intensity of a constant force from the same equation. If a force is variable, or if we do not know either its intensity or the time it has acted, the product MV will still show the number of second-pounds or second-tends of toil which the force has performed in imparting the velocity V.

Prof. Tait asserts that force is not an objective reality, but a convenient abstraction—a *mere name*—and that the *product of a force into the displacement of its point of application* has an objective existence. How the product of a *mere name* into the *displacement of its point of application* can have an objective existence, while that which the name denotes cannot, I leave to the metaphysician.

Prof. Tait, at the end of his lecture, says, that "in defense of accuracy, which is the *sine qua non* of all science, we must be 'zealous,' as it were, even to 'slaying.'" Whether the points of the lecture to which I have called attention are merely slips, due to the unpropitious circumstances of time, place and surroundings, under which he says the lecture was prepared, or whether he would, if he thought it worth while, show that my objections are groundless, I do not venture to say. But if his position is not sound, the high and well earned fame attached to his name may make the lecture a source of much future confusion; so that I have thought it worth *my* while to consider it here at some length.

VI. THE MEASUREMENT OF IMPULSIVE FORCES.

Writers on mechanics usually distinguish two classes of forces, viz: continuous forces, or those which act during a measurable time, and impulsive forces, or those which act for a time so short that it cannot be measured. The distinction is, or ought to be, one of convenience merely; for however quickly a force may produce its effect, the action does require a certain lapse of time. The text books generally say that the *measure* of an impulsive force is the *quantity of motion*, or the *momentum*, which the force produces. I need not recall my objections to the phrase *quantity of motion;* but I will here consider whether it is correct, or in what sense it may be correct, to take the product MV for the measure of an impulsive force which has communicated the velocity V to the mass M.

It is evident, from what has preceded, that if by the word *force* we mean pressure or tension, the product MV would be by no means a true measure of the force; for the velocity V might have been produced by *any* force in a proper time; and since we are supposed to be ignorant of the *time* actually employed, the product MV can tell us nothing as to the *intensity* of the acting force. Indeed, the intensity of an impulsive force must generally, if not always, be variable during its action; beginning at a minimum value, rising to a maximum, and then diminishing to the end. But we have explained that, with the proper units, this product MV represents the number of units of *toil* that a continuous force, whether constant or variable, would have performed in giving the velocity V to the mass M. Hence, since an impulsive force does not differ from a continuous force, except in our ignorance of the length of time of its action, we may take the product MV as a measure of the *toil* performed by any impulsive force in imparting the velocity V to the mass M. The only attribute of an impulsive force which I regard as measurable by a product MV, is then its *toil*, reckoned in English units by second-pounds or second-tends. The product MV does not measure *an impulsive force* any more than $\frac{1}{2}MV^2$ does; for this latter measures the *work* performed by it in imparting the given velocity. If an impulsive force in imparting velocity also performs work in developing heat, the amount of this work may be determined by observed changes of temperature. If the force acts for an immeasurably short time or distance, we can not know its intensity, but must be content with knowing the toil and the work performed by it.

VII. CONCLUSION.

I will add a very brief summary of some of the definitions and explanations which the foregoing discussions would lead me to adopt.

1. The *mass* of a body is its quantity

of matter. The *unit* of mass may be the quantity of matter in a standard piece of platinum, called the pound avoirdupois. Or, if preferred, (see next paragraph), we may take for the unit of mass a *matt*, which is a quantity of matter equal to 32.1912 times the quantity in the standard piece of platinum.

2. *Force* is any pressure or tension acting on a body, either to produce or maintain distortion, to set the body in motion, or to in any way alter the velocity of its motion. The *unit* of force is the pressure, due to gravity, exerted at London by the standard pound of platinum. This is called a pound pressure. A pressure of this intensity, acting for one second on a mass equal to one matt, free to move, will impart to the mass a velocity of one foot per second. If preferred, we may take for the *unit* of force, such a pressure as will, by acting for one second on the standard pound of matter, impart to this a velocity of one foot per second. This unit of pressure, or force, is called a *tend*, and it is equal to a pound pressure divided by 32.1912; that is, a pound pressure equals 32.1912 tends.

3. *Toil* is the exertion of force during time. The *unit* of toil is either the exertion of a pound pressure during one second, called a second-pound of toil, or the exertion of a tend pressure during one second, called a second-tend of toil. The amount of toil of any force will be the product of the number of units of pressure by the number of seconds during which it acts.

4. *Work* is the exertion of force through distance. The *unit* of work is either the exertion of a pound pressure through one foot, called a foot-pound of work, or the exertion of a tend pressure through one foot, called a foot-tend of work. The amount of work of any force will be the product of the number of units of pressure by the number of feet through which it is exerted.

5. The *momentum* of a moving body is its power of performing toil, or, more briefly, its toiling power. If the matt of mass and the pound of pressure are assumed as units, or if the pound of mass and the tend of pressure are assumed as units, the number of units of toil which a force performs in imparting the velocity V to a mass M is equal to the number of units in the product MV; it is also found that a body having this mass and velocity can perform the same number of units of toil while being brought to rest by any opposing force. Hence the product MV may be taken as the *measure* of the momentum, or toiling power, of a moving body.

6. The *vis viva* of a moving body is its power of performing work, or more briefly, its working power. If the matt of mass and the pound of pressure are assumed as units, or if the pound of mass and the tend of pressure are assumed as units, the number of units of work which a force performs in imparting the velocity V to a mass M is equal to the number of units in the product $\frac{1}{2}MV^2$; it is also found that a body having this mass and velocity can perform the same number of units of work while being brought to rest by any opposing force. Hence the product $\frac{1}{2}MV^2$ may be taken as the *measure* of the *vis viva*, or working power, of a moving body.

7. If the velocity of a body in one direction be regarded as positive, and velocity in the opposite direction be regarded as negative, then by the impact of two bodies the algebraic sum of their momenta or toiling powers will not be altered; but the total vis viva or working power of the two bodies, *as masses*, will be reduced by the amount of energy converted by the impact into heat.

RELATIVE VALUE OF WATER OF DIFFERENT DISTRICTS.*

From "Nature."

No less than 10,000 square miles of England and Wales are occupied by the new red sandstone and permian formations, which absorb not less than ten inches of rainfall annually, and probably more where the overlying drift is pervious or absent, and the sandstone open and permeable.

The Rivers Pollution Commissioners classify waters in the order of their

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* Abstract of a Report of a Committee for Investigating the Circulation of Underground Waters, etc.

excellence, for general fitness for drinking and cooking, as follows:

A. Wholesome.	1. Spring water. 2. Deep well water. 3. Upland surface water.	Very palatable.
B. Suspicious.	4. Stored rain water. 5. Surface water from cultivated land.	Moderately palatable.
C. Dangerous.	6. River water to which sewage gets access. 7. Shallow well water.	Palatable.

The average amount of hardness of the water of the deep wells of the new red sandstone tabulated by the Rivers Pollution Commission being 17°.9, and that of the springs no less than 18°.8, the relation of hardness of water to the rate of mortality of the persons drinking it becomes a matter of great importance.

The Commissioners give three tables of statistics that bear directly upon this point:

From Table I, it appears that in twenty-six towns, inhabited by 1,933,524 persons supplied with water, not exceeding 5° of hardness, the average death-rate was 29°.1 per 1,000 per annum.

From Table II, we learn that in twenty-five towns inhabited by 2,041,383 persons drinking water of more than 5°, but not exceeding 10°, the average death-rate was 28°.3 per 1,000.

Table III, gives sixty towns, with an aggregate population of 2,687,846, drinking water of more than 10° of hardness; the average death-rate was only 24°.3.

Of the towns in Table I, none are supplied from the new red or permian formations.

In Table II, three are so supplied.

In Table III, ten are so supplied, from which it will be observed that the largest number of towns supplied with new red water are found in the table with the lowest death-rate and the hardest water.

The same result is obtained if we compare towns of corresponding populations and occupations supplied with soft waters from surface areas and those supplied with deep well water in the new red sandstone. Thus:

	Per 1,000.
Manchester, 351,189 inhabitants, average death-rate	32.0
Birmingham, 343,787 inhabitants, average death-rate	24.4

And again—

Stirling, 14,279, inhabitants, average death-rate	26.1
Tranmere, 16,143 inhabitants, average death-rate	18.8

The averages are, of course, also dependent on many external causes. Thus, Greenock and Plymouth, both supplied with soft water, with an equal number of inhabitants have a death-rate respectively of 32.6 and 23.3 per 1,000, due to difference of density of population, Greenock only having one house for every twenty-eight people. And again, Liverpool and Birkenhead, both supplied with moderately hard water in the one, an old and densely-populated town with a site saturated with what is injurious to health, the death-rate is 31 per 1,000, while Birkenhead, a new town on an open site with wide streets, has a death-rate of only 24 per thousand, though mainly inhabited by a poor and struggling class of persons.

Still it is worthy of note that the five inland manufacturing towns with the lowest death-rate are all supplied with hard water, and all from the new red sandstone.

	Population.	Mortality per 1,000 per annum.
Birmingham	343,787	24.4
Leicester	95,220	27.0
Nottingham	86,621	24.2
Stoke-on-Trent	130,985	27.9
Wolverhampton	68,291	25.9
Average	144,981	25.5

And again the average death-rate of twelve inland non-manufacturing towns supplied with soft water was 26.0 per 1,000, while that of twenty similar towns supplied with hard water was only 23.2.

When, however, the mortality of the districts, including the principal English watering places, is compared, there appears to be little variation in the death-rate, whether the population be supplied with soft, moderate, or hard water, so that it may be safely concluded that where sanitary conditions prevail with equal uniformity, the rate of mortality is practically uninfluenced by the degree of hardness of the water drunk, and the Rivers Pollution Commission are of opinion that soft and hard waters, if equally free from deleterious organic substances, are equally wholesome.

The Committee are of opinion that it

is desirable that they should continue to inquire into areas where new red and permian waters might be obtained by means of deep wells. Looking to the national importance of utilising the underground waters of England, it is desirable that the sphere of this inquiry should be extended so as to include the oolites, which are often not made available for the supply of the population living upon them until the water is hopelessly polluted with sewage. The result of their labors, since the formation of the Committee, has been to prove that there is an available supply of water from the new red sandstone and permian of England of not less than a billion and a-half of gallons of water, the quality of which is remarkably free from organic impurity, and the hardness of which does not in the least appear to affect the health of the population at present taking their supply from it. The death-rate of this area compares well with the best soft-water districts.

Mr. J. Mellard Reade, C.E., F.G.S., added a special report *On the South-West Lancashire Wells,* in which he analysed the information he had obtained for the Committee through the printed forms of inquiry, supplemented by further inquiries which had suggested themselves to him. For the purpose of comparison Mr. Reade selected three nuclei or centers, about which the most important systems of wells are grouped, viz., Liverpool, Birkenhead, and Widnes, and illustrated them by maps and vertical sections showing the relative water-levels reduced to a common datum.

The President thought it important to note the influence of heavy and long-continued rain in relation to absorption by rocks. When rain lasts only a short time, even if it were very heavy, only a little was absorbed; but if the rainfall were spread over a longer time a larger proportion would sink into the rocks. M. Lebour described the method adopted by the French engineers for representing the underground water-contours on maps, there being also lines showing the strike of the rocks; he commended this method to the consideration of the Committee.

LONDON WATER SUPPLY.*

By Mr. F. J. BRAMWELL and Mr. EDWARD EASTON.

From "The Engineer."

Probably some of the members present may know that we have—in conjunction with Sir Joseph Bazalgette—at the request of the Metropolitan Board of Works, recently reported upon the water supply to London, especially in relation to the quality of the potable water and to the provisions of water at an adequate pressure for the extinction of fires. We make no apology for bringing the subject of the London water supply before this section, because that subject is an extremely large one, and the question involved in its economic considerations are, therefore, of very considerable importance. In 1874, the population supplied was 3,655,000, dwelling in 511,000 houses. The daily average quantity throughout the year was 116,250,000 gallons. The quantity per head, therefore, for all purposes was a little under 32 gallons. As is well known, this water supply is in the hands of eight companies. The aggregate capital employed in 1874, including share and loan capital, was £11,196,000. The gross income from water was £1,137,000; in addition to this there was £16,000 from land rents—making a total of £1,153,000. The expenses were £447,500. The net income was £705,-700, giving a rate of interest upon all the capital employed, share and loan together, of proximately 6.3 per cent. In Colonel Bolton's last report, June, 1877, it is stated that the population supplied was 3,796,000, dwelling in 533,000 houses, and that the daily average quantity was 132,500,000 gallons, equal to a little less than 25 gallons per head

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* British Association.

during the summer months, and representing a probable average daily delivery throughout the year of from 120,000,000 to 125,000,000 gallons. London, in common with other towns, requires water for the under-mentioned purposes: (*a*) drinking and culinary; (*b*) cleanliness—personal, domestic, and civil; (*c*) extinction of fires; (*d*) manufacturing; (*e*) road watering and miscellaneous. With respect to the heads (*b*) (*d*) and (*e*) we believe that the present water supply is generally satisfactory; but except on rare cases this cannot be said with respect of head (*a*), nor in any case can it be said in respect of head (*c*). The importance of these two heads it is difficult to exaggerate, as on the quality of the potable water depends to a large extent the health of a vast population, and on an adequate provision for the extinction of fires depends the preservation of the largest aggregation of wealth in the world. With respect to head (*a*) we have already shown that there are in London close upon 4,000,000 of human beings who should be supplied with wholesome potable water; let us now see, as regards head (*c*), what is the mere money value of the property requiring protection from fire. This subject of fire extinction has frequently been the subject of Parliamentary consideration—in 1862, in 1867, and again in 1876 and 1877. In these later years a committee, presided over by Sir H. Selwin Ibbetson, has made the fullest investigation into the question of the fire brigade and into the means of extinguishing fire in the metropolis. From the evidence given before that committe it appears that as much as £540,000,000 of London property is insured in the fire offices. With respect to the uninsured property there is a great diversity of opinion. No witness puts its lower than equal in amount to the property insured; so that no one gives the total value as being much less than £1,100,000,000. But other estimates made the proportion as much as one insured to four uninsured; the total value of the property, therefore, according to those estimates, amounts to £2,700,000,000. Captain Shaw's opinion, however, is, there is one-third insured, and two-thirds uninsured. If he is right, then the total value of the property of London is a little over £1,600,000,000. To revert to (*a*) potable water. One of the writers of this paper is old enough to remember the wheels at London bridge pumping up the water of the Thames as it flowed past them, and delivering it direct into the houses of the inhabitants; and even within thirty years some of the companies took their supply from the Thames opposite what was then Hungerford Market, from the Dolphin at Chelsea, from Hammersmith, and from Kew; but year by year the river got more foul and the population became more critical. Complaint was made as to the quality, and as the companies from time to time came to Parliament for further capital to meet the increasing wants, conditions were put upon them which have resulted in all companies who derived their sources of supply from the Thames, taking that supply from above Teddington weir, so as to be out of the tidal influence, and in the employment of depositing reservoirs and filter beds so as to git rid, as far as possible, of all foreign matter. But, in spite of all this, the public is not satisfied. Occasionally the water is declared on authority by Colonel Bolton, in his monthly reports, to be turbid, and in his last report—for June, already quoted—Dr. Frankland says that the suspended matter in the water supplied by one of the companies was full of moving organisms. Naturally the public, even after all that has been done, are not satisfied as regards the supply of potable water. Nor do we believe that they ever will be satisfied, or can be satisfied, so long as that water is derived from rivers into which is poured the sewage of a great part of the large population of the basins of the Thames and of the Lea, and also the surface drainage from the highly cultivated and therefore highly manured land of those basins. With respect to fire extinction, nothing at all has been done by the water companies beyond the giving of a supply of water at street levels, to be picked up by the pumps of the fire-engines and pumped upon the fire. Captain Shaw has said in his evidence before the late committee that his requirements, even at the largest fires—which requirements he calls very moderate—amount to no more than 2000 gallons of water per minute, capable of being played upon the fire through lengths of hose not exceeding in the aggregate half a mile—that is, through fourteen hoses of 200 feet each, each hose conveying 150 gallons per minute. And yet he says that in many instances this moderate quantity cannot be obtained. It appears that the volume of water used annually in London for the extinction of fires is only one four-hundredth of the total supply—that is to say, not one day's water is used for extinguishing fires in the year. But it must be remembered that fires do not burn with average regularity throughout the year, throughout the day, and throughout the metropolis, but that they break out at intervals and at particular spots, and, therefore, this comparatively small annual percentage has to be delivered, not as a regular percentage of the usual delivery, but in quantities vastly in excess, for the moment, of anything required for the locality in which the fire may be raging. Two thousand gallons per minute is at the rate of about 3,000,000 gallons in the twenty-four hours—that is to say, when a large fire occurs there should be concentrated at the particular spot a rate of delivery equal to one-fortieth of the whole power of the London water supply, or enough to meet the ordinary daily wants of a town of 100,000 inhabitants. Bearing these facts in mind, it is not surprising that the pipes of the companies, which have been laid with a due regard to economy of capital, large enough merely to supply the needs of the houses—these needs being at the average rate of a pint and a quarter per minute per house, or 227 gallons per house per day—should be inadequate, in many instances, to bring to the scene of a fire 2000 gallons per minute, even when delivered at the level of the street for the services of a fire-engine. But there can be no question that to insure the prompt extinction of fire it would be in the highest degree desirable to be able to apply an effective jet from the water main, without the intervention of a fire engine. This has been amply proved by the experience of Manchester, Liverpool, and other towns, where there is, by gravitation in the mains, a constant high pressure competent to deliver large quantities of water in the form of jets. This section is not one which deals with engineering matters, but we think we may be pardoned for making a few remarks upon the subject of jets. The Metropolitan Board of Works, in its desire to do that which is best for the safety of the inhabitants of London, and to do nothing rashly, more than twelve months ago instructed us, in conjunction with Sir Joseph Bazalgette, to carry out a series of practical experiments upon the question of fire jets, which should put beyond all doubt the engineering points involved. We may state briefly the result of these experiments. With a very low jet, say of some 30 feet, about seven-eighths of the head or pressure effective at the orifice of the jet will be obtained, as the height of the column of water—that is to say, 40 feet of head at the orifice would give a jet of about 35 feet in perfectly still air; but as the heights of the jets are increased, and increased they must be, if they are to be of any service in extinguishing fires in the most valuable buildings in London, the modern buildings, which are so lofty, the percentage of which the column of water produced bears to the effective pressure producing it becomes less and less, so that for a jet to rise to a height of 80 feet there must be, roundly, a pressure equal to 128 feet. To rise to a height of 100 feet, there must be an effective pressure of about 180 feet. Moreover, the higher the jet the greater must be the diameter of the column of water. A small jet, which will attain a respectable percentage of the head producing it, if that head be low, will be unable to cope with the resistance of the air if the pressure be high. We do not suppose that the section will care to go into details on this matter; but we will give them as an instance a fair average jet required for London purposes. A jet that would rise 80 feet in still air, if of 1 inch in diameter, would deliver the 150 gallons per minute, and would demand an effective pressure, as has already been said, of 128 feet at the very orifice of the jet; and it might be thought, therefore, that if a pressure could be maintained in the pipes equal to 128 feet of head, when the water was flowing, that all that was desired would be provided. But this is not so. There is the very striking and to many people very unexpected consideration of the friction of the water through the hose to be taken into account; and the section may, perhaps, little expect to be told that every foot of the usual size of hose employed by the London Fire Brigade, when conveying 150 gallons of water per minute, requires a pressure of a little over 3 inches to drive that water through. As a matter of fact, the 200 feet of hose demands 53 feet of pressure to get 150 gallons per minute through them. Therefore, to obtain a jet of 80 feet high, expending 150 gallons per minute at the end of 200 feet of hose, there is needed a pressure at the main of 181 feet, and this pressure must be maintained while the water is flowing. We believe the section will now well appreciate why it is that if it be difficult for the water companies in many instances to deliver 2000 gallons per minute at the street level, it would become absolutely impossible for them to deliver it were its exit opposed by this 181 feet of pressure. More especially will the section understand the difficulty when they are told that there is not one of the London companies which is bound to give throughout its district this pressure of 181 feet above the pavement, or anything bordering upon it; indeed, the obligations of the largest company but one are fulfilled by giving a pressure of only 40 feet above the pavement—a pressure which, so far from being able to produce a jet delivering 150 gallons at the end of 200 feet of hose, can only succeed in driving 122 gallons per minute through a hose of that length at the ground level without any jet at all. These things being so, we think that upon this point of the extinguishing of fires London will no longer be content with the provisions, as regards quantity and pressure, which now prevail. The question now arises, how are the legitimate complaints as regards potable water and water for fires to be met and to be satisfied? There are many who suppose that the defects arise solely from the fact of the supply being in the hands of private trading companies, and who urge that if the undertakings of these companies were acquired by the governing body of the metropolis so as to put the whole water supply under one management, every complaint would disappear. But this could not be the case if, after the acquisition of the property by the governing authority, the present system were continued. That system, as is well known, is the obtaining of water for all purposes, either from the Thames, the Lea, or from wells. With the exception of the well supply, the water so obtained is, as we have said, put into depositing reservoirs and is filtered, and is thus treated whether it be used to flush a sewer or whether it be drunk, and is delivered at one and the same pressure, whether it be required at the basement of a house or for the extinction of a fire. Suppose the governing authority to pursue the same system and to obtain water from the same sources, obviously, though there might be some economy in management, there would be no radical change either in quality for drinking or in the quantity and pressure for fire purposes. The mere fact of corporation ownership does not change either quality and pressure, although this simple truism is very commonly overlooked. What, therefore, would be the position of the governing authority? They would speedily find they had bought a "white elephant," the possessing of which would, under any ordinary system, compel them to set about finding some unobjectionable source of supply, so that all the water might be fit for potable purposes, and would compel them to alter the pipeage and also to increase the height to which the water is raised that it might be available for fire purposes. It is commonly a sufficiently difficult thing to find an unobjectionable source of supply for a town of even 100,000 inhabitants; but in the case of London the difficulty of procuring 125,000,000 gallons a day is one of the greatest magnitude. There have been, as the section well knows, various propositions for bringing the water from the lake districts of Wales and Cumberland by means of gigantic aqueducts to London; but, after full examinations by royal commission, these propositions have been rejected; and, moreover, an opinion has been expressed by those commissions that it is not expedient the water supply of any district in England should be taken from the towns in its immediate neighborhood to supply the wants of a town afar off. But assuming that such works could be carried out, the very projectors of them admit that their cost would be enormous. Moreover they could not deliver the water at a height which would give sufficient pressure without the aid of pumping. The whole of the pumping expenditure, therefore, must be maintained, or, if an adequate pressure is to be kept in mains for fires, must be extended. Would it be wise for the metropolitan authority to acquire at a cost which from numerous instances we could readily show to the section would not be less than £25,000,000 sterling the undertakings of the present companies, when that acquisition was burdened with the necessity of forthwith discarding the sources of supply and of forthwith rearranging and modifying all the distributing apparatus and all the pumping power at an expense of at least as many millions more? We need scarcely say that, rich as London is, such outlays would never be sanctioned. But it is evident that something must be done. London must be supplied with wholesome, potable water, and it must have a greater security from fire. We have reported to the Metropolitan Board of Works, and our report is before the public, that the only practical mode would be to separate the water for drinking purposes and for the extinguishment of fire from the water which is used for all other purposes. Although, as we have said, it is next to impossible to find 125,000,000 gallons per diem of unobjectionable water, it is perfectly possible to find 30,000,000, gallons per diem, or even more than that quantity of water, which is pronounced by all authorities to be of the best possible quality for dietetic purposes—viz., the pure spring water from the chalk. This has been recognized by the Duke of Richmond's Commission in their report. Now it will be found that the most liberal allowance of water for drinking and cooking is covered by two gallons per head per diem. And by cooking we mean, not only the water used in the saucepan, but the water used in the washing of vegetables and fish, and in otherwise preparing food for cooking. Two gallons per head per diem is only 7,000,000 or 8,000,000 gallons for the whole metropolis. But in our report we have doubled this, and have allowed 16,000,000. Our proposition is that there should be made on the high ground to the north and to the south of London, reservoirs at a height of 400 feet above ordnance datum; that these reservoirs should be supplied by pumping engines, drawing their supply of spring water at distances of from eight to fifteen miles beyond the reservoirs—that is to say, in the open country; that the reservoirs should all be united by large arterial mains traversing London from north to south, and that these mains should be united by subsidiary mains. From these latter mains a service pipe would proceed to each house, delivering the water into a close vessel having a draw-off tap, and containing, according to the size of the house, from three to ten gallons, and filling up gradually, after having been emptied. On these mains also would be placed, at the time they were laid down, the hydrants for fire extinction. The plan we have proposed would, we believe, overcome every difficulty in providing for the important requirements of (*a*) the potable and (*c*) the fire extinction water; and even if the property of the water companies were acquired by the governing body, this plan, as it seems to us, is the only feasible one by which the present complaints could be satisfied. If this be so, why should not the plan be at once carried out, leaving the question whether the companies should or should not be acquired by the governing body for after consideration? The two things are distinct. The acquisition of the water companies would be strenuously resisted. It would probably involve years of parliamentary warfare, and during the whole of this time London would be left where it is as regards its water supply. After very careful calculation we have estimated that the total cost of the whole of the works necessary for carrying out this separate supply of potable water, under sufficient pressure for extinguishing fires will not exceed £5,500,000, and this includes the house fittings and also the hydrants. The annual cost attendant upon such a capital expenditure and upon the working of the undertaking will at first sight appear to be large, but, when contrasted with the expense which would be involved in meeting the improved fire requirements alone under any ordinary system, that cost will be found to be considerably less. The evidence adduced before Sir H. Selwin Ibbetson's committee showed a very curious state of facts as regards the small annual money expenditure for the preservation of London from fire as compared with other places. For the protection of the sixteen hundred millions of property in London there is accorded an annual sum of a little under £80,000. Of this £80,000, £48,000 arise from a halfpenny rate, £19,000 from a contribution by the fire-offices, at the rate of £35 for each million insured, £10,000 from the Government, and £2750 from miscellaneous sources. Let us see now what proportion this sum of £80,000 per annum for the preservation of London from fire bears to the sums expended in other large towns, and the best way of comparing will be by taking the expenditure at per thousand of the population. London, with a population of, say, 3,500000, and an annual charge of £80,000, spends £22 17s. per thousand. Paris, with 1,986,000 of population, spends £100,000, or £50 6d. per thousand. New York, with 1,098,000 spends £246,600, or at the rate of £233 14s. per one thousand—more than ten times the sum spent in London. Chicago with 550,000 of population, spends £111,093, or exactly £202 per thousand. Numerous further instances might be quoted, all of which would tend to show that London, although undoubtedly the most important city in the whole world as regards population, wealth, and magnitude of the interests involved, is the one that spends relatively the least for the extinction of fire. That London, in such circumstances as these, should suffer so little as it does from disastrous fires, we think must be admitted to be due to the excellent organization of the Fire Brigade and to the unwearying diligence of Captain Shaw. The London Fire Brigade under his management has gone on from year to year coping with and even mastering the increase in their labors arising from the increase of London. But it is clear that there is a point beyond which it is impossible for them to protect the metropolis unless in some way their powers are increased. This point is certainly reached now, and in fact has been reached for some time past. The report of Sir H. Selwin Ibbetson's committee clearly recognizes this, for they have recommended in their report that the limit of the rating power for fire purposes should be doubled, thus adding nearly £50,000 per annum to the present income. That committee further recommends that hydrants should be fitted to mains and services wherever there is a constant supply, and should follow the extension of that supply; this, as appears from the evidence, would involve an expenditure of £750,000 at least, which at 3½ per cent., would cost £26,250 per annum. They also recommend that the water system should be consolidated in the hands of a public authority, which, in dealing with the questions of constant supply, pressure, and pipeage, should be bound to have regard, not only to the convenience of consumers, but also to the requirements for the extinction of fire. This alteration of pipeage means the outlay of an unknown sum, certainly not less than one million, or an annual charge of £35,000. It further means the increase of pumping power at an increased cost, stated in evidence before this committee, of £150,000 per annum; but it has also been shown by the engineers of the water companies, and we entirely agree with them, that it would be idle to pretend to maintain such a pressure until the house fittings were all altered to suit it, and this could not be effected for less than £3,006,000, which, at 3½ per cent., amounts to £126,000 per annum. These are the sums involved in the recommendations of Sir H. Selwin Ibbetson's committee, who conclude their report by saying that "effect should be given by the Legislature to these recommendations." Let us now contrast the financial results of the two plans:—Purchase of the undertakings of the water companies, £25,000,000, at 3½ per cent., £875,000; alteration of pipeage, £1,000,000, at 3½ per cent., £35,000; annual expenditure of the companies in 1874 was £447,529; say diminished owing to the concentration of management, £47,529; leaving £400,000; increased cost of pumping for higher pressures required for jets, £150,000; interest at 3½ per cent. on cost of hydrants £26,250; interest on cost of altered house fittings, £126,000—total, £1,612,250; equal to about 16½d. in the pound on the present rateable value of the metropolis. We should mention that we have not included among these items the extra £50,000 a year expenditure on the Fire Brigade contemplated in the report of the committee, because we are convinced that if a hydrant service with proper pressure be given, not only need there be no increase in the annual charge for the Fire Brigade, but the existing expenditure will be very considerably lessened. The annual cost of the plan we propose is—interest at 3½ per cent. on £5,500,000, £192,500; working expenses for pumping and management, £32,500; total, £225,000—equal to about 2½d. in the pound on the rateable value of the metropolis. The gross income of the water companies in 1874 was, as we have stated, £1,137,000, or about 11½d. in the pound, making, with the 2½d., a total of 14d. in the pound, as against 16½d., or a saving in mere money of 2½d. in the pound, or close upon £25,000 a year. But it will be seen that by the dearer process no improvement whatever has been made in the quality of the drinking water, and that nothing has been attained except a better provision for the extinction of fire, whereas by the cheaper plan not only has this latter object been attained, but the difficulty with regard to the most important question of all, that of potable water, has been entirely overcome."

MISDIRECTED EFFORT.

By WM. A. AYRES.

Written for VAN NOSTRAND'S MAGAZINE.

When we consider the splendid engineering achievements and triumphs of mechanical ingenuity in which the present century has been so prolific, and remember, at the same time, how often the originators of these enterprises have struggled against almost insurmountable obstacles of incredulity and contempt, and only after the utter failure of their first attempts, and a profound plunge into the abyss of proved error and pecuniary disaster, succeeded in carrying their conceptions to a successful realization, it seems an ungracious task to accuse any man of wasting time in invention. There is always a possibility that he is wiser than his critics, and has a genuine discovery which is yet but dimly apprehended in his own mind, and may be aptly represented by Hermann Grimm's conception of a colossal statue in a dimly lighted hall. As the observer goes to this side or that, gigantic features and members come into momentary prominence, and it may well be discerned that some great work is there, though it is impossible to form a clear and adequate conception of the whole until more light is obtained. Some such real, yet indistinct perception of a new theory or combination or relation, may exist, and need only to be made plain by persevering study. Leaving aside then exceptional cases, it is intended to point out certain errors of purpose and method which are extremely common, and involve, in the aggregate, an immense yearly loss both to inventors as individuals and to the country at large.

As regards the purpose, that is to say, the end proposed, it is often so trivial that its pursuit can be regarded only as a waste of time. This is no question of the thing produced being in itself slight, for thousands of articles of common use, domestic conveniences, little attachments for machines, trifles that lighten labor or save time, are worthy of respect because they are of real service, even if the service is small. But there are many others for which no such claim can be made. They furnish a new means of accomplishing something that was well enough done before, and it is their novelty, not their superiority, on which the expectation of a profitable sale is based. Every hour spent in devising one of these things is an hour thrown away. No less labor and thought are involved than go to the production of something that really adds to men's facilities for living easily and comfortably, while the inventor himself suffers a positive injury in the dulling of that enthusiasm for and belief in his work which is necessary to a continuous success in any department of original inquiry. The first thing to be considered is not what can be patented and sold, but where some actual want exists and what is its nature, and to get this question clearly answered is often half the battle. Whoever follows this course has the satisfaction of doing honest useful work, and is relieved in advance of most of the risk and labor of introducing his goods to the market, for a well-considered useful invention is sure of sale unless held at some wholly unreasonable price. It is to be observed, too, that every man gains an advantage by working in the field with which he is most familiar. If his eyes are open he can see plenty of things needing improvement in connection with his daily work, and by turning his efforts in this direction, he utilizes all the slowly acquired detail information that has accumulated during his life, and would count for nothing in some new field. Its place is, however, not so much in the general conception, as in its reduction to practical shape, and it is only at that stage that the real advantage of such a course can be properly appreciated. In truth the same rule of careful consideration in advance, that applies to all other weighty attempts holds good as to invention. No man sets out to build a house without having reckoned its cost and determined the plan and material, or undertake to write a book until he has its general scope and purpose settled in his mind, or enter on a great speculation without having studied the markets and formed a satisfactory opinion of the probable course of trade. Inventors alone plunge into their subject as heedlessly as if the time and labor involved were of no value; thereby not merely delaying or preventing success, but robbing their pursuit of the dignity that should naturally belong to it, and making it no more than a child's plaything used in a childish way. It does not help the matter to say, as some do, that invention is merely their amusement. Recreation it may be, but sooner or later it is dead hard work, and that this work should be done to no purpose, and that simply for want of ordinary care and foresight at the start, is a thing hard to be borne by any one who desires the advancement of the race. It is just so much power run to waste. Granting however, that the end has been thoughtfully considered, we yet encounter errors of method that go far to neutralize whatever success might be expected from the industry or the natural parts of the inventor. Of these the most common and injurious is the neglect of previous experiments in the same direction. Intricate machines and processes are reinvented every year at a great outlay of time and money, simply because of neglect to consult records to which almost every one may in some way obtain access. The Patent Office Reports, with the detailed list of subjects containing reference by name and date to patents already issued, serve to give a general idea of what has been done, and a copy of any patent may be obtained at a slight expense, if the information gained from the Report be insufficient. Mechanical dictionaries, too, serve a good purpose, and such general books as Nicholson's "Operative Mechanic" often yield unexpected and valuable results, while technical books relating to the matter under consideration should never be neglected. A brief examination will in each case show whether anything pertinent is to be obtained, and a few hours spent in work of this kind may save the labor of weeks or months in experiments that have been already tried, and thus bring the inquirer so much nearer his goal, or by showing the futility of the search, or the fact that it has already been brought to a successful conclusion in other hands, leave him free to turn to something else in which there is a better opportunity. If by reason of distance from a library, or other cause, it is impossible to consult works of authority, publisher's lists or some catalogue like that of the Boston Public Library, should be obtained and carefully examined for technical books that bear on the matter in hand, and some of these should be selected and purchased. They are no luxury or unnecessary expense, but instruments to an end just as truly as planes or saws or chisels.

Another common error is to hurry the work, usually from impatience or carelessness, but sometimes from a wish to secure the main idea with a view to its later development at leisure. The plan, however, has too many disadvantages to be easily justified. Every detail should receive the same elaboration as the central idea before publicity is given to the invention, not merely because the first impression made is always important, but because the practical details often involve unexpected difficulties. We hear again and again of something working well in the model, and then failing utterly when submitted to the test of actual use; and this is commonly because, in the model, so many of the lesser features were neglected that the conditions of success were essentially altered. Detail work is never specially agreeable, but it is an absolute necessity in every pursuit of life, and has more to do with success or failure than any amount of brilliant general conceptions. A man who despises or neglects it can never expect any better praise than goes with the words "he might have done something if he would," or any of the pecuniary success that belongs only to a well completed undertaking. The disposition just alluded to, to secure the main idea as soon as possible, may be easily excused though seldom thoroughly justified. Its cause lies presumably in the fear that at the last moment some other person shall anticipate the result and so bring the labor of months or years to nothing. This is an ever present contingency, and doubtless self-control is difficult when one supposes that the new and worthy feature of his discovery is established. On the other hand a premature announcement tends to give observers a lower idea of the thing produced than it actually deserves. They have not the inventor's knowledge of the matter, or his interest, and do not pause to learn what may be coming in time, but passing their verdict on what they actually see can hardly be blamed for sometimes neglecting what is capable of becoming useful, because as shown to them it actually is not so. If, as exhibited, it needs constant apology and explanation they cannot be expected to stop and inquire into its prospective merits. Bring it to us when it is perfect, they say, and when that time comes they have forgotten all except that once they were disappointed by it. A thing completed before publication obtains a better and stronger patent, one that is less liable to have its value impaired by improvements at the hands of some other person, and in every respect more saleable. It should also be observed that if the whole work of inventing and perfecting be done at one heat, it will probably be better finished than if there has been an interruption and delay to secure a patent. All experience shows that there will be interruption. Not one man in a thousand, while waiting for the result at Washington, can go on calmly working out details that will be useless if his application is refused, while it is comparatively easy to develop them before the crisis of application has actually come, and in the force of an enthusiasm which has not yet halted.

Still another error is the indulgence of an unreasoning confidence. It is part of the very character of an inventor to hold sanguine views of the final result of his efforts, and certainly no despondent man, doubtful of himself and easily disheartened, is fit to attempt this pioneer work in the unknown regions of applied physics. But there is a confidence that comes from experience of one's own resources and perseverence, and another that results from ignorance or vanity; and it is the latter kind that is obtrusive. It is often taken by careless observers as a distinctive trait of the inventive mind, and, by a total misconception of the nature of the case, elevated to the rank of a virtue,—*nota bene,* a virtue for an inventor, not for his critic. Usually there is little danger that one who has escaped the various forms of error previously described will fall into this last one. If he has considered in advance what useful end to strive for, has qualified himself by investigation to work understandingly, and not merely repeat the useless or the successful attempts of earlier inquirers, and in addition to this, has waited patiently for the full completion of his work before claiming either safeguard or praise, he is not likely to cherish wholly unreasonable views of the value of what he has done. Wisdom breeds modesty, just as much in invention as in any other matter. To those who, being less wise or more excitable, find it difficult to bridle their imaginations when there is question of the future results of their own efforts it may be suggested, that hardly any new thing succeeds in exact proportion to its merits. It has already been said that what is good and meets a want is sure of a sale, but it is impossible to say that the sale shall be proportioned to the real value of the article, and not governed to a great extent by the manner in which it is put on the market. Almost always it is to this latter item that the chief credit of an exceptionally large sale belongs. Then too, only the test of experience can determine whether the new article is really as perfect or as much needed as it seems to be, while, even if this be assumed to be the case, there is a further contingency that just at the nick of time some one else may bring out a still better article for the same purpose. In fact the folly of over-confidence is so apparent that it would seem ridiculous to consider its phases in detail, were not the failing so common and well known.

Keeping now in mind the various errors that have been pointed out, it is easy to see the explanation of the common but usually unexpressed opinion about inventors, especially when we also consider that the great mass of inventions are rather petty, and not fitted by apparent importance to lend additional dignity to a calling that is usually held to require some such support. The few really great discoveries of any age stand out isolated, and hardly own kinship with the thousand-and-one trifles that come and go unnoticed by those who have no personal interest in them. An outsider must judge by what he sees and hears, and is apt to listen with amusement mingled with a little contempt to the cackling and chattering of the small fry, each of whom is sure his own goose is a swan. He cannot fail to notice how seldom the degree of success answers to the claims and predictions that have been made, how often a total ignorance of established principles is apparent, and he forgets that the great discoveries of the time are not thus trumpeted forth in advance. He puts the hasty unthinking multitude for the whole body, and is apt to ignore even what is really accomplished by these noisy vaunters of their own powers. He does a double injustice, but it is certainly done under provocation. Yet it is safe to say that on personal acquaintance few men stand higher, even with those who have no technical knowledge of their business, than honest and modest searchers in this particular field of investigation, though it is their misfortune to be handicapped by a name that to many persons suggests a certain want of balance, and a tendency to over-sanguine and exaggerated statement.

The practical importance of a recognition of the faults most common to a class lies in the hope of their amendment. Those which have here been pointed out are clear and can not be disputed. They are not of a sort that require argument to prove the desirability of a change, but each one carries on its face the stamp of its character, and would be hard put to it to find even that good side in which hardly anything is wholly deficient. The absolute waste of useful energy, frittered away and brought to nothing for want of proper direction, is almost incalculable, and could a tangible showing of it be made at the year's end, the lesson so taught would be as impressive as ever came from tongue or pen.

REDUCTION OF THE HEIGHT OF WAVES BY LATERAL DEFLECTION UNDER LEE OF BREAKWATERS.

By THOMAS STEVENSON, F. R. S. E.

From "Nature."

When a wave encounters an obstacle, such as a breakwater, the portion which strikes it is either entirely destroyed or reflected seawards, while the portion which is not so intercepted passes onwards, and, spreading laterally under lee of the barrier, suffers a reduction of its height. In the second edition of my book on Harbors I expressed regret that no attempt had been made, so far as I was aware, to obtain any numerical value of this reduction of height derived either from theory or experiment, although the extent of shelter which is to be gained by the erection of our great national breakwaters depends entirely upon its amount.

From a few observations taken in the sea under lee of the breakwater in Wick Bay, and from some experiments made in a large brewer's cooling vat, it apprared that after passing round an obstruction *the reduction in the height of waves varied as the square root of the angle of deflection.* The approximate formula given in my book was

$$x = 1.00 - .06 \sqrt{a}$$

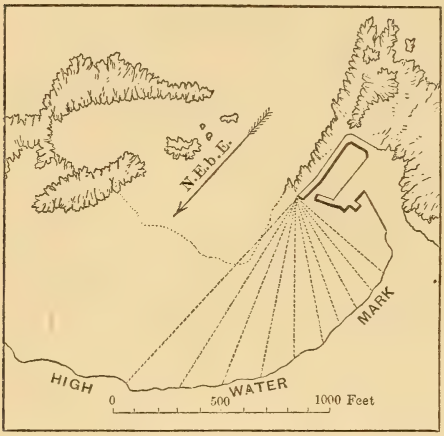
where x represents the ratio of the reduced to the unreduced wave, and a the angle of deflection.

On a recent visit to North Berwick, the finely-curved storm and tide marks, traced out on the sandy beach under lee of the promontory at the harbor, reminded me of some observations I had made many years ago at other parts of the Firth of Forth. These observations which were, however, very imperfect, had for their object the determination of the reduction of the waves by ascertaining the positions in reference to the center of divergence of different parts of the line of high water mark where, of course, all the wave forces become *nil.* If a beach throughout its whole extent consists of easily moved materials, such as sand or gravel, the incursion made at any one place by the sea will obviously depend upon the force of the waves which reach the shore at that place, providing the materials of the beach are homogeneous. In other words, the heavier the waves at any part of the shore the farther inland will the high-water margin retire beyond the tide mark of more sheltered places. And where the waves vary in height owing to some local cause, as, for example, the existence of a sheltering promontory, the high water mark,instead of being straight and parallel to the prevalent waves, will assume a curved outline.

At North Berwick the projecting promontory at the harbor, shelters a small bay, or rather *bight,* from the heaviest waves that fall on that part of the coast. The waves, therefore, are deflected at the pierhead, from which point as a center, each section of every wave, taking its own divergent direction, runs its course till its energy is expended at high water mark. The maximum effect on the beach will consequently be in the line of direction of the undeflected swell, and the minimum effect will be in the direction of the landward end of the promontory where the waves are most deflected from their natural course. Under these conditions, supposing the particles of sand to be of uniform size and of the same specific gravity, the high water margin must assume, as it does at North Berwick, a curved outline owing to the inequality produced by deflection on the height of the waves.

If the distance between the pierhead and the high-water mark, measured parallel to the usual direction of the undeflected swell (shown by the arrow in the diagram), be assumed as unity, that distance may be regarded as the measure of the amount of work that the undeflected part of the wave has been able to do, inasmuch as its force has been wholly expended within that distance in driving the beach landwards. The varying lesser distances between the same point and other parts of the high water mark may, in like manner be regarded as representing the work that has been done by the varying lesser forces exerted by the different parts of the wave after being deflected. It is, no doubt, true that the undeflected wave has the full force of the wind to help it, while the deflected has not; but in so far as relates to the engineering aspect of the question this effect, even though it had been much greater than it is, would be of no importance, as the same conditions hold true with an artificial as with a natural breakwater.

I may mention in corroboration of the views that have been expressed that in the course of my practice as an engineer I have, at different exposed parts of the coast, had occasion to fill up a small creek with soft materials produced by works of excavation at an adjoining part of the shore. In the course of time the whole of these artificial deposits have, in every instance, been removed by the waves, and the former line of high-water been restored. By analogy, therefore, we must believe that if the bay at North Berwick were in like manner filled up artificially with sand as far seawards as the pierhead, we should find, after a certain number of storms had occurred, that the whole of the sand had been washed out and the former line of high-water reproduced. If this be true, then the different distances between the pier-head and the high-water mark at North Berwick may justly be regarded as the measures of the varying forces of the different sections of the deflected wave under lee of the promontory.



The first column in the accompanying table shows the angles of deflection, while the second gives the measurements from the pier-head to the high-water mark as taken from the Ordnance map of North Berwick. The directions in which these measurements were taken are represented by dotted lines on the accompanying woodcut. The third column shows the ratios of those measurements to unity. The fourth column gives the ratios of the heights of the deflected wave calculated by the formula $x = 1 - .06\sqrt{a}$, and the last the *plus* and *minus* differences. Though the employment of the *square-root of the angle* may perhaps be regarded as somewhat unusual, the formula as given is nevertheless more convenient for use than a logarithmic spiral formula, which might give nearly the same results.

(*See Table on following column.*)

Angles of deflection θ.	Distances from center of deflection to high-water mark v.	Ratios of measurements.	Ratios of Heights of waves calculated by formula.	Differences.
°				
0	1150	1.00	1.00	.00
10	1000	.87	.81	−.06
20	920	.80	.73	−.07
30	840	.73	.67	−.06
40	735	.64	.62	−.02
45	700	.61	.60	−.01
50	675	.59	.58	−.01
60	600	.52	.53	+.01
70	570	.50	.50	.00
80	555	.48	.46	−.02
90	530	.46	.43	−.03

Although it is possible that the agreement of the measurements with the results calculated by the formula may turn out to be to some extent accidental, yet the results can hardly be regarded as very far from correct. And in a case of such importance to the maritime engineer where we have so very few direct observations of the waves in the open sea to guide us, and where it is undeniable that all such observations are invariably found to be excessively difficult to get, and even when got prove often unsatisfactory, any contribution to our knowledge, however imperfect, may be considered of some value; and all the more when, as in this case, the curve traced out on the beach is the result of long-continued action produced by innumerable storms.

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The trustees of the British Museum are in treaty, says the *Anthenœum*, for the purchase of a copy of the largest book in the world. Towards the close of the seventeenth century the reigning Emperor of China appointed an Imperial Commission to reprint in one vast collection all native works of interest and importance in every branch of literature. In the beginning of the following century the Commissioners completed their labors, and were able to lay before the Emperor a very palpable proof of their diligence in the shape of a compilation consisting of 6,109 volumes, entitled "Kin ting koo kin too shoo tseih ching," or "An Illustrated Imperial Collection of Ancient and Modern Literature." Only a small edition was printed off in the first instance, and before long the greater part of the copper types which had been cast for the undertaking were purloined, and the remainder were coined into cash. The trustees of the British Museum have entered into negotiations for the purchase of a copy, just offered for sale at Pekin.

SPONTANEOUS COMBUSTION IN FACTORIES AND SHIPS.

By CHARLES W. VINCENT, F.R.S.E., F.C.S.

From "Journal of the Society of Arts."

Strictly speaking, there is no such phenomenon as spontaneous combustion. The inflammation of various organic and inorganic substances without the immediate contact of any ignited matter, which has given rise to the term, is, nevertheless, as certainly the result of some direct act or acts which can be accurately traced, as is the firing of a lucifer match when struck on a rough surface.

The discovery of what the active agents are, and the circumstances under which they occasion visible ignition, has long been the subject of research, and, one by one, they have been so fully investigated that there should be little difficulty in at once assigning the specific cause to every species of "spontaneous combustion." It is, nevertheless, so difficult to shake off the trammels of incorrect observation, and theory founded thereon, that there are even now many cases in which combustible substances catch fire, in spite of great precaution having been taken to prevent such catastrophes, and this because care and attention has been wholly directed to prevent something which is not likely to take place, and no thought has been given to that phenomenon upon which all combustion, as is commonly understood, depends, *i.e.*, the combination of the components of the combustible body with oxygen.

It is not here intended to treat of fires arising from friction of machinery; lighting of explosive gases or vapors; the action of the sun's rays through unintentionally disposed lenses, such as may be formed by glass globes filled with water or knots in a pane of glass; firing of disseminated flour, or cotton, &c.; but to examine some of those cases when starting with the combustible at the ordinary temperature, it gradually becomes hotter and hotter, and at last takes fire without, to the unscientific observer, any apparent reason for its so doing.

Many animal and vegetable substances heaped together in a large mass, when neither too wet nor too dry, are liable to take fire by the heat produced by their own decomposition. Haystacks often afford examples of this. Heaps of any dry fibrous vegetable material, or of dry wool, cotton, cloth, calico, paper, sawdust, &c., when smeared with grease, oily matter, or particularly a drying oil, such as linseed, and left undisturbed in a warm dry place, are tolerably sure to be consumed by fire. Coal, piled in quantity, and left too long in one place, or confined in great bulk in the hold of a ship on a long voyage, frequently behaves in a similar way. A comparison of the conditions under which the spontaneous combustion of these three classes of bodies—viz., damp organic matter, dry oily organic matter, coal—takes place, may be useful by suggesting the means of preventing them from happening.

In a rough way, we may describe heat as being the effect of a blow upon an immoveable body. A nail on an anvil struck by a hammer becomes hot, the visible motion of the hammer is converted into the invisible "mode of motion" (to use Dr. Tyndall's happy phrase), heat. If the nail were perfectly free to move, the blow would produce no heat. A wave of the hand in the air, though made with the exertion of the utmost muscular power, produces no sensible heat, because the air is set in motion. The same force applied to strike a blow upon enclosed air—as, for instance, in the fire syringe, or in a force-pump—produces a very sensible quantity of heat. The air is less free to move, and the heat produced is in proportion to the destruction of its motion. Were we to follow out, to its fullest extent, the effect of the blow on the free air, we should find that heat resulted here also; each particle of air struck drives before it the particles surrounding it. But, though air is very mobile, it is not perfectly so; amongst other restraining agencies, it is somewhat held together by the force of gravitation. The circle of motion, though extensive, still has its limits; the full force of the blow received is, therefore, not imparted to the surrounding molecules, and the amount of force which thus disappears is manifested as heat, but the concussions, being feeble and spread over a large space, are imperceptible to our senses. The resulting heat is, nevertheless, quite as great in quantity as when the air is struck in a chamber with rigid boundaries.

Imagine a vessel devoid of air, to which air is suddenly admitted; the air will rush in, will strike against the side of the vessel, its motion will be arrested; the force which produced that motion cannot be destroyed, it is converted into heat.

Freshly burnt charcoal is as a vacuous chamber to many gases, since it has the power to condense within itself many times its volume of some of them. The falling in of these gases and their condensation is a succession of blows followed by a stoppage of motion, heat is produced, and in some cases makes its presence unpleasantly manifest. Condensation of a gas, by whatever means, implies application of force followed by cessation of visible motion; every such condensation must, therefore, produce heat.

Freshly reduced iron is in a similar way vacuous to oxygen. If the iron is so finely divided as to afford room for the gas to strike a sufficient number of blows at it, the clashing of the atoms produces enough heat to raise the temperature to that at which chemical combination of the iron and oxygen can take place, and this with so much rapidity that the metal takes fire and burns spontaneously.

In the instances already given heat is clearly the product of physical action. The heat produced by chemical combination is also the result of blows given and received. The stroke of a hammer concentrates and expends a certain amount of force in one blow; friction expends the same amount of force in producing a succession of blows less in intensity; in condensation by charcoal the gas expends force in producing a succession of blows, still feeble, but infinitely more numerous. When a substance burns in air, the action which takes place is of precisely the same character, the blows being, however, indefinitely multiplied.

The combustible is first heated to a particular temperature, varying in different substances, called its point of ignition; to continue burning it must continue at this temperature; if cooled below this point the combustion would not be maintained. The initial temperature is that at which the particles of the bodies uniting receive sufficient momentum, when the molecules of oxygen and the combustible rush together.

At the moment of combination their motion is checked and converted into heat and light. The greater the rapidity of the action the more intense is the heat, and the less its duration; the slower the action the more feeble is the heat, and the greater its duration. The quantity is in each case precisely the same.

This meeting together of the atoms of bodies is an expenditure of force; a combustible in burning loses something; the chemist of the past century termed it "phlogiston," those of to-day "energy." The heat produced is something more than that due to the condensation in volume; this surplus is the heat due to the loss of energy.

Heat and energy are equivalent values. All the elementary bodies have a specific power of performing work; if this power is lessened, as it is when they enter into combination with each other, it is lessened by the expenditure of part of their power for doing work, and heat results.

The combinations so formed may be broken up and reconstituted by the accession of some element or combination of elements, whose power of working is not exhausted, and again heat may be evolved, but at last a point is reached when the constituents of a body have lost all power of motion, and then no further change can take place till they are torn asunder, and their energy restored. This can be done by heat; given a sufficient amount of heat, there is no compound whose constituents are so tightly bound together but that they would fly asunder.

The heat developed by the disruption of any compound substance capable of combustion is exactly equivalent to the amount of heat absorbed in its construction. The heat developed by the combination of elements is exactly equal to the amount of heat required to tear them apart if once united, and no change of constitution of any substance can take place without a certain amount of heat being either absorbed or evolved. If the substance undergoing this change were formed by the expenditure of a large amount of heat, and is passing into combinations which can be formed by the expenditure of a less amount of heat, then heat is evolved; and if this change of state takes place with rapidity, incandescence ensues. If, on the other hand, during the formation of the compound, heat was evolved, then, in order to bring its components into their original state, the same amount of heat must be restored which they parted with when they entered into combination.

There is a powerful factor which can restore to many burnt (that is combined) elements their original working power, by enabling them to absorb gradually, and by slow degrees, the amount of heat expended in their combination; this is vegetable life. The seed, the plant, the tree, derive all their sustenance from tightly combined elements, which they combine to a more complex, but a more loosely held together state; in point of fact they bring them more nearly to their elementary condition. Plants do this by absorbing the radiant heat of the sun, and by some incomprehensible action of that mysterious power, "life," bringing it to bear upon the inert compounds in the air and the soil.

So long as a plant is living the heat which falls upon it, if not so great in intensity as to be incompatible with the life of the organism, is absorbed and stored up in the complex, yet feebly combined, substances which form woody fibre, starch, the vegetable juices, &c.

Walking on a midsummer evening beside a grove of trees which has been exposed all day to the glare of the sun, no heat is felt; placing a hand on the trunks of the trees, or on the leaves, they are found to be of the same temperature as the air; the heat has all been absorbed; but touch a dead trunk which has been exposed to the same sun, and, though it has been some time shaded, it will be found to be hot; the heat which fell upon it is now radiating back into space. At the foot of Drayton park, Highbury, is a crescent of houses upon which the sun falls for a great part of the day; on setting oneself in the focal point of the crescent some time after the sun has gone down, the concentrated rays of heat make the position conspicuously hotter than the rest of the street.

Dead or mineral matter is incompetent to store up the solar heat, or in any way to make it available for chemical decomposition. The work of the sun, unassisted by life, is purely mechanical. A vegetable whilst alive eagerly absorbs heat, using it for "unburning" burnt matter, but so soon as the life has departed, the dead body appears as eager to part with the heat it had gathered; the elements composing its structure are, as it were, rejoicing in their new powers, and are as eager in their turn to enter into more fixed states, in doing which they will part with the heat lately bestowed upon them. It is for this reason that substances of vegetable origin are liable to spontaneous combustion. From the moment that life is extinct decay (which is really only another term for combustion) sets in, carbonic acid, water, &c., are evolved, the plant crumbles into a blackish brown mould, and is ultimately entirely destroyed. In the course of this change, the whole of the heat which had been collected by the plant is dissipated.

It need not, therefore, be a matter of surprise that bodies containing so much bound up heat as do vegetable fibres of all kinds, more particularly hay, cotton, &c., coal of all kinds, oily rags, greasy sawdust, varnish rags used by painters and polishers, the waste at railway lamp stores, &c., should be particularly liable to take fire when stored in bulk.

Spontaneous combustion of these substances can only take place when they are so packed that small increments of heat may be stored up. If they are freely exposed to the air, the heat evolving action which goes on with it may be increased rapidly; but, as when the hand is waved in mobile air, the infinitesimal blows are free to rebound and react over so large a space that their effect is dissipated. But when their striking space is limited they act like the struck air enclosed in the fire syringe, their heat motion is intensified by concentration, and ultimately throws the whole mass into such commotion that the moving power of the molecules is manifested as light as well as heat.

But there must be a beginning to this

heat motion. Oiled rags and bodies of that kind derive the initial movement from the oxydation of the oil. Perfectly dry vegetable fibre at ordinary temperatures oxydises extremely slowly, it is practically indestructible by atmospheric oxydation. Mummy cloths possess much strength after the lapse of two or three thousand years. Oils, however, are formed at the expense of a still larger amount of heat than vegetable fibre, the elements comprising them are combined much more loosely, and they are consequently ready to form more stable compounds, and release their bound up heat on very slender provocation. Oils absorb oxygen with great avidity, becoming viscid, and, in the case of drying oils, solid. In doing this they give out heat, which is due partly to the condensation of the oxygen, and partly to the carbon and hydrogen of the oil having by combination with the gas lost part of their energy, in fact they have been partially burnt.

In a heap of oiled rags, greasy sawdust, or matters of that kind, air, though not excluded, has only limited access to the center, the heat of oxidation is not there carried off by convection currents; and though not at first great, goes on increasing, because it is surrounded by barriers of non-conductors, *i.e.*, bodies incapable of transmitting the heat motion, and so spreading it over a larger area, until at length it reaches a temperature of about 323° C., when the oil takes fire, and speedily fires the whole mass of which it is a part. The combustion being caused by absorption of oxygen, it is sufficient to exclude the air from the oily materials in use in factories, to effectually prevent their becoming so hot as to take fire. To revert to our parallel, the oils are vacuous to oxygen, absorb it with great rapidity, become hot, and the finely divided vegetable fibres become sufficiently hot to enter into combination with the oxygen on their own account.

In the spontaneous firing of hay-stacks, there is no oil to act as an absorber of oxygen, and thus to raise the temperature to that point at which the mutual action of the fibre and gas can take place. Here water is the active agent, though not in the sense in which it is commonly supposed to operate.

It was formerly held that plants underwent decomposition by direct absorption of oxygen from the air, and that the effect of water in promoting the action was to soften the tissues, and allow free access of the air to fresh surfaces, as those acted upon fell away.

Thoroughly dry wood and thoroughly dry hay are safe from destruction by fermentation, decomposition, or "eremacausis," as it is termed somewhat indifferently by various writers. But add water to the woody fibre, and the change takes place with greater or less rapidity, occurring most rapidly in young, spongy plants, which admit the air freely, and contain a good deal of albuminous matter more slowly in slow growing hard woods.

Fermentation produces heat, and, if the heat is shut in by non-conducting matter, as it is in a haystack, it will, under favorable circumstances, accumulate to such an extent as to cause the hay to take fire; and as fermentation cannot happen unless moisture is present, the water is, therefore, blamed for the combustion. This view, however, does not express the whole truth. Carbon combining with oxygen to produce carbonic acid evolves 7,900 units of heat (Andrews), this unit being the quantity of heat required to raise one kilogram of water 1° C. This amount is invariable.

Grass, when growing in the field, is covered with numberless microscopical organisms, whether vegetable or animal matters not for our present purpose; it is sufficient to state that amongst their number are very many which, in the course of their growth, consume oxygen and give off carbonic acid in great quantity, and derive their nutriment from substances of vegetable origin. Such organisms as these are the active agents in every kind of decomposition by fermentation.

If hay were stacked immediately it was cut, it would, in a very short time, become mouldy; its organic structure would be broken up by the feeding upon it of the parasites which prey upon it, and its power of affording nutriment to cattle would entirely disappear. Experience, gained in the remotest ages, has taught the farmer that these detrimental effects can be entirely stopped by the process called "curing the hay," *i.e.*, by choosing hot weather in which to cut the grass, and then allowing it to remain exposed to the sun's rays until it has become thoroughly dry. Such hay not only keeps perfectly good, but does not take fire, however long it may be stacked.

What has the farmer done? He has by evaporation removed the water from the interior of the stalks and leaves, and has by so doing cut off from the germs of ferment the means of absorbing into their own organizations the sustenance which the grass would otherwise have afforded them. But he has done something more than this. He has, by exposing the grass to the high temperature of the summer sun, destroyed the life in a very large number of the germs themselves. Whilst the grass was living, they were preserved from the intensity of the heat, by reason of its absorption by the grass; but, so soon as the grass was cut, it lost its power of gathering up the heat, so that by far the greater part of the germs have lost all chance of doing further mischief.

Hay, properly cured, is not much damaged, even if afterwards partially moistened while being stacked, nor is it liable to take fire. Suppose, however, that the hay, instead of being thoroughly dried, was stacked whilst still damp; or that though dry, it had not been exposed long enough to the sun's rays to kill the larger part of the microscopical germs, and subsequently was accidentally wetted. The germs would grow, would throw off millions of offshoots, would prey upon the grass, drink the water, and breathe the oxygen, which, however closely packed the hay may be, nevertheless penetrates to the innermost recesses of the stack. In doing all this, they would produce carbonic acid, and this fact alone is sufficient to prove that heat must be generated, since carbon and oxygen cannot be combined without giving forth heat, any more than carbonic acid can be broken up by the plant without absorption of heat.

The ferment germs also induce other dangers of the vegetable structure in which heat is evolved, but if their formation of carbonic acid stood by itself, it would be sufficient proof that they are heat-producers.

Fermenting germs feed by choice on the juices of the plant; this peculiarity of their growth aids spontaneous combustion, inasmuch as it leaves a large part of the carbon in a fine state of division and the better able to condense and combine with oxygen.

The center of a fermenting hay-stack, when on the point of combustion, is blackened as if charred, and a sudden influx of air is almost certain to cause it to blaze. This is well known by farmers, and if on testing a suspected stack (by thrusting into it an iron rod and withdrawing it) they find the middle dangerously hot, they admit air to cool it by degrees, and with much caution.

The heat produced by the germs has no means of escape; it therefore accumulates, and at last, as in the case of the oily waste, varnish, rags, &c., a temperature is reached at which the woody fibre and oxygen can combine directly, vivid ignition takes place, and conflagration of the whole stack ensues.

That this is the real course of events, and that water, without germs, is powerless to set hay, cotton, jute, and other woody fibres on fire, is indirectly proved by the mode of making a hot-bed practised by gardeners when manure is scarce. Having taken a quantity of hay sufficient for the required purpose, this is spread out in a thin layer, and, according to the weather, watered several times a day for several days, or perhaps a week. It is then raked together in a heap, and left to ferment, and soon becomes hot enough to be used for the required purpose. Or, a space of ground the size of the intended hot-bed is covered with a thin layer of hay, and watered; after a short time a second thin layer of hay is put on and watered, and so on until the hot-bed has the required depth; the frame is then put on, and in a short time a very high degree of heat is attained.

If water were the only thing required to set the fermentation at work, it would surely be sufficient to water the heap when thrown together, but this is found to be inadequate to cause the hay to become heated. Why? Because the hay, if properly cured, has had so many of the germs killed that enough do not remain to start and keep up an active fermentation. No doubt, in time, the whole mass would ferment, but it would not do so until fresh germs had been imported into the heap, or the few that remained had time to multiply. When the hay is spread out in a thin layer, a large number of germs settle upon it, and when afterwards heaped up. the greater part are carried to the middle of the heap, where the heat can be retained, instead of being for the most part on the outside, as they would be if the hay were piled up before it was watered. Since then the heat which fires a haystack is the result of oxydation produced by fermentation, and inasmuch as it has been abundantly proved by Pasteur and others, that fermentation is decomposition of organic structures brought about by the growth and multiplication of living organisms, whose seed is ever floating in the air, it necessarily follows that to these minute seeds the combustion is due.

Thanks to the indefatigable exertion of our foremost scientific men, the world at large is at least beginning to appreciate the immense power of destruction possessed by these living creatures, some of whom, though so minute as to be invisible to the human eye when assisted by the most powerful microscope, can yet light up a beacon to alarm a whole country side; whilst others of their brethren can in a few weeks devastate a continent, killing more cattle and men than would fall in an extensive, prolonged, and bloody war.

The most serious cases of spontaneous combustion are in ships carrying coal. These have, of late, been so numerous, and have so often occurred in despite of manifold precautions, that a Royal Commission was recently appointed to make inquiry into the causes of these terrible catastrophes, and to suggest remedies which it may be possible to adopt for preventing and guarding against them. Numerous Board of Trade inquiries had already been made into casualties caused by explosions or fires in coal-laden vessels, but without any improvement in the safety of shipment resulting therefrom, until, at length, the Board declined to hold any more, for the reason that the findings of their courts (which invariably took the form of an exoneration of the ship's officers, and a recommendation in favor of better ventilation) appeared to be entirely disregarded, both by shipping and underwriting interests.

It was evident, from this state of things, that the coal shippers conceived that they knew more of the causes of these accidents than did the assessors of the Board of Trade. The Salvage Association thereupon urged that an inquiry should be held by a Commission, consisting of men of high scientific attainments, and especially acquainted with the nature of coal in all its conditions, and of men practically acquainted with coal, both at the pit and in the ship's hold. They pointed out that the result might be to fix, with a certainty absolute or qualified, the causes of this dangerous combustion, thereby attaching to proper persons the responsibility for due precautions.

The Commission sat, and has now issued its report, from which much valuable information as to the nature of these disasters can be obtained, and, to a certain extent, modes of preventing them, though this part of the subject still requires most serious consideration, since at present the carriage of coal for a long distance at sea must be regarded as hazardous, whatever precautions may be adopted.

At the commencement of the inquiry, they were struck by the circumstance that by far the greater part of the casualties happened on board ships which were on long voyages. In 1874, among 31,116 shipments, amounting to upwards of 13,000,000 tons of coal, the accidents were 70. But, of these shipments, 26,000, amounting to over 10,000,000 tons, were to European ports. This left 60 casualties among only 4,485 shipments, amounting to 2,955,831 tons of coal, to Asia, Africa, and America.

Again they were startled to find that the proportion of casualties traceable to spontaneous combustion increases, *pari passu*, with the tonnage of the cargoes. This becomes still more apparent when the European trade is deducted.

There were in 1874:

2,109 shipments with cargoes under 500 tons, in which 5 casualties occurred—or under $\frac{1}{4}$ per cent.

1,501 shipments with cargoes between 500 tons and 1,000 tons, in which 17 casualties occurred, or over 1 per cent.

490 shipments with cargoes between 1,000 and

1,500 tons, in which 17 casualties occurred, or 3½ per cent.
308 shipments with cargoes between 1,500 and 2,000 tons, in which 14 casualties occurred, or over 4½ per cent.
77 shipments with cargoes over 2,000 tons, in which 7 casualties occured, or 9 per cent.

The casualties in vessels bound to San Francisco were the most remarkable. Deducting vessels under 500 tons (in which no cases of spontaneous combustion were recorded) the returns show 9 casualties out of 54 shipments. These also increase in proportion to the tonnage of the cargoes, till we find that out of 5 ships with cargoes of over 2,000 tons sent to San Francisco in 1872, 2 suffered.

Careful thought might have predicted results of this kind, from a consideration of the nature of the substance carried, the mode of carriage, and the place to which it was going.

So long ago as 1852, Graham pointed out that the tendency of coals to spontaneous ignition is increased by a moderate heat, and gave examples. For instance, in one case coal had taken fire by being heaped for a length of time against a heated wall, the temperature of which could be easily borne by the hand; in another, coals ignited spontaneously after remaining for a few days upon stone flags covering a flue, of which the temperature never rose beyond 150° Fahr. Coals thrown over a steam-pipe ignited, &c. At the Chartered Gas Works, coals piled against a brick wall two feet thick, of which the temperature did not exceed 120° to 140° Fahr. became ignited. Neither did it appear to matter whether the coal was Lancanshire and sulphurous, or Wallsend and bituminous. If they were exposed to this very moderate heat for a short time they were sure to ignite.

Coals conveyed through the tropics are certainly put in this state of danger. When coal takes fire spontaneously, it is invariably in the center of the heap of small coal at the foot of a hatchway, or in the middle of the cargo, in this respect resembling the spontaneous combustion of haystacks, oily waste, &c., and from hence it may be inferred that the increments of heat which cumulate in vivid combustion are very small, since they require to be held prisoners by impassible barriers of non-conducting matter, or they would escape.

Coal in small quantity, and in a cool place, never ignites spontaneously, but it does not, therefore, follow that all the conditions leading up to spontaneous combustion are absent, but only that one of them, and that an all-important one, the means of accumulating of heat, is absent, since the barriers interposed to its escape are not sufficiently close-fitting.

The large ships to San Francisco had to encounter elevated temperature, and at the same time the coal was in great mass; they were, therefore, much more liable to accident than those carrying smaller quantities, and for shorter distances.

It has already been pointed out that, whilst wood is living, moderate heat, so far from causing its destruction, promotes its growth; it is the heat which disappears, not the plant. When the wood has ceased to live, moderate heat dries up its juices, renders it brittle, and ultimately causes its complete disintegration and combustion if air is supplied, though the process is exceedingly slow. Some years ago, the sawdust packing round a steam pipe at the Queen's Printing Office, Shacklewell, was found to be charred. Wooden beams resting against hot plates which never reach the boiling point of water, are sometimes found to be charred, but oxygen being of necessity excluded by the position of the wood, combustion does not happen.

At the ordinary temperature of the air, oxygen has so little action upon wood that it is practically indestructible. In coal, however, the wood has undergone changes which render it far more readily affected by the oxygen of the air than it was heretofore, and it must be borne in mind that if once a combustion is sufficiently rapid to overcome the cooling effect of currents of air, it will proceed with increased vigor, and the ignited coal will burn, not only in the interior of the cargo or heap, but on its surface also. Knowing, then, that coal if kept in bulk at a temperature slightly raised above the common is sure to ignite, the question still remains,—How does it attain the degree of heat at which active combination can take place? And at what temperature do the combinations of the carbon and oxygen, the hydro-carbons and the oxygen begin to take place? In other words, what is the temperature of the initial point of the combustion, and how is it reached? Many explanations have been given. The well-known fact that heaped-up iron pyrites in shale, when wetted, often causes the combustion of the pile, as in alum making, has been used as an argument against the shipment of "brassy" coal, *i.e.*, coal containing these pyrites. But, supposing this were so, and that the pyrites were disseminated through a part of the cargo in sufficient quantity to cause evolution of heat when wetted, this would account for but a small number of the cases of spontaneous combustion of coal, since by far the larger number happen with coal free from pyrites.

Condensation of oxygen by carbon, which was referred to at the outset of this paper, is a far more likely mode of attaining the initial temperature. This is pointed out by Professor Abel and Professor Percy in the appendix to the report of the Royal Commission. As already stated, carbon, in a finely-divided state, has the power of condensing oxygen within its pores; now, to condense a gas force is consumed and heat is produced. In the fire-syringe a piece of tinder is set on fire be the heat evolved by the condensation of the air. When charcoal condenses oxygen heat is liberated, and if the charcoal is freshly burned, the rapidity of the action will produce such an amount of heat as to cause the chemical combination of the oxygen and carbon, when, of course, combustion takes place with evolution of light and heat. The initial temperature of the action is here due to the sudden squeezing together of the gaseous molecules, for if the air be admitted to the freshly-burned charcoal by slow degrees no combustion takes place.

The Appendix suggests:

"The tendency to oxydation which carbon and carbon compounds, existing in such a substance as charcoal, possess, is favored by the condensation of oxygen within its pores, whereby the very intimate contact between the carbon and oxygen particles is promoted. Hence the development of heat and the establishment of oxydation occur simultaneously; the latter is accelerated as heat accumulates, and chemical action is thus promoted, and may, in course of time, proceed so energetically that the carbon or carbo-hydrogen particles may be heated to igniting point.

"This explanation has a direct bearing upon a spontaneous ignition of coal. The more porous and readily oxydizable portions of coal, which are known to be more or less largely disseminated through seams from different localities, undergo oxydation by absorption of atmospheric oxygen, and by the exposure of large surfaces to its action, and the heat developed by that action, will accumulate, under favorable conditions, to such an extent as soon to hasten the oxydation and the consequent elevation of temperature, until some of the most finely divided and readily inflammable portions actually become ignited."

Water does not assist in the spontaneous combustion of coal, except where pyrites are concerned. There is much misunderstanding as to the part played by water in the changes leading to spontaneous combustion. The water itself is not decomposed, as some people have imagined. The heat evolved during the combustion of hydrogen and oxygen, during their combination to form water (the heat of the oxy-hydrogen blowpipe) must be supplied before they can be again torn apart, so that, so far from water being a producer of heat, it is likely to be a consumer.

Moreover, experiment goes to prove that coal requires no initial temperature for its combustion, and that the supposition of condensation by porous coal, though it may take place, is unnecessary for its spontaneous combustion. The ties holding the constituents of coal together, which in the living plant were so strong that they defied the power of the sun to rend them asunder, have now become so weak that the oxygen of the air, even when no hotter than 55° Fahr., can seize upon and combine with at least some of the carbon, forming carbonic acid. Air blown through a tube, filled with coarsely powdered coal, into baryta water furnishes a considerable amount of barium carbonate in a very short time. Other circumstances may aid, but it is sufficient to prove the production of carbonic acid to make it certain that heat is set free, and if escape of the heat is barred, it must accumulate, till at length it reaches the point at which combustion becomes visible, and in too many cases uncontrollable.

As there is no amount of coal which may not be intensified by free radiation, so there is no limit to the heat which may be produced by concentration, or, in other words, by stoppage of all radiation except to one common point. Siemens' admirable regenerators act on the principle of continuous passage of heated gases through passages, where the gases are deprived of heat; when the heat producers go forth to do their work, they are first made to pass through these heated passages. In addition to their own proper heat, they thus convey forward the stored-up heat, and the most intense heat yet met with for practical purposes is attained.

It is to be feared that, in ventilating the cargoes of coal-ships, the principle of Siemens' regenerator is infringed upon, to the great damage of the cargo. Air is forced through the coal, oxydation and heat follow throughout its course, the heat is absorbed by the coal, and the air, as it is continuously forced in, passes over surfaces which are becoming hotter and hotter, the air is itself heated, and the work of combustion, once begun, goes on more and more rapidly.

In view of these facts, there is small wonder that the uniform recommendation of the Board of Trade Assessors, "To ventilate the cargoes," should have met with cavalier treatment. The subject is, however, yet far from being fully understood. Evidence has been collected from most trustworthy sources, and a clear understanding obtained of the various conditions under which spontaneous combustion of coal takes place. What is now wanted is a thorough experimental investigation of the causes of the spontaneous combustion. The reasons already given are probably correct, but they are supported by the feeblest experimental support, and until this is strengthened, we can neither speak with the necessary boldness in reproving the actions which lead to the lamentable losses of good fuel, good ships, and, but too often, of good men; nor can we decide as to the proper means for preventing their occurrence.

NOTE ON THE MISAPPLICATION OF CORRECT THEORIES.

By JOHN D. CREHORE.

Written for VAN NOSTRAND'S MAGAZINE.

In the August number of this Magazine for the current year, I gave a formula connecting the ultimate resistances of material to tension, compression, and cross-breaking; and applied the formula to various kinds of iron, wood, and stone, citing, with other data, three results of what I supposed to be experiments made by Mr. William Kent at the Stevens Institute of Technology.

In the Magazine for October, Mr. Kent disclaims the experiments, and asserts that I say "nothing of the formula given with them," which formula he "discovered about two years ago." I confess that Prof. Thurston's statement of the service rendered by Mr. Kent does not necessarily imply that the latter is the author of the experiments which he compared with theory; but the equality of the numbers assigned to wrought iron (60,000 lbs. both in tension and in compression) left no doubt in my mind that Mr. Kent made the whole comparison, including the experiments. For I know of no authority giving wrought iron, in general, so high a strength in compression as it has in tension; yet there may be iron possessing this singular quality.

Even Professor Wood, whose "*average* figures" Mr. Kent employed, says in "Resistance of Materials," page 53 (edition of 1871): "Hence it appears that the crushing resistance of wrought iron is from $\frac{1}{2}$ to $\frac{3}{4}$ as much as its tenacity."

In regard to the formula which Mr. Kent discovered, I suppose it was Professor Thurston's; and, as I had no use for it, and did not consider the particular manner of deriving the Navierian equation of any consequence, I passed the formula by with a mere reference to the article containing it.

Mr. Kent further remarks that "It is not, therefore, safe to assume the absolute theoretical correctness of the formula. . . The hypothesis of Navier, that the sum of the moments of compression is equal to the sum of the moments of tension, . . . is disputed by late authorities."

Now I suspect that no late authority disputes this equality of the total moments on each of the two sides of the neutral surface. But the thing derived is, that the resistance offered by the material at every point of the cross-section, is continually proportional to the displacement of the material at the point, or to the distance of the point from the neutral surface. In other words, the objection lies not against the equality of the two members of the equation,

$$\tfrac{1}{6}\mathrm{C}bx^2=\tfrac{1}{6}\mathrm{T}b(h-x)^2,$$

if they express the total moments on the compressed and extended sides respectively, but against the two members themselves as not expressing those moments.

Without doubt, since each of these expressions is established on the supposition that the displacing forces at all points are proportional to the distances of the points from the neutral surface, they will fail to express the total moments in cases where this proportionality does not exist. Hence the equation,

$$\tfrac{1}{6}\mathrm{C}bx^2=\tfrac{1}{6}\mathrm{T}b(h-x)^2=\tfrac{1}{6}\mathrm{B}b(\tfrac{1}{2}h)^2,$$

is not applicable to such cases.

The truthfulness of the formula should not, therefore, be called in question any more than we should deny the soundness of the laws involved in Maclaurin's and Taylor's Series, because these laws are not applicable to all functions. For, as Price says, "We are trying to make that which is true only when certain conditions are satisfied comprise all, whether such conditions are fulfilled or not."

Considered with reference to a universal application, and the comprehension of all possible conditions, the above formula undeniably comes far short of the ideal completeness suggested by Prof. Thurston.

Experiments, however, have shown that the formula is practically applicable to many varieties of material used in construction, and it is confidently expected that the much needed experiments, still to be made, will increase rather than diminish the number of cases to which the formula may, with sufficient accuracy, be applied.

On the other hand, were the perfect formula already written, and ready for universal application, it is manifest that it could never, to its last refinement, be used in *predicting* the strength of a beam or bar; for no one beam can, in accordance with the severe requirements of the perfect formula, be said to be exactly like any other beam, so that the experimental constants required for any given beam will differ from those required for any other. Of this fact Mr. Kent and Prof. Thurston seem well aware; the former stating that "A complete theory must necessarily include the effect of the 'flow' of the material after it has passed its elastic limit, which will modify the law of proportionality of resistance to distance from the neutral axis, and will vary with every material;" and the latter to the same effect. "The increase of resistance, after passing the elastic limit, will not be similar for both forms of resistance, and each substance will probably be found characteristically distinguishable from every other."

As regards what must be considered different "material" or "substance," in applying the perfect formula, there can be no question that blocks from different parts of the same tree are different material, as well as blocks of iron from different parts of the same rolled bar. And the exactions of the perfect formula must preclude our using with certainty, for one half of a rolled beam, constants deduced from experiments upon the other half of the same beam. Nor could we safely affirm, from testing a joist cut from the south side of a tree, what would be the strength of a joist of equal dimensions, cut from the north side of the same tree. Neither does the common formula enable us to do this with absolute accuracy.

The engineer is to be congratulated upon the facts, that, in practice, such extreme refinement of distinction in the quality of material is not indispensable, and that through the labors of such men as Prof. Thurston and the Government

Testing Commission (which, let us hope, an enlightened Congress will liberally sustain) the time is not far remote when enough will be known, from experiment upon the strengths of materials used in construction, to enable the designer invariably to assign economical and safe proportions.

IRON VERSUS WOODEN CROSS-TIES FOR RAILROADS.

By J. L. WEYERS, C. E., Brussels, Belgium.

Journal of the Iron and Steel Institute.

Before entering into details regarding the practical working and the economy inherent on the substitution of iron for wooden cross-ties, in its bearing on the interests of railroad companies, we believe it will not be deemed uninteresting to hear, in as few words as possible, what the very important results of such a transformation would be to the iron trade in general.

According to Steurmer's statistics, the railroad mileage of the world may be estimated at the approximate figure of 181,500 miles. Including double track, but deducting half the American lines on account of the cheapness of lumber in that country, we find that the adoption of light wrought iron cross-ties, such as we shall have occasion to speak of further on, would necessitate the consumption of no less than 13,600,000 tons of wrought iron, exclusive of bolts, nuts and other accessories, and that the value of this iron calculated at the low rate of £ 6 per ton, would not be less than £81,600,000 sterling.

Taking only the smaller field of Europe alone with its 114,800 miles of continuous lines, including double track, these would consume 8¼ millions of tons of iron at a minimum cost of £ 49,500,000 sterling.

For Great Britain alone without her colonies, a similar computation indicates a necessity for 700,900 tons of iron equivalent to a money value of not less than £ 4,200,000 sterling.

Such figures are sufficiently eloquent in themselves, we believe, to make it quite useless for us to comment, in any way, on the immense advantage to be derived by the iron manufacturer from the substitution of iron for the actual perishable and costly wooden ties and to stimulate him by every means in his power to bring about such a desirable result.

We therefore proceed at once to some details in relation to the practical feasability of promptly realizing this radical transformation, which sooner or later, as the forests of the world disappear, will undoubtedly impose itself upon all nations.

The idea of establishing metallic railways has been before the public for many years, the difficulties to be contended with having however long delayed the introduction on a large scale of such a system, notwithstanding the constantly rising price and increasing scarcity of timber.

The problem to be solved by the engineer was to combine simplicity with solidity and safety, as well as with economy of maintenance. Let us review rapidly, following in this the classification of M. Couche, the various systems proposed to this day.

Four main ideas have been followed:

1° The first of these consists in supporting ordinary double-headed or vignole rails by cast iron bearings or supports.

2° The second consists in laying a vignole rail on a concavo-convex iron cross-tie having much the external shape of the old wooden one.

3° The third consists in the adoption of longitudinal iron sleepers instead of cross-ties.

4° The fourth consists in letting the rail, whose shape has been modified, rest directly without intervening appliances, on the bare ballast.

I. SYSTEM WITH CAST IRON SUPPORT.

This category, which has been tried on a considerably extensive scale, includes the bell-shaped bearers of M. Greve, the discs of M. Henry and the Barlow support.

The first of these seem to have given

the most satisfactory results, having been applied to the Isthmus of Suez railroad and to those of Madras, Calcutta and the Punjab.

The main objection is the high cost of building and maintenance of the way, as the ballast employed must necessarily be very finely divided. Besides, the weight is supported by too small a contact surface, so that it becomes necessary to exaggerate the breadth of the basis as well as the thickness and cost price of these discs.

II. SYSTEM WITH IRON CROSS-TIES.

Very many different types of these have been offered, more especially since the hollow rolled beams of the Zorès system were introduced by the forges of Franche-Comté.

Here, in every instance proposed, the mode of attachment of the rail to the cross-tie has proved defective and ineffectual, so much so indeed, that to this cause alone must be attributed the non-generalization of iron cross-ties on all railroads.

Several forms of these ties have been experimented on in Belgium by the Grand Central railroad and in France on the Eastern railroad, as also on the Paris-Lyon-Mediterranean railroad, the Paris-Mulhouse railroad, the Paris-Strasburg railroad and the Northern railroad.

These trials resulted in showing clearly two things; first that iron tires are quite as stable as wooden ones, and secondly that when the road is in constant traffic they remain as free from rust, as is the case with the rails themselves, the vibrations imparted to the metal having this effect.

Among the best forms of iron ties which have been proposed, we will name that of M. Vautherin, 10,000 of which were laid on the Grand Central railroad of Belgium in 1869.

The one studied by the the French enneers of the Chemin de fer de l'Est is also good.

It is impossible for us in this short notice to exhibit all the various methods put forward, for fastening the rails to the cross-ties. They number over forty.

Every one of these modes of attachment has the same defect, the strain is made to bear on the sides of the appertures cut in the thin tie, and the natural consequence of this is, that at the end of a few days these orifices gradually increase in size until the jars and jolts to which they are submitted give exit to the attachments. Quite recently M. Vautherin has proposed to replace his preceding system by a Ω shaped spring, but we fear, that such a delicate appliance is ill fitted to the rough usage of a railroad and to the atmospheric influences.

III. LONGITUDINAL CONTINUOUS SLEEPERS.

It is in England but more especially in Germany that railroads have been constructed with longitudinal sleepers or *longrines*, as they are called on the continent.

As early as 1853 M. Donnell began applying this system to the Bristol and Exeter railroad where, after many modifications had been made to it, and persevering experiments tried also on the Bridport railroad, it was finally abandoned in 1860.

The cause of the non-success was laid to the weakness of the elements used in this construction; the sleepers weighing only sixty lbs. per yard while the gradients were heavy and the ballast too coarse. Other causes must however have worked against the application besides those enumerated.

Longitudinal sleepers made of several parts or sections have been proposed by MM. Scheffler, Kostlin and Battig, Jordan, Daelen, Heusinger, Altzinger and a few others; these may be stated in a general way to present the following defects:

1° Being made of too many parts.

2° The necessity existing of having the various portions adapted to one another by special workmen in special shops.

3° The difficulty experienced in keeping the track level and the necessity of bending all the rails and angles in curves.

4° The necessity of tearing up the track for the renewal of the rails, which is both a long and a costly operation.

5° The difficulty, not to say impossibility, of reversing the rails.

6° The largely increased cost of maintenance of the road over the ordinary system.

Four only, out of the many longitudinal-sleepers roads are at present prominently before the public; these are the ones proposed by M. Heusinger, Chief Engineer to the Hanoverian railroad; by M. Hohenegger, Chief Engineer to the N. W. railroad of Austria; by MM. Kirsch and Degreef, Engineers to the Belgian Grand Central and by M. Hilf the Chief Engineer to the German state roads.

The system which has obtained the most general acceptance is the latter one.

M. Heusinger has adopted a sleeper having a longitudinal central ridge which enters into a Ω shaped rail eight centimèters in height. Opposite each joint he inserts small iron saddle pieces. The worst feature of this plan is the necessity of having thirteen different shapes of saddles so as to be able to regulate the distance between the rail and ridge and to give the first the necessary inclination in curves. This arrangement only allows a shifting space of from four to six millimèters for the rail, which is evidently insufficient.

The steel rail recommended by M. Heusinger appears too light to us and the number of pieces in his system too numerous.

The Hohenegger plan differs only from the Hilf's by the shape of the sleeper, the central ridge being done away with, in view of a better setting on the ballast, but the number of various pieces required is still more numerous than in this last.

MM. Kirsch and Degreef claim to need no more than eight different types of material for the construction of their metallic way.

They use a rail having the following shape Ω and with uplifted outer edges which are required for their special attachment. On straight track they employ clamps and bolts for this purpose, but in curves, a rack with seven teeth five millim. deep and two millim. broad at top, between any of which the tightening bolt can be inserted. This ingenious method allows eleven different positions to rails in curves each of these being 2¼ millim. from its neighbour. The rails used are thirty feet long and the sleepers five inches less so that water on the track escapes freely by their extremities.

The principal objection to this system is the difficulty of rolling a good rail of the proposed section. The impossibility of using fish plates is also objectionable. The rail proposed weighs only seventeen kilogr. per mèter. This rack arrangement needs the sanction of continued experience before being finally adopted.

The Hilf system which has been accepted for the strategical railroad running from Berlin to Coblentz, Trèves and Thionville, and which exists on the railroads of Nassau and some other German lines, and is at present day being tried on thirty miles of the Belgian state railroad, is too well known by engineers to need a long description.

The vignole rail used weighs 25.8 kilogr. in Germany and 29 kilog. in Belgium per mèter, and rests on iron sleepers which have a length of 8.96 mèters on straight lines, and 8.86 mèters in curves. The height of the rails is 108 millim. with a thickness of web of only ten millim. Stay bolts twenty-six millim. in diameter screwed into the web of the rails serve to establish the breadth of gauge of the road, and to procure the necessary slant.

The ends of these sleepers rest on cross-ties of the same section as themselves 2.60 mèters in length.

Both sleepers and ties have to be submitted to differential punching of holes according to their being placed in curves or not. This necessitates the use of eight different graduated gauges answering to curves of respectively 200, 240, 330, 450, 620, 875 and 1,600 mèters radius.

Both the laying down of the rail and its renewal are easily effected, but the whole operation must be conducted by an able foreman, with experienced workmen and having a workshop at their disposal. The rails and sleepers are brought to the track adapted to one another on a car with a double armed crane which lowers the whole fixture on to the ballast. This is rapidly done, but any mistake in the forwarding of any one section for another, may have serious consequences.

A great defect of the Hilf system consists in the need of no less than forty-eight stay bolts for every nine mèters of road, and these do not always present the desired security, as they are constantly submitted to a twisting strain as also to a cutting action.

The greatest inconvenience, however, to this system, which, with all its defects, is the best which has yet been practically applied, consists in the necessity for the differential punching of the sleepers and cross-ties which makes it a necessity to have continually in stock no less than thirty-three different types so as to allow of prompt repairs in case of need.

This small thickness of the web of the rail we can hardly recommend.

IV. RAILS LAID ON THE BALLAST WITHOUT ANY EXTRANEOUS INTERMEDIATE SUPPORT.

This bold venture which we owe to M. Barlow and M. Hartwich has unfortunately, for its inventors, not stood the crucial test of practical application. This is partly attributed to the defects of the material employed, partly to a tendency to lateral or outside pressure on the ballast and partly to defects in the bolting.

M. Brunel, who made considerable use of the Barlow system, seems to be the only party who has expressed himself as satisfied with it.

After having made a very careful and complete study of all the preceding systems and the causes of their failures, M. Potel C. E. of Brussels has lately tried to grapple with the difficulties to be overcome, and he has, we sincerely believe, done so in a manner which deserves the very serious consideration of all railroad men as well as of iron masters and members of this Institute.

This new and latest system, which is brought before the public for the first time, consists in the use of a wrought iron cross-tie resting simply on the ballast. Its shape is semi-elliptical, the base measuring 285 millim. across; the table on the top 100 millim. and the height being seventy-five millim. The table has a thickness of eight millim. These cross-ties weigh fifteen kilogr., about thirty-five lbs. per mèter.

The rail is fastened to the cross-tie by a quite novel and most ingenious, although simple contrivance, called by the inventor the "Jawbone-chair" or coussinet-machoire. This is formed of two portions which hook in to one another and whose shape is such that they hold the rail as if in a vice.

Half of this chair is firmly fastened to the cross-tie by means of a bolt and preferably on the outside of the track.

The other half hooks into the first beneath the cross-tie and is moveable. An ordinary eye-headed bolt entering through the hole of the tie adapts itself to the end of this moveable arm and by its successive tightening compensates for any downward wear of the rail.

The laying of a track of this description presents no difficulties whatever, being rapid and inexpensive and confined to the tightening or loosening of a bolt.

To the above arrangement, M. Potel has added another apparently small, but really important contrivance, which does away with all necessity for differential punching of the cross-ties (or even, if desired, of such in longitudinal sleepers) and which resides in the adaptation of a washer of hexagonal shape placed below the bolt head, but which, instead of being pierced by a central orifice has the bolt hole placed eccentrically so that, by rotating this plate after loosening the bolt, any one of the sides of the hexagon can be brought to bear on the shoulder of the jaw.

In the model exhibited the variation can be modified to the extent of three millimèters at a time up to fifteen millimèters according to the side of the washer presented (thirty millim. for the two chairs on a same cross-tie).

In order to avoid longitudinal sliding of the rail, the half of the chair situated beneath the tie is covered by means of a plate which, while it serves to strengthen the weakest portion of the tie, fits by means of a sort of finger into the ordinary end notches of the rail.

The cross-ties are slightly arched while hot, in the middle, so as to give them an incline of 1 120th.

Believing the subject of this paper to be of some importance, we will now sum up what we consider the special advantages of the system just described :

1° It does away with the use of wood in the construction of railroads;

2° It is easy of application everywhere and can be adapted without special workmen;

3° The rails can be reversed at any time without difficulty;

4° The stability of the rail is great from the number of points of contact which exist between it and the chair;

5° The drainage of the middle track is as perfect as possible;

6° Repairs and renewals are of the easiest and simplest kind;

7° Transportation of material is facilitated as only one sort of cross-tie is used for either straight or curved road.

8° The system adapts itself equally well to any section of rail actually in use;

9° Solidarity of parts is obtained without any excess of rigidity;

10° The number of various parts of the appliance is limited to a minimum, as it comprises only: 1 cross-tie, 1 Jawbone-chair, 2 small washers and 2 bolts.

11° The economy in the laying and maintenance of the track as compared with other systems.

We append a table showing the comparative weight and number of pieces composing the Hilf and the Potel systems, the figures for which, this last gentleman has kindly placed at our disposal for the present report.

TABLE.

Pieces needed to build nine mèters of track by the Hilf system:

Pieces.				Kilogrammes.
2 rails of 9 mèters,=18 mèters weighing			29 kilog.	522,000
2 sleepers of 9 mèters.=	“	“	29.370	528.660
1 cross-tie of 2.60 meters		“	29.370	76.360
1 stay bolt	6.050			6.770
4 inclining plates	0.180			
52 bolts, each	0.310			16.120
44 clamps (ordinary), each	0.160			7.040
4 shoulder clamps, each	0.620			2.480
16 fish plate and cross-tie bolts				8.800
2 pair fish plates	3.820			15.280
8 angles	0.590			4.720
136			Total weight	1188.230

$\frac{136}{9}$=15 1/9 pieces for one mèter

Total weight for one mèter of track 132.025

NOTE ON THE USE OF STEAM JACKETS.

By M. H. Resal.

Translated from "Comptes Rendus" for Van Nostrand's Magazine.

It is known that the use of steam jackets on engine cylinders have effected a saving of fuel to the extent in some cases of nearly twenty per cent.

I have endeavored to account for this fact which at first seemed paradoxical. For this purpose I have considered the case of two similar cylinders supplied from the same boiler, one of them furnished with a covering of poor conducting material, and the other furnished with an envelope of steam, and protected by a good non-conductor of heat.

I have assumed that in the first cylinder the steam is expanded without either loss or gain of heat; and in the second that the pressure during expansion is in accordance with the law of Mariotte.

The admission of steam is the same in both cases. A numerical example given below gives results as follows:

1st. The ratio of the quantity of heat transferred into work to the quantity of heat furnished by the boiler, during admission and expansion, is slightly less in the first case than in the second.

2d. If we suppose that the heat of vaporization of the steam condensed at the end of the expansion, to be borrowed entirely from the cylinder during exhaust, and then restored in some way by the boiler, we find that the work performed by the heat is greater in the second case than in the first; and that the difference is between a fifth and a sixth. This accords with experience.

I have assumed the pressure in the cylinder during admission equal to the boiler pressure. This is nearly correct when the inlet pipe is short, of large diameter and well covered, especially when the steam ports are large and quickly opened as in the Corliss and Sulrer engines.

Let

$A=425$ kilogrammeters: the mech. eq. of heat.

p_0=the boiler pressure 10333×6 atmospheres.

t_0=the corresponding temperature.

$$\rho_0=0.001164\left(\frac{p_0}{10333}\times 760\right)^{0.943}=3,2837$$

=the specific gravity of steam, dry at the temperature t.

$r_0=496$ the heat of vaporization of this steam.

$n=0.80$ the proportion of dry steam admitted to the cylinder.

For these quantities which vary with the expansion I have used the same letters, replacing the index 0 by 1. I have neglected the waste space; also the slight variation in specific heat of water depending on temperature. I have assumed the heat of the feed-water to be 12°.

1st. case. The steam expands without loss or gain of heat

In this case we have

$$(1)\quad \frac{n_1 r_1}{273+t_1}=\frac{n_0 r_0}{273+t_0}+2.3026 \log. \frac{273+t_0}{273+t_1}$$

and for the calorific equivalent of the work due to the expansion of a kilogram of vapor.

$$(2)\quad \frac{1}{\mathrm{A}}\int p\frac{dn}{\rho}=t_0-t+n_0 r_0-n_1 r_1 + \frac{1}{\mathrm{A}}\left(\frac{n_1 p_1}{\rho_1}-\frac{n_0 p_0}{\rho_0}\right)$$

If we assume $\frac{p}{p_0}=\frac{1}{12}$, we find

$$(3)\quad \left\{\begin{array}{l} t_1=82^0,\ n_1=0.721,\ \rho_1=0{,}316 \\ \frac{1}{\mathrm{A}}\int p\frac{dn}{\rho}=60.2 \end{array}\right.$$

and for the ratio of volumes at the beginning and end of the expansion

$$\frac{n_1}{\rho_1}+\frac{\rho_0}{n_0}=9.32$$

It is to be especially noticed that the proportion of water primarily in the cylinder is nearly double that at the end of the expansion.

The ratio of the heat transformed into work to the quantity expended during admission is

$$\frac{\frac{1}{\mathrm{A}}\left(\int p\frac{dn}{\rho}+\frac{n_0}{\rho_0}p_0\right)}{n_0 r_0+t_0-12+\frac{n_0 p_0}{\mathrm{A}\rho_0}}=0.1655$$

If we add to this denominator the heat of vaporization of the water in the cylinder at the end of the expansion, as proposed above, the ratio will become $=0.1325$.

2d. case. The steam expands according to Mariotte's law.

To assume the same conditions as above we must have

$$p_0=9.32\ p$$

whence

$$t_1=88°$$

and by hypothesis

$$\frac{n_1 p_1}{\rho_1}=\frac{n_0 p_0}{\rho_0}$$

or by the formula

$$n_1=n_0\left(\frac{p_0}{p}\right)^{0.057}=0.909$$

and the proportion of water contained at first in the cylinder has diminished about half.

The ratio of heat transformed into work to total heat admitted is

$$(7)\quad \frac{\frac{n_0 p_0}{\mathrm{A}}\left(1+2.3026 \log.\frac{p_0}{p}\right)}{r_1 t_1+t_1-12+\frac{n_0 p_0}{\mathrm{A}}\left(1+2.3026 \log.\frac{p_0}{p}\right)} =0.1673.$$

This differs but slightly from the similar ratio of the preceding case.

If we add to the denominator the value of the heat of vaporization $(1-n_1)r_1$ the ratio becomes

$$=0.1557.$$

Finally, dividing this value by the one found under similar conditions in the preceding case, (0.1325) we obtain 1.175 for the ratio of work of a kilogram of steam under the two conditions of steam jacket or no steam jacket. The economy of its use is represented by .175 which is nearly $\frac{4}{23}$ a value between $\frac{1}{5}$ and $\frac{1}{6}$ as stated in the beginning of this article.

The Astronomer Royal states that the accuracy of chronometers continues to improve, but very slowly, and not uniformly. The "trial number," exhibiting the errors of the chronometers on the 1876 trial, is lower than that of any year except 1873. Have we reached the limit of perfection in mechanical timekeepers?

RESULTS OF EXPERIMENTS ON THE SET OF BARS OF WOOD, IRON AND STEEL, AFTER A TRANSVERSE STRESS.

By WM. A. NORTON, Professor of Civil Engineering in Yale College.

From "The American Journal of Science and Arts."

At intervals, during the last two years, I have carried on a systematic series of experiments, with the view of determining the laws of the set of materials resulting from a transverse stress under varied circumstances. The experiments were made with the testing machine which I devised several years since, for the purpose of experimenting on the deflection of bars under a transverse stress. A detailed description of this machine is given in the Proceedings of the American Association for the Advancement of Science, Eighteenth Meeting, Aug., 1869, (p. 48). The depressions of the middle of the bar experimented on,—while under a transverse stress, or remaining after the stress has been withdrawn—are measured by it to within $\frac{1}{10000}$ of an inch. The experiments on set have been fully discussed in two papers read before the National Academy of Sciences, Washington, (April, 1874 and April, 1875). The first paper set forth the results of the experiments on bars of wood, and contained a detailed account of the course of experiments instituted for the purpose of detecting instrumental errors, and of the precautions taken to reduce the incidental errors, from variations of temperature and other causes, to a minimum. The second paper discussed the experiments on the set of bars of wrought iron and steel; which gave results generally similar, under corresponding circumstances, to those obtained with wood. I propose, in the present communication, to give a succinct statement of the general conclusions that follow from the whole discussion.

The experimental investigation was prosecuted under three general heads :

I. Sets from momentary strains.

II. Sets from prolonged strains.

III. Duration of set; and variation of set with interval of time elapsed after the withdrawal of the stress.

Each of these embraced several special topics of inquiry. The bars used in most of the experiments consisted of one of white pine, 3 inches by 3 inches and 4 feet long; another of wrought iron $\frac{1}{4}$ inch wide, 1 inch deep, and 4 feet long; and a third of steel of the same dimensions. The discussion of the entire series of experiments has brought out the following results, as alike applicable to bars of wrought iron, steel, and white pine.

1. The immediate set,—that is, the residual deflection which obtains immediately after the transverse stress is withdrawn,—increases in nearly the same proportion as the stress applied; until this exceeds a certain amount, beyond which the set increases according to a more rapid law than that of proportionality to the strains. It is to be understood here that the varying strains are applied at considerable intervals of time.

2. The immediate set augments with the duration of the stress, up to a certain interval of time. In the experiments with white pine, the duration of strain which gave the maximum immediate set, varied with the strain, from ten minutes to one hour. The immediate set resulting from a prolonged strain, was found to be from five to nine times as great as that which succeeded a momentary strain.

3. The residual depression below the original line of the bar, is greater if the stress is reached by a series of increasing weights than if the full stress is directly applied.

4. When the same strain is repeated on the same bar, after a short interval of time, the set first obtained is not augmented, unless the load applied exceeds a certain amount, varying with the material and dimensions of the bar. With loads greater than this limit each repetition of the load augments the total set. The amount of the increase varies with the interval of time since the previous application of the load and the number of previous applications.

5. The set, or residual depression of the middle of the bar, experiences marked variations as the interval of time subsequent to the removal of the stress in-

creases. When the immediate set is less than about 0.0005 inches it passes off in a few minutes (10 m. or less). When it is greater than this it habitually varies as follows: it invariably decreases for a short interval of time, and then ordinarily increases for a longer interval, with moderate fluctuations. The period of decrease varies from about 5 m. to 20 m.; and is the longer in those instances in which the stress is prolonged. The subsequent increased set, or augmented depression of the line of the bar, may attain in less than an hour to an amount even greater than the set observed immediately after the stress is withdrawn. In some of the experiments the depression increased until it came to be about double that first observed. The proportionate increase of set is usually, however, much less than this. This increase of set is eventually succeeded by another decrease. These remarkable fluctuations observed in the line of the bar were more conspicuous in the experiments with white pine, than in those with iron and steel. The difference was, however, only in degree. Under similar conditions the general character of the fluctuations was the same whichever material was used. The fluctuations observed with the bars of iron and steel, as well as with the wooden bar, far exceeded any errors to which the observations were liable. They were also much too slow, and too prolonged, to be regarded as simple vibrations of the bar, consequent on the removal of the downward pressure.

6. Abnormal variations from the general law of variation of the set just noticed, may occur under especial circumstances. Such deviation were observed after the bar had been subjected to repeated strains from day to day. Under these circumstances the bar may be in such an abnormal condition that the set observed immediately after the stress is withdrawn may pass off rapidly, and the line of the bar may even rise considerably above the position held when the stress was applied—though not above its original line some days previously, before any strain was applied.

7. When the load, or stress at the middle of the bar, exceeds a certain amount, the set resulting from one or more applications of the load on any one day is not only still discernible on the following day, but the actual result may be that the middle of the bar may be lower than at the close of the observations on the previous day. Such effects were observed in the experiments with white pine, when the load was sufficient to produce a longitudinal strain on the upper or lower fibers of 500 lbs. per square inch; and in the experiments with the steel bar, resting edgewise on its supports, when the strain on the outer fibres amounted to 1500 lbs. per square inch.

8. Repeated applications of the same load, from day to day, are attended with an indefinite augmentation of the residual depression of the middle of the bar, if the load exceeds a certain amount. When a smaller load is similarly applied the set attains after a few days to a maximum, and subsequently subsides more or less. The load answering to the critical point here referred to is, obviously, the maximum safe value for a variable load that can be applied with an indefinite number of repetitions to the bar. In the case of a white pine stick (3 in. by 3 in., and 4 feet long) the experiments show it to be less than $\frac{1}{4}$ the theoretical breaking load. Under repeated applications of 500 lbs. (or about $\frac{1}{4}$ the theoretical breaking weight) the set steadily increased from day to day—that is, the middle of the stick became more and more depressed—during the entire period (seven days) that the prolonged effects were noted. Under daily repetitions of a load equivalent to $\frac{1}{10}$ the breaking weight, the depression increased for three days, and after another interval of three days the stick had recovered its original line. The depressions here referred to are those which obtained on the morning of each day just before the first application of the stress on that day.

9. In connection with the phenomena of set which have been signalized, it is important to note that, during any interval in which a bar was kept under a transverse stress the resulting deflection commonly experienced a continual variation. In general the deflection increased as the strain was prolonged. But the deflection of the steel bar in some instances diminished, under the prolonged strain. This unusual result was appar-

ently dependent on some molecular condition of the bar, induced by previous strains. The comportment of the wrought iron bar, as regards varying deflection under a continual strain, was not particularly examined.

It is also noteworthy, in this connection, that the deflection resulting from any single stress was found to be more or less dependent on the previous strains to which the bar had been subjected. The wooden bar, when it had been exposed to a cross strain not long before, was generally in a condition to suffer a greater deflection than it had before experienced under the same load. The same was true of the steel bar during several successive days of experiments with loads of 4 lbs. and 6 lbs.; but as the result of these repeated strains the bar came eventually to be in a condition in which each renewal of the stress gave, for the most part, a less and less deflection.

10. It is apparent from the foregoing experimental results, that every application of a transverse stress to a bar must induce some change in its molecular condition, which continues, with variations that may be either progressive or fluctuating, for a greater or less interval of time. The duration of sensible influence varies with the amount and duration of the stress. For the smaller strains it is but a few minutes; for the larger several days. The prolonged influence of strains applied from day to day to a bar, was apparent from the fact that the same stress did not on different days produce either the same deflection or the same set. It was strikingly shown in the experiments with the steel bar by causing the bar, to which loads had been repeatedly applied for several previous days, to rest on its opposite side, and comparing the deflection and set with those obtained immediately before the reversal. It was found that the deflection produced by $18\frac{1}{3}$ pounds was $\frac{1}{16}$ greater than the deflection produced by the same weight just before the reversal; and the set obtained was now many times greater than before. The deflection also now increased with a prolongation of the strain, whereas it before decreased. Also the set now increased for a considerable interval of time after the withdrawal of the strain, whereas it before decreased.

11. There was no discernible limit of elasticity, revealed by the experiments, with either wood, iron, or steel. A perceptible set obtained, with each material, immediately after the stress was removed, however small its amount, until the set fell below the lowest possible determination of which the apparatus was capable, viz: $\frac{1}{10000}$ of an inch, as the experiments were ordinarily conducted. To test the question still farther, the delicacy of the measuring apparatus was largely increased, by the adaptation of a device for magnifying the movements to be observed; and it was found that the least perceptible immediate set was still limited only by the capability of detecting, with the apparatus, minute displacements.

If we take for the limit of elasticity the condition of things at which a *permanent* set is obtained the case is different. Thus it was found that the set which subsisted after the pine stick (3 in. by 3 in. and 4 feet long) had been loaded at its middle with 200 pounds ($\frac{1}{10}$ the theoretical breaking weight), eventually passed off entirely. This was the case whether the stress was momentary or prolonged, and whether it was applied but once or repeatedly. But with a load of 500 lbs. a permanent set was obtained, as the result of a single application of the stress; and repetitions of the stress were attended with a continual increase in the depression of the middle of the bar. It may accordingly be affirmed that a practical limit of elasticity exists, but not a theoretical one.

12. If a bar, on the withdrawal of a transverse stress, fails to recover its original line of position, or, technically speaking, has a set, it is plain that its integrant molecules have not returned precisely to their original positions, and that the distance between contiguous molecules have either increased or diminished—increased in the line of the longitudinal fibres that have experienced a tensile strain, and decreased in the line of those which have experienced a compressive strain. Now we have seen that, as the result of a series of increasing transverse stresses, the set increases continuously with the stress, from the

lowest amount capable of detection with the measuring apparatus employed. We must therefore conclude that, after the application of a series of increasing strains, in which the molecules are relatively displaced by minute fractions of their intervening distances, they take up, when the strain is removed, a series of new positions of equilibrium, differing by excessively minute degrees from those previously occupied. We may draw the same conclusion from the experiments on the set produced by a series of direct tensile and compressive stresses, made by Hodgkinson, Chevandier and Wertheim, and other experimenters. This general conclusion, to which experiments on set, under every variety of strain, conduct leads to the inevitable inference that *the effective forces exerted by the molecules on one another have suffered some change of intensity, in consequence of the stress applied to the bar under experiment.* Viewing the residual displacement of the molecules, in their relative positions, as a mechanical problem, we are constrained to regard the effective molecular forces that take effect at a given distance, as having acquired a different intensity. We have confirmatory evidence of this induced molecular condition of the bar in the fact that all the diverse effects, which may ensue on subsequent applications of a transverse stress, are found to be either less or greater than those previously observed under similar conditions.

13. The fluctuations which have been noticed as occurring in the set with the lapse of time, reveal the fact that the change in the intensities of the effective molecular forces, which results from the temporary application of the stress, is not permanent but fluctuating; and may, according to the amount of the stress applied, rapidly pass off, or, after a partial collapse, be slowly recovered again.

It should be observed, however, that the curious fact of the increase of set which ordinarily succeeds the first sudden fall, may be in part attributable to the gradual propagation inward of the greater disturbed condition of the molecules of the upper and lower fibres.

14. The general correspondence in the phenomena of set and altered deflection, that obtain with different materials, altogether precludes the idea that they may result, either wholly or in a considerable degree, from irregular strains subsisting in certain parts of the bar before the stress is applied, and which are more or less modified by the stress; as some persons have conjectured. The change that supervenes must be a general one, or one in which all the molecules participate, though in diverse degrees according to the amount of molecular displacement. The especial character of the change, for each individual molecule, must depend upon the kind of strain to which the molecule is exposed, whether tensile, compressive, or shearing; and not on the nature of the material subjected to strain.

15. If, as experiment has established, when the distance between two contiguous molecules has been forcibly altered, the molecules, when again left to their mutual actions, no longer exert, at the same distance, effective actions of the same intensity as before, it is apparent that *the molecules in the act of displacement have experienced some change, either in their dimensions, or in their internal mechanical condition.* This change must result from the change that took place in the mutual action of the molecules when they were urged nearer to each other, or separated to greater distances. It must be experienced by the ultimate molecule, whether this be identical with the integrant molecule or not—that is whether we regard the integrant molecule as a single ultimate molecule, or as a group of ultimate molecules. For it is plain that a group of ultimate molecules could not undergo an internal change, that abides after all actions have ceased, unless its constituent molecules have suffered a change, by reason of which they no longer act upon one another with the same intensities of force as before.

It is well known that with Physicists the "chemical atom" has come to be replaced by the "ultimate molecule." Of the probable physical constitution of the ultimate molecule different conceptions have been formed. To those Physicists who regard it as made up of a limited number of precisely similar atoms, endued with unvarying forces—of attraction at certain distances, and repulsion at other distances—I leave it to reconcile this conception with the legitimate inference to be drawn from experimental results, that the ultimate molecule is liable to a change of mechanical or physical condition, with every slight displacement it may experience—a change which subsists after the constraining cause of the displacement has ceased to act; and may, under different conditions, either be permanent, or gradually subside with fluctuations.

MACHINES AND TOOLS AT THE PHILADELPHIA EXHIBITION.

From "Journal of the Society of Arts."

Dr. Anderson's elaborate report on machine tools is pervaded throughout by a perfectly enthusiastic admiration of the American exhibits. None of the countries of Europe were strongly represented, in fact the reporter thought the department "could hardly be regarded as an international competition." Though Dr. Anderson will not allow that we are falling behind, he yet admits that our exhibits at Vienna gave foreigners the impression that we were being passed by the Americans, and that the show we made at Philadelphia will certainly strengthen this impression.

A special feature in this section was that all machine tools were tested for accuracy if their owners wished it. Nearly all the Americans agreed to this, but none of the foreign exhibitors, except one Canadian and one French firm. Some machines were also tested for rapidity of work; for example, a locomotive cylinder was bored out, the ends of the bores recessed, and the flanges turned and faced in three hours and a half. On the whole, the results of the tests were such as to take the judges by surprise. The saws were not tested, time not sufficing for the exhaustive series of trials which alone, it was thought, would be of any real value.

Another special feature of the Machinery Hall consisted of the great pair of Corliss engines that drove the machinery, and the arrangement of shafting and gearing by which the power was transmitted. All this gear worked with perfect smoothness and quiet. The spur teeth were all cut by a machine exhibited near the engine, and were so scientifically shaped as to work almost without noise or tremor.

The English display was, for this country, poor, and it was only by the exhibits sent by Canada that the credit of the empire was saved. Both the metal and the wood-working machines shown by the colony showed considerable originality, especially, as might be expected, the latter class. The small cost of material induced a tendency to neglect economy for the sake of speed, and the wood-cutting machinery was, therefore, of a very massive and powerful character. The first English exhibit noticed by Dr. Anderson was that of Messrs. Massey, who showed a number of steam hammers, varying from the smallest size up to five tons. A novel and ingenious machine for transferring patterns to printing rollers for calico, &c., was shown by Mr. Gadd, of Manchester. The pattern is impressed from a steel milling tool running against the roller, or from a block pressed against it. In either case pressure alone is used to produce the copper printing surface. Besides a stone-dressing machine, in which the action of the chisel and mallet is imitated, Mr. E. K. Holmes showed a new and untried coal-cutting machine. The cutter was a very coarse saw, with teeth an inch apart. The machine for the same purpose shown by Messrs. Baird, of Glasgow, had had the advantage of practical trial. In this the cutters were carried on an endless chain, supported by a projecting arm. A machine for painting articles in number was shown by Mr. Roberts, of Bootle. Amongst the extensive display of American circular saws it was noticed with regret that none were sent from Sheffield.

The German exhibit was neither extensive nor good. The machines that attracted notice were one for cutting small bevel wheels, and a special arrangement for grinding metal surfaces with a fast running emery wheel. Some tools for machine watch-making were also highly commented upon.

In France, the machines sent by Arbey, of Paris, are noticed by Dr. Anderson, almost to the exclusion of any others. His planing machines, with their steel cutters wound vertically on cylinders (like the knives of lawn-mowing machines), were unique. Another machine, for shaping the edges of cask-staves, so that they may fit water-tight, was considered the best of the class, though there were numerous exhibits of cask-making machinery in the United States section.

Belgium showed the actual apparatus by which Mont Cenis was tunnelled. Sweden had an exhibition of sawing machinery, but it was quite overpowered by the magnificent American display of similar machinery. Russia showed great improvement on Vienna, and seemed to have acquired some of the practical knowledge which was wanting to combine with her theoretical knowledge. A noticeable feature in the Russian section was an exhibition of models and machines made by the students of the technological schools of Moscow and St. Petersburg. This collection included lathes, planing machines, parts of steam-engines, valves, governors, &c. Brazil is the only other country mentioned.

It is to the exhibits of the United States that Dr. Anderson devotes the larger portion of his report, though, as he says, that can only note the more salient points in the space at his disposal. He begins by reference to "probably the most exquisite set of machine tools ever made," those employed for machine watch making by the American Watch Company, of Massachusetts. Several other sets of tools are shown; one for making gun-stocks, another for cutting the wheel gearing for the Machinery Hall, are also referred to. The use of special tools for special purposes was specially remarked by the reporter, and he gives as an instance, a lathe specially constructed for making water injectors for steam boilers. Though no furnaces were allowed to be lighted in the Exhibition, there was a fine show of apparatus for smith's work, in some of which there were remarkable instances of the way in which perfect mechanism can replace skilled labor. Where articles have to be repeated, as in making the interchangeable parts of railway bridges, the bars of iron are brought forward on trucks, the ends of the bar are heated in a furnace, and then put into a set of dies worked by hydraulic pressure.

"A touch of a handle by a skilled attendant causes the dies first to hold firmly and then to set up or shorten and squeeze the hot iron into form. If there is a hole to be made, a taper mandril passes through the dies, driving the red-hot iron into every crevice of the steel mould, the whole operation occupying only a few seconds. In connection with these hydraulic forging machines an accumulator is used, and the cylinders being of large diameter the hot iron is like soft clay in the hands of the potter, and pieces of work that would occupy a good smith with a couple of strikers for half a day are made perfect at once, and at a small cost; the chief expense for labor being the removal of the bars of iron to and from the smithy. The great expense is in the tools and plant, which could only be incurred in a country where the work is systematized to admit of repetition."

In drilling machines, the Americans have almost a specialty. The Morse twist drill is of course an American invention, and this is only one of many. Their latest improvement seems to be the use of a spiral tube for transmitting power to drills round corners and in positions where the ordinary apparatus cannot act. On a small scale, this arrangement has been employed for some years by dentists, but it is now employed for heavy work. A spiral is also used to stiffen a tube which has to be bent. It is stiff enough to prevent the tube collapsing, and can be readily withdrawn by winding it in its own direction, so as to tighten the coils and reduce its diameter. The use of the emery wheel is, as might be expected, noticed by Dr. Anderson. This is another practical improvement we owe to America. The wheel is there used for much work which is generally effected by planing and shaping machines. The required results are produced "not by the old system of hap-hazard grinding, but by well-defined movement, rendered as certain in its action as the ordinary tools employed in the various branches of engineering." For trimming rough castings, too, Dr. Anderson saw these wheels largely used, and he was much struck by the way in which they did their work.

In one respect "a new departure" has been taken by the Americans in punching metal, and in shearing and detrusive operations generally. This was principally illustrated by the specimens shown by the firm of Hoopes and Townsend, of Philadelphia, and described by Dr. Anderson in the following words :

"The articles shown consisted chiefly of nuts and other similar perforated specimens; all were of remarkable beauty, and were given away in profusion. These nuts had two peculiarities, they were of inordinate depth, and showed clearly that they had been punched cold. Visitors, however, did not hail this new fact in practical science; they said it was an impossibility for a ⅜ inch punch, however good the quality of steel, to penetrate through 1¾ inches of cold iron; that whatever might be the explanation, a punch of that diameter could not do it without being broken or crippled.

"In time the secret leaked out, for it was no imposture. The firm, in punching, take advantage of the fluid property of solid cold iron or steel by introducing the element of time into the performance of the operation, giving to the punch only such a load of pressure as it can comfortably sustain, then giving up the reins to nature, when the instrument penetrates at a rate dependent on and in proportion to the fluidity of the mass."

In the wood-working machinery there was very much to command attention. The saw-mill annexe was a characteristic and important part of the Exhibition. There were circular saws whose velocity was only limited by the resistance of iron to centrifugal force, in one case a driving pulling having actually burst and gone off in pieces through the roof. A band saw 8 inches wide, and running at sixty miles an hour, was also shown, a striking contrast to the saw described a few lines above, "little more than a steel thread."

In conclusion, Dr. Anderson sounds a note of warning to our own manufacturers:

"Hitherto we have been justly proud of the perfection of the system and organization in our cotton manufactories. What Arkwright did for the cotton mill, the Americans have introduced into numerous other branches, watch-making, for example, with equal advantage. In past times, England has been the nursery-ground of the manufacturing system; her factories have been visited, and her system of cotton and other textile manufactures have been copied by all nations; but the time seems to have arrived when we shall have to visit America in the same way, and for the same purpose, in regard to the production of other things, and there is no time to be lost if we mean to hold our own in the hardware trade of the world, at least in regard to the class of things that are required in large number or quantity."

MACHINES AND APPARATUS USED IN SEWING AND CLOTHING.

Mr. Paget's report on this section includes sewing and knitting machines, tailoring apparatus, watch-making machines, &c. It was the first-named class of machines which constituted the principal portion of the group, and to these most of the report is devoted. As might be expected in the native country of the sewing machine (for such America must fairly be considered, despite the prior European attempts of only partial success), the display of these most popular labor-saving machines was extremely complete. Even in European exhibitions, America had eclipsed all rivals in this department, and on her own ground she was hardly likely to find competitors. Besides, there is no original machine now well known that is not American in its origin, and, consequently, all the sewing machines not exhibited by American makers were only copies of American machines. The extent of this industry in the States may be estimated from the fact that the yearly production of sewing machines is calculated 500,000, while during the last 25 years 2,000,000 machines have been made and sold by a single firm. This enormous business is chiefly in the hands of a few firms. It has developed into an elaborate system, requiring a large amount of capital, both from the long credits given and from the fact that extensive plant is required. The details of the machines are all interchangeable, and this means a complicated and elaborate plant, which, of course, locks up capital. The convenience of this interchangeable system, both to buyer and manufacturer, is so great that firms employing it have a great advantage over others. From these causes the element of competition is greatly lacking; and Mr. Paget foretold what has, it is believed, proved to be the case—that the falling-in of a very important patent during the present year was not likely to make any difference either in the prices of the machines or in the firms making them.

The machines tried were all exposed to very severe tests, it being considered desirable to try them upon work of a more difficult character even than they might fairly be expected to meet with under ordinary circumstances. The result was that comparatively few passed all the ordeals successfully, though the best justified the rigour of the judges by doing so. Some of the machines were tested for easy working by driving them with a loop of cotton in place of the ordinary leather band, and the fact that they passed this test may be taken as a proof both of the nature of the machines shown and of the way in which they were tried. The machine most approved was the new Wheeler and Wilson. The report also commends the recent improvements in the Wilcox and Gibbs, by which the tension of the thread is made automatic, and the size of thread and needle for each sized stich indicated by the mechanism for altering the stitch. Other machines, whose names are as familiar to the public as the two above-named, are also referred to more or less favorably. A number of small motors for driving sewing machines were shown, but these did not officially come before the judges of this section, as they were considered among the "motors." They included, it may be noted in passing, engines worked by steam, water, electricity, and hot air. Several knitting machines are mentioned in the report, and various machines for tailors' use are also briefly referred to. Among the exhibitors of watch-making apparatus, the American Watch Company, of Waltham, Mass., were the principal.

THE ART SCHOOLS OF BELGIUM AND DUSSELDORF.

By JOHN SPARKS, Head Master of the National Art Training Schools.

From "The Architect."

I WENT to Antwerp, Ghent, Brussels, Dinant, Namur, Liége, Dusseldorf, and Cologne, and saw art schools in all those towns, though, with the exception of Antwerp and Dusseldorf, the students were in vacation. Hence the observations I have to make are often founded on the statements of professors, and a close inspection of the drawings, and not always from actual personal intercourse with the students.

The whole of the Belgium system of art instruction has a certain resemblance, whether it is practised for the development of artists or those who practice industrial art. These resemblances may be tabulated thus:—1. Gratuitous instruction. 2. Compulsory attendance of all who join the classes. 3. Elementary and advanced classes in all the sections. 4. For men only. 5. The working time is for six months in the year only. 6. A fixed time for the completion of each exercise. 7. A fixed time for a student to remain in any one section. The above principles are common to all schools, and they have but little modification and expansion even in those schools where high art is the subject principally taught.

The schools are supported from three different sources. The Government, the Commune, and the Province all contribute to their maintenance. The Municipality provides the building, the Government and Province contribute money for the professors' salaries and for exhibitions for students. Teachers are appointed in some towns by the Municipality, in others by the State. The higher professors and directors are Government appointments. All schools are under Government inspection and are nominally under a Government system, but considerable room is left for development to meet local necessities and the views of individual professors. The only school really formed on the Government model is that at Ghent; all the others are so modified by the above-mentioned influences as to be in great measure the product of the localities where they exist. Prizes, which are chiefly honorary, are awarded by a jury of professors and local artists and members of the municipal body. They all have a local character, except those granted by the State, as exhibitions and traveling studentships to the Schools of Fine Art. The courses are graduated, but the students pass from one grade to another at the will of the professors. Examinations in theoretical subjects take place annually, and usually theory and practice are taken together in order to come to a decision on a student's position, so that clever handicraft in drawing is not allowed to come to the front if it is not also associated with sound reading.

ANTWERP.

To take the schools more in detail, Antwerp is a Royal Academy of Fine Art, and the instruction is arranged with the intention of developing a painter, sculptor, or architect to the highest point. A section of the work here, however, is applied to the education of ornamentists, chiefly decorators. A student enters for one or the other of these two sections. There is no continuous course of study which leads from one to the other; thus, while in the School of Fine Art ornament and archæology are taught, and in the School of Ornamental Art, in the same academy, figure drawing is taught, it is always understood and kept in view that in one case it is ornament for artists, and in the other that it is figure study for the decorator that is given.

The elementary classes, corresponding with our artisan art classes, meet only in the evening, from October till April. The Fine Art classes are open from October till August. April and September are months of vacation. In this section the daily work is from 6 in the morning to 5 in the afternoon in summer, and from 9 in the morning to 8 in the evening in winter.

In the course of study for ornament, the students begin by copying an outline of ornament, and proceed to copy an example of shaded ornament with black and white chalk, and carry on this method of copying from the flat until they pass into the class of mural decoration. Here they are required to form compositions of ornamental forms for decoration, and in the same course to copy white casts of ornament on a large scale, the power of imitating the whiteness of the cast being much prized. The whole plan of instruction does not run into figure work, except so far as an ornamentist requires a knowledge of the figure. The ultimate practical product of this school is not such as to justify any imitation of it in this country.

In the Fine Art Section there are three courses of figure instruction: the elementary, the middle, and the advanced. The course of figure drawing is commenced by outlining drawings from rather coarse but strongly drawn lithograph copies of heads, masks, or extremities, always the size of life. In these examples everything is very strongly expressed. The second step in this course of outline is where the student draws the whole length figure, still from the copy in outline, on which the proportions are marked; while still in the elementary section the student copies a drawing of a head, or torso, done by some past student or perhaps a professor, with the view of learning the quickest method of laying in a drawing. Therefore the stump is employed with a restricted use of chalk added to it, but nothing like our method of "stippling" is permitted, and the head is treated in the largest, broadest method of light and shade; no reflections are represented, and only the largest half tints are expressed. It is an exercise to help the student to master his material, while at the same time he learns to see the model in its simplest masses.

The middle course is now taken up. In the lower section of this course the student draws from casts of masks and parts of faces, limbs, hands, and feet, and groups are made of these. The torso and full bust are the middle section of the middle course. The principle of having a fixed time for each exercise is strictly carried out, and each object or group of objects is changed every week. The advanced section of the middle course is drawing from the whole antique figure; each figure is placed for eight days, and is worked at two hours every day; therefore sixteen hours is the whole time allowed for the completed work. The exception to this is only when figures are drawn for competition: then twenty hours are allowed, the additional time being given in consideration that the student receives no advantage from the professor's supervision. A time limit is imposed on students in this section; that is to say, if they are not passed on into the highest class in two years it is assumed that they have no strong gift for their profession of painter, and are denied the privileges of the school.

The next step is with the advanced course, where they paint from the life, having either the head or both head and torso. The head is painted in eleven hours; the model sits on two consecutive days for five hours and a half each day. The torso is painted in twenty hours on four days. In this painting practice the student applies the teaching he has had from the beginning of his career, viz., that form consists not only of outline in profile but in actual projection towards the spectator, and has to be truly represented by a double expedient; one is the contour, where the form cuts against a background, for example, and the other is tone (or change of real color), by which means only all advance and recession of surface is expressed. This practice is combined with drawing from the whole length living model. It is a morning class from six to eight in summer, and an evening class in winter. Sixteen hours is the time allowed here, two hours a day on eight days. No female model sits in the classes.

Throughout the whole school the professors do not touch the students' drawings, but giving many illustrative sketches at the side of the drawings, and insist on great accuracy in proportion, form, and anatomy. Local color or tone is everywhere insisted on, as leading to the painter's practice more even than forms in contours. Everything is expressed with the least amount of mechanical work, hence no stipple or elaboration of tints with either stump or chalk is permitted. The consequence is that the apparently short time that is given to the work is, in reality, amply sufficient for the purpose. Each student in one year's practice in the antique and life school commences and finishes drawings of thirty-three whole-length antique figures, and has obtained experience and power in his work. In this matter, especially, the contrast of our system to that of Antwerp is seriously to our disadvantage.

The student in sculpture passes through a similar course. He draws the outline copies for proportion and action, and the fitting of limbs and head and body together, then models from the mask and extremities, and the bust. In the higher elementary course he models the torso. In the middle course he is taught the principal of bas-relief, and models the whole figure in relief and in the round. Finally, in the advanced course he models from the life. For the modeller of ornament a similar course is prescribed. In the elementary course he models in clay from simple copies in plaster, and then works out similar designs from drawings. In the middle course he carries on his work from drawings or greater intricacy, and in all grades of relief; and, finally, in the advanced course he models flowers and foliage from nature. He then passes on to the section of applied arts, and works out original compositions for various purposes in various materials. This section is perhaps better in theory than in practice.

The architectural course is remarkably thorough and good, but does not so immediately apply to the subject I had more particularly to report on.

An essential part of this system is the combination of theory and practice. The work of each day closes with a lecture of an hour's duration, and these lectures are attended by the students according to their position in the school.

These lectures are on the following subjects :—1. Perspective, linear aërial, or picturesque; 2. Costume and antiquities; 3. History; 4. Anatomy of bones and muscles; 5. Expression; 6. Æsthetics, or comparative history of art in different schools, with their tendencies; 7. Composition, that is, historical painting, or modelling; 8. General literature. The above course is compulsory on all students in painting, sculpture, and architecture who are in the advanced sections, and an examination is conducted for the highest rewards annually, in which the student who is successful must have come out well in all the subjects that are taught theoretically, as well as in those which more relate to practical art. I cannot speak too highly of the excellence of the drawings and paintings executed in this school. A great amount of power and handicraft is developed, but this is properly subservient to the thorough understanding of the model, and also to the student's individuality.

The highest competitions for the traveling studentships are conducted on the usual continental plan, viz., a sketch is made in a day of the subject which has been drawn, in the manner of a lottery. The students who enter still into competition are then confined to a studio from early morn till late at night for a certain number of days, and produce a picture after their sketch. The materials they require, such as models, costumes, &c., are supplied by the Academy. The same plan is adopted for the competition in sculpture and architecture.

These competition works, when produced "en loge," are remarkable for their power and the evidence they give of perfect command of the material. Other classes in Antwerp are those for engraving on wood, on copper and steel, and for naval architecture.

BRUSSELS.

At Brussels the Royal Academy of Art was not to be seen, as the old building of the Academy had been pulled down, and the new one, on a very fine spacious plan, with good exterior elevation, was not finished. The classes had met in various rooms belonging to primary schools during the winter, but the inconveniences were so great that they had given up their meetings early in spring. Being thus unable to investigate the system of work by the Academy, I had the greater opportunity of seeing the practical working of the art-work in primary schools, and in those which correspond with our night classes. These were in full operation, and I visited—1. The Model School; 2. The Normal School; 3. The School of St. Josseten-Noode; 4. The Free University; 5. The "Ecole professionelle des femmes."

The Model School is a secular school, of which many are established in Belgium, as a protest against clerical domination. They are absolutely without a religious system of teaching. Many friends of the anti-religious party assume that all a child should learn at school is such as can be taught on a scientific basis as distinguished from a speculative one, and, consequently, science is the basis of their system of instruction. The root of a scientific education is double—one is arithmetic and mathematics, the other is drawing. Hence we see a great development in the application of drawing to education; for instance, geography is almost wholly taught by drawing on the black board, made by the teachers and copied by the children; physiology and entomology, and other sciences, are taught in the same manner. The black boards surround the room, as a dado adjusted to the children's height; and on these they add line to line, following the teacher, and develop the plan of the town, the construction of a country, the anatomy of an insect, or the anatomy of the human body.

In the lower classes the instruction is given by a graduated system, invented by Mr. Hendricks, and does not materially differ from our own except in one particular; it is that the drawings, whether of straight lines or symmetrical curves or floral forms, are all made to a large scale. In a large communal school —for girls—the same system is admirably carried out; drawing is thus made to subserve an important use in general education, and the children are brought up in the practice of expressing their knowledge and thought through their handiness in chalk drawing. It was natural to find that sufficient opportunity and teaching were given to the pupil-teachers of the town to qualify in this art of drawing diagrammatically on the black board. This was seen in a class in the normal school for teachers, where youths and young men who had passed the morning in the primary schools came to carry on their own higher studies in the afternoon. The drawing lesson was by Mr. Hendricks himself. The drawings that were being done were four and five feet long, and were nailed down to large desks, which served as drawing boards. The original drawing, of ordinary octavo size was placed in a frame before the student, and glazed, so that no measurement on the surface of the copy could be made. The originals were elaborate engravings of ornament, shaded. The copies were first made correct in outline, and then were either stumped in chalk or washed in water color with the greys and shadows, to produce the effect of the original drawings. Everything is translated, not slavishly copied. A lithograph copy is not imitated as a lithograph, but is enlarged, and then translated with sepia, Indian ink, or stumping chalk. The advantage of this more thoughtful method over the comparatively thoughtless one of mere mechanical imitation was shown where the students drew from the cast. The habit of translating and interpreting was already formed, and they drew well from the cast from the beginning of their practice. In the drawing from the cast the objects for light and shade were simple, and required but little drawing; thus the students' attention was concentrated on the large masses of light and shade, the reflections being carefully ignored in the first steps of the work. Elementary design also was being taught, but it went no further than the composition of simple geometrical lines to produce patterns by their intersections, bringing out frets of various angles and repeats, more ingenious than useful.

The drawing at the Université Libre was that of the ordinary polytechnic school of the Continent, and is confined to the students of that section of the University. The subjects are model drawing from geometrical forms; mechanical and architectural drawing. All are thoroughly drilled in perspective and the projection of shadow. Viollet le Duc's plates are extensively used as examples for construction and for drawing copies. The classes for construction study the various questions of strength of material and resistance, and all scantlings were worked out arithmetically.

The "Association pour l'enseignment professional des femmes" have a school in which women, principally young girls, are trained in various subjects to qualify themselves for employment. A sort of apprenticeship system is adopted, by which the constant attendance of the girls is secured for three years. Book-keeping, English and German languages, for those who wish to qualify as clerks, are taught. Dressmaking, with scientific cutting out. Artificial flower making in another section. In practical art the classes are formed for drawing ornament from copies and the cast, elementary design, painting on fans, and on china and earthenware. An excellent set of copies for the construction of ornament is used. All copy work is enlarged considerably, and cast drawing is stumped and represented in the simplest light and shade. The figure drawing from the bust and extremities was very good, large in style, and of simple light and shade. The school is partly self-supporting, but requires subscriptions to meet the payments to teachers, &c. It is interesting to see that the exclusion of women from the advantages of secondary education is being abandoned in Brussels.

The evening classes held in the upper floor of the primary school of St. Josseten-Noode, a suburb of Brussels, may be fairly compared with the similar classes held in our district schools. There are 300 to 400 students who work every evening from 7 to 9, and are taught gratuitously various subjects useful to the artisan population which fills the suburb. Building construction, such as walls for bricklayers and masons, joiners' work, metal work for locksmiths, are carefully fitted to the particular trades of the students.

The course of ornament is excellent for construction, or analysis is taught from the beginning, from exceedingly good copies, by Mr. Hendricks. These are always drawn enlarged to about the size of an imperial sheet in charcoal or chalk. An advanced course of copying from ornament is formed of those students who copy elaborate designs from the flat of partly natural and partly conventional construction on large sheets of paper, 4 feet long. A firm strong chalk line expresses all the contours. The light and shade is filled in either with the stump or with washes of Indian ink. Finally, the students in this class compose ornament, certain conditions being given by the professor, such as a spiral line and a drawing of an olive branch, to be worked into an Italian panel of the cinque-cento type, and so on. These are always worked out to a large size—about 5 feet seemed to be the usual length. The arrangement of the desks here is not uncommon in Belgium. All the students stand to their work, and pin or nail their paper down to the broad desk before them, which is painted black, and is of size sufficient to take the large sheets of paper in use. There are thus no drawing boards in the school, except only in the architectural section.

The section of drawing from the cast was excellently represented. Cones, spheres, or other geometrical figures, are placed on a moulded or hemispherical or other shaped basis and then hung to the wall, so that the apex of the figure points towards the student, while the base is parallel to the wall; and being then strongly lighted by a side light, the shadows naturally fall over the moulded surfaces, and give great variety of depth and tone. Shading is also carried on from large casts, examples of Roman ornament (the Trajan scroll, for instance) ; everything is rendered realistically, that is, the whiteness of the plaster is the first thing to be noticed, then all the gradations from light to dark. The stump and chalk alone are used, but to such good effect that the drawings are true to illusion. The classes for the antique and life presented nothing remarkable, except from the fact that the antique drawings, even from the full-sized figures, were always the size of the casts. A daylight class for painting the head of the living model was an exceptional thing, but had produced good firm painting from copies, intended to show to inexperienced students where the greys and local color were to be found. All the painting was direct imitation, without any method of work being discernible. A painting school, having for its aim the production of decorative artists, worked through the elementary course to the composition of ceilings and wall spaces with geometric lines and panels of decorative forms, either conventional or natural, or to any given historic style. The highest art practiced in the class is the painting of flowers and groups from nature. These were always done the size of nature, and with arranged backgrounds, and the usual time for a flower and foliage, such as a group of peonies, is three hours.

Prizes are awarded to each section yearly by a jury of professional artists and masters in various trades. The awards are made on the whole mass of work. No award is made to a small amount of work, however good. The time limit is not a hard and fast rule here. The professor gives out the time that he thinks sufficient to complete each object when it is posed. The time is, however, short, as a large Roman acanthus nest of a scroll was done in ten hours. My general impression is, that in the system of drawing ornament on principal and planned construction, this school is far in advance of anything we have in England; so with the imitation of the cast in the elementary classes, and also in painting from nature, all of which is done by a certain powerful straightforward method, quite in contrast to the methods in use in the average English art schools.

NAMUR.

At Namur is an academy of painting and drawing, conducted on the same general principles as that at Antwerp; but copying from the flat, especially heads in oil and still life in water color, is carried on to a greater extent, to the disadvantage of the students. The copies were poor and the originals not good. It is evident that, although the system may be excellent, yet that very much depends on the capacity and power of the teachers. I cannot but think that at Namur the want of careful supervision was evident in the students' works, although this opinion is not formed on a large experience of the drawings, as the students were in vacation, and not one of the professors was in the town. Here, as at other provincial towns, the "plant," that is, desks, gas arrangements, tables, stands for framed copies, &c., is all excellent, and there also seemed to be abundance of room to each student.

DINANT.

At Dinant the same system is said to be in operation, but the whole town was *en fête*, and I could not obtain access to the rooms in which the School of Art met.

LIEGE.

At Liége I found an art school of 300 students on the usual plan, *i. e.*, a seat or place to each elementary student, ample space in large rooms specially lighted, and graduated classes working up to the highest art school practice. The works of a recent annual competition were shown me, and were all of high quality. Every exercise was the result of twenty hours' study. In the modeling school there were:—1. Model from life, three feet six inches in height, in high relief, and in strong, somewhat exaggerated style, but manly and full of character and knowledge of anatomy. 2. Model from the antique—very good. 3. Elementary work from the bust was excellent in style and power. 4. Modeling from a drawing. The original was a rich Roman scroll, full of detail, and eighteen inches long. This was increased to thirty inches, admirably done, and completely finished in twenty hours. 5. Architecture; here also the same time was allowed, viz., twenty hours for an excellent drawing of an Ionic portico and façade with a gateway; the proportions, etc., are given in description. 6. Architectural design. In the competition in this class a written specification of a subject is drawn from a bag and given out. The last was the peristyle of a university building in the Italian sixteenth century style. The sketch is made in a day, then twenty hours are allowed to work it out; the result being a highly-finished design, far beyond what is obtained from our students. It is a different thing from the general course, and is arranged for architects solely, and attended by men who pass all their time in this study. All students are superannuated at twenty-one years of age. They enter the school as young as ten years of age.

Lectures on theory are given, and examinations are held in the subjects of these lectures, with only one exception. They are: Perspective, elementary and archæology, or history of art (but on this subject there is no examination); expression—anatomy; construction; descriptive geometry, and ornament.

In the middle and elementary sections the practice prevails of enlarged work all done within a time limit. The imitation of the plaster was a matter much insisted on; dark backgrounds are invariably used and imitated.

As at Antwerp, the figure class is of two advanced sections and two elementary; that is to say, the work done from extremities and the torso is distinct from the antique, as we understand it, and the life. The student thus graduates in his work. The drawings from life were well drawn, but roughly and imperfectly shaded compared with those from Antwerp.

For the whole school of 300 students there are ten professors, who are engaged for six months in the winter. They are architects or painters, who thus occupy themselves during their evenings at a season when their time is of small value. A very superior class from which to draw teachers is thus available. Of the 300 students, twenty form an advanced art class, and paint from life, and compose pictures under the director. Of these, some who do not come under the definition of artizan pay twenty francs annually for their instruction; otherwise it is free. The town provides and keeps the buildings. The professors are paid salaries from a fund partly voted by the Government and partly by the province. Medals and honorable mentions are awarded every two years by a jury of local artists, Government inspectors, and the professors. The school seems to be doing a large and important work in the Birmingham of Belgium, especially in the modeling sections. Students from this school hold lucrative appointments as modelers for silver and metal work in England.

GHENT.

The school at Ghent is in some sort the most interesting school of any in Belgium, for here the pure and simple Government system is carried out without any mixture of local influence to modify or suppress any part of the scheme. The elementary rooms are three in number, extending in one line 250 feet. They are twenty-nine feet wide, and accommodate 600 students sitting at their desks, with ample room to each. This portion of the school is only used as an evening class six nights a week. The professors arrive five minutes before the doors are opened, and remain till all the students have left. The course is planned for a three years' practice in the elementary rooms, to be supplemented by a further course of two years in advanced classes.

The lowest elementary class practice is drawing from an ornament, drawn by the professor, line by line on the blackboard. This may be a simple or a complicated piece of work, and may be finished in one evening or in four. On two nights in each week the students draw on paper; on other nights they draw on their black desks with white chalk. In this section the students are thoroughly drilled in the construction of ornament, from the simplest geometrical form to the most complicated. In the second year they draw, in light and shade, from models and geometrical figures, devised especially to teach all the variety of light and shade on mouldings, on spherical or cylindrical surfaces, and the shadows of these, cast on to every variety of background. In the third year they draw from the cast of the figure, that is to say, masks modeled so as to be clearly seen, and of colossal scale, for the same end. This mask is lighted by a gas arrangement independent of the lights by which the students work. These students' lights are screened from the cast, so that it is visible only by its own light. Above the mask on a shelf is placed a cube and a cylinder, in order that the students may see the light and shade of the largest surfaces of the cast in their simplest expression, the cube showing the largest planes, the cylinder the gradation in the half tone to the shadow. In the same section is a class for the torso and extremities.

All students, whether painters, designers, architects, sculptors, or of any other calling, must pass through this three years' course. Architects then follow a course of ornament, always shaded; sculptors then model; the painters pass on to the whole-length figures and the life. This class and that for painting from the model is reached in the fifth year. Although this is the course always prescribed, yet much is left to the discretion of the teacher, who may promote any student to the class above his own if he is satisfied that the student can do the work required intelligently. In the antique or life classes, 30 hours is the time allowed for the completion of the drawing or model. Advanced students draw in charcoal and stump the size of life from the model; this is done occasionally. The modelers form the outer ring in the artisan and life schools, and always work in relief.

The course of architecture is probably the most thorough and complete in Belgium. The teaching of this art is based on classic forms. There are three classes, one (the lowest) works from large flat copies which have all the dimensions, sections, &c., of the orders. The second class carries this copying system further by working out details, plans, and elevations of known buildings. The third class works at compositions and designs. All this study is still based on classic models, but special classes are formed for the study of the Romanesque and Gothic styles. A valuable feature in the elementary classes is a set of models to a large scale of the orders from base to pediment complete.

In the competitions, which are annual, for honors, the work is done strictly against time. In alternate years the highest prizes are awarded, all competition is done under strict rules, the students' sketches are made in locked studios : one man in each room, which he does not leave for twenty hours : he is during this time cut off from any opportunity of obtaining assistance in his work.

The sketch thus produced is examined, and, if worthy, is the basis of a finished drawing to a large scale, if in the architectural, or of a painting if in the painting class, which must be finished in 40 days. No deviation from the original design is allowed. There are 200 students in the class of architecture; 30 of these are in the highest class, of which number 20 are in the Gothic section.

Concurrently with this practical work are lectures on theory. All the elementary students above the lowest class attend lectures on perspective and geometry. Those in the upper elementary classes carry on their perspective into advanced work, and add descriptive geometry, the architects adding trigonometry and algebra, the painters anatomy. In the highest classes of the fourth and fifth year, all, *i.e.*, painters, sculptors, and architects attend lectures on the proportion of ancient building. The architects work at perspective and stereotomy. The painters and sculptors now add advanced anatomy, archæology;

especial stress is laid on the history of arms, armor, costume, domestic architecture, &c. A peculiarity worthy of adoption is that of providing ample space, so that the student sits at arm's length from his board, which is never moved from its support in front of him. The teachers never touch the students' work. Here, as at every other school of importance, there is a gallery of casts or store room, in which each cast, whether of extremities or whole length figure, has its place. The whole length figures are on castors, and are wheeled into the antique room as they are wanted. Only one cast at a time is there.

The impression conveyed by seeing the work of the whole school is that the architects' training and work are excellent, perhaps the best in the country, and far in advance of anything provided in this country. The figure work, although very good, is not so excellent as that produced at Antwerp. The elementary system is carried out to perfection, but perhaps too elaborately, considering that it is only a preparation for a higher end in artistic practice. And the figure work did not seem to lead so directly to the development of the painter as at Antwerp, the figure paintings that were shown me being inferior in brightness and effect to those of the Antwerp school. The student does not paint so soon as at that school.

The obvious deduction to be made from the whole Belgium system is, that while good teachers produce good students, irrespective of system, yet that the Belgium system is in advance of our own at certain points, which may be recapitulated here :—1. Ample buildings for the school, so that every cast is well lighted, well seen, and drawn from in comfort; 2. The time limit that is placed on every work, whether for practice or for competition ; 3. The universal teaching of students to imitate what they copy on the assumption that they are all to become painters; 4. The plan of theory by lecture and practice in the schools being carried on simultaneously, and stern examinations in both.

DUSSELDORF.

In Germany a different system is met with; for instance, at Dusseldorf all students pay for their instruction. In the elementary school instruction is given in drawing from copies, usually outline, of a large but somewhat coarse character, and from shaded drawings of heads and extremities; these are copies done by professors or advanced students. Form in contours and method of manipulation are chiefly looked to here. The proper intonation of the drawings from the figure or from the cast is not attempted in this stage; hence the use of white chalk on gray paper is common.

In the antique and life schools drawings are made also in this manner, and students compete for promotion from one section to another, with drawings the size of the casts, which are done in a fixed time, which varies according to the professor's prescription. The advanced classes are those which work in the studios of the academy, and paint pictures under the eye of the professors of composition; thus the young students are brought in contact with the leading men of the country. This section is very vital to the traditions of the school, and here the future artist is formed in style and habits of thought. The chief professor does not remain many years at his post. Seven or eight years is probably the time each composition master passes with his students. Students work about five years in the Academy; they must pass into the life classes in two years; if not they are denied the privileges of the school. In the life school the student paints at once from the head, and draws at night from the nude figure.

Here again is no fixed time other than that prescribed by the professor; at one time a head will take a month to finish, at another it must be done at a single sitting of five hours; he thus exercises his students in observation and extreme finish; at others, urges them to express their knowledge in the most rapid manner. There is throughout no great respect for academic traditions, but an earnest striving to discover and foster the individuality of the student; there is no compulsion in attendance, and no early work. The standard from an academic point of view is not so high as that reached at Antwerp.

The hours of attendance are 9 till 12 and 2 to 4. Painting or drawing with a life class for two hours in the evening from 6 to 8 or 7 to 9.

The fees are 12 thalers per annum in the antique school (£1 16s.), 24 thalers in the life school, and 36 thalers for the studio. This latter payment includes gas and heating, and all properties and apparatus, but not models. There are about 40 students in the antique school, about 20 in the life school, and 12 to 15 in the studios. In general no great reliance seems to be placed on the system, but very much on the teacher.

It happens that many students commence work who are unfited for development in high art. It is generally regretted by past and present professors that there is no school for industrial art which could absorb those persons who miss their aim in painting as a fine art.

COLOGNE.

In Cologne there is an art school meeting in the museum under the direction of a well-known local painter of landscape and genre. The drawings are neat, and elaborated on gray paper with white chalk, not better than the average work of an English art school. All the work from the casts truly represents the texture, color, and relief of the original. In the absence of any official connected with the school, I could not ascertain the plan of work.

In general the impression conveyed by the drawings was that they are somewhat overworked with an artificial mechanical finish.

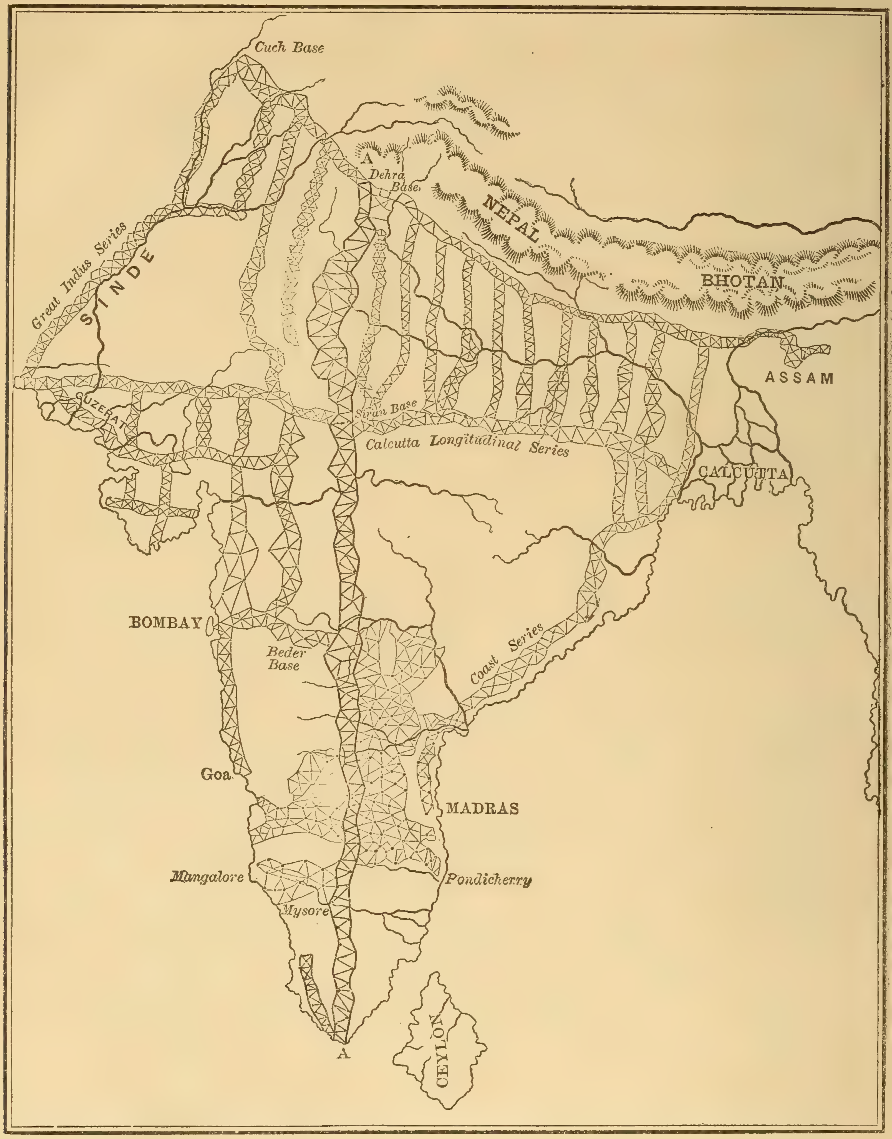
THE SURVEYS OF INDIA—THE TRIGONOMETRICAL SURVEY.

By F. C. DANVERS, A. I. C. E.

From "The Quarterly Journal of Science."

The surveys of India may be divided into two classes—*viz.* the Great Trigonometrical, and the Geological. In connection with the former, other minor operations are undertaken under the title of topographical and revenue surveys, to which we shall refer more particularly in due course.

The idea of a great trigonometrical survey of a country, to be undertaken by the Government of that country, was first conceived by General Watson, at the suppression of the rising in Scotland in 1745. The execution of it was committed to General Ray, and was originally intended to extend no farther than the disaffected districts of the Highlands. The design, however, was subsequently enlarged, and the grand trigonometrical survey of Great Britain and Ireland was projected. Perhaps a more important survey, in some respects, than the British one was that undertaken by the French nation at the period of the Revolution. About that date the philosophers of France undertook to introduce a great reformation in regard to all those habits and usages of men which have reference to numbers, and everything—lengths, areas, moneys, weights, periods of time, arcs of circles—was to be numbered by tens, hundreds, thousands, &c. The question then came to be, What should be adopted as the basis of this standard, which was designed not only for France, but for the world? This question having been brought to the attention of the Constituent Assembly, it was proposed by M. de Talleyrand, and decreed accordingly, that the Parliament of England should be requested to concur with the National Assembly in fixing a natural unit of weights and measures; that under the auspices of the two nations, an equal number of Commissioners from the Academy of Sciences and the Royal Society of London might unite in order to determine the length of the pendulum which vibrates seconds in the latitude of 45° (as proposed originally by Huyghens), or in other latitude that might be thought preferable, and to deduce from them an invariable standard of measures and of weights. The Commission named by the Academy had under their consideration three different units, namely, the length of the pendulum, the quadrant of the meridian, and the quadrant of the equator. The length of a quadrant of the meridian having been determined on, the measurement of an arc was entrusted to MM. Mechain



and De Lambre, who began their labors in 1792, and thus commenced the trigonometrical survey of France.

The origin of the Great Trigonometrical Survey of India was not unlike that of the first Scottish Survey. After the successful termination of the war with Tippoo Saib, at the close of the last century, Captain Lambton (who had previously served as a surveyor in America, and who joined Her Majesty's 33rd Regiment at Calcutta in the year 1797) brought forward his plan of a geographical survey of part of the territory that had been conquered, and he proposed to throw a series of triangles across from Madras to the opposite coast, for the purpose of determining the breadth of the peninsula in that latitude, and of fixing the latitudes and longitudes of a great many important places, which were believed to be very erroneously determined in the survey previously executed by Colonel Mac Kenzie. Captain Lambton first submitted his plan to Colonel Wellesley, in whose regiment he had formerly served, who at once sent up the proposal to Government supported by his strong recommendation. Lord Clive was at that time Governor of Madras, and warmly approved of the undertaking, and it was accordingly sanctioned by Government.

The first base line measured by Colonel Lambton was on the table land of Mysore, near to Bangalore. The chain used by him was one of blistered steel, constructed by Ramsden, and precisely similar in every respect to the one used by General Roy in measuring his base of verification on Rumney Marsh. It consisted of forty links of 2½ feet each, measuring in the whole 100 feet, at a temperature of 62°, and fitted with two brass register-heads, with a scale of 6 inches to each. This chain, it appears, had originally been sent with Lord Macartney's embassy as a present to the Emperor of China, and having been refused by him, it was made over by his Lordship to the astronomer, Dr. Dinwiddie, from whom it was purchased. The measurement of this base line was commenced on the 14th October, 1800, and completed on the 10th December following. Its total length was 7.4321 miles.

Whilst these operations were being carried on, an order was on its way to England for a supply of instruments of the best manufacture that could be obtained. Amongst these was a new chain which Colonel Lambton never allowed to be taken to the field, but it was reserved as a test, whereby that actually used was constantly verified. The other instruments received from England were a 36-inch theodolite, by Cary; an 18-inch repeating theodolite, by the same maker; a 5 feet zenith sector, by Ramsden; a standard brass scale, by Cary; and several small theodolites, by different makers, for minor purposes. These instruments were the finest that the state of art at the commencement of the present century could produce.

On the 10th April, 1802, the real commencement of the Great Trigonometrical Survey of India was made, although at that time the extent to which those operations would be ultimately carried was not even contemplated. Upon the resumption of operations no notice appears to have been taken of the Bangalore base line. Work was commenced by the measurement of a fresh base line of 40006.4 feet, on a plane near Saint Thomas' Mount, Madras, at no great distance from the shore, and nearly on the level of the sea. From this a series of triangles was carried, about 85 miles eastward, north as far as the parallel of 13° 19′ 49″ N., and south to Cuddalore, in latitude 11° 44′ 53′′, embracing an extent of about 3700 square miles. Before describing further the progress of the survey, we must pause for a moment in order to give some account of the care taken in measuring the base line. The chain was in all respects similar to the one used at Bangalore. It was laid in coffers or long boxes, supported on stout pickets driven into the ground, and their heads dressed even by means of a telescope. At one end of the chain was a draw-post, to the head of which the near end of the chain being fastened, it could be moved a little backwards or forwards by means of a finger screw. Near the handle of the chain, and at a point where its measuring length was supposed to commence, there was a brass scale, with divisions, which was fixed to the head of another picket, distinct both from the draw-post and from those supporting the coffers. This scale could, by means of a screw, be moved backwards and forwards on the head of the post till it coincided with the mark on the chain. A similar arrangement was made at the other end, but the handle of the chain, instead of being firmly attached to the weigh-post, as it was called, had a rope passing over a pulley; and to this rope was appended a weight of 28 lbs. to keep the chain stretched. This arrangement enabled the measurer to move his chain backwards or forwards with the greatest nicety, and when satisfied that it was correctly placed, to keep it there perfectly steady; while, by means of the registers, he marked the places of the two extremities of the chain. The chain was then lifted by twenty coolies and carried forward, the near end being adjusted to the scale which had before marked the fore end. A new chain's length was then laid off in a similar manner, and so on, until the base was finished. During these operations tents were erected over the line, and thermometers were placed in the coffers to determine the temperature of the chain; and the rate of expansion having been previously determined by experiment, the necessary corrections were made for the varying temperature of the measurement. The quantity of this correction was .00725 inch for every degree of Fahrenheit.

Many of the triangles carried forward from this base line had sides of from 30 to 40 miles in length. In computing their length Colonel Lambton reduced the observed angles to the angles of the chords, according to the method of De Lambre; and though he computed the spherical excess, he did not use it in any other way than as a measure of the accuracy of his observations. The chords, which were the sides of the triangles, were then converted into arches; and as Colonel Lambton had contrived that the sides of the four triangles which connected the stations at the south and north extremities should lie very nearly in the direction of the meridian, their sum, with very little reduction, gave the length of the intercepted arch, which was thus found to be 95721.326 fathoms. By a series of observations for the latitude, at the extremities of this arch, made with a zenith sector, the amplitude of the arch was found to be 1°.53233, by which, dividing the length of the arch just mentioned, Colonel Lambton obtained 60494 fathoms for the degree of the meridian bisected by the parallel of 12° 32′. This, till the survey was extended farther to the south, was the degree nearest to the equator—excepting that in Peru, almost under it—which had yet been measured. The next object was to measure a degree perpendicular to the meridian, in the same latitude. This degree was accordingly derived from a distance of more than 55 miles, between the stations at Carangooly and Carnatighur, nearly due east and west of one another. Very accurate measures of the angles which that line made with the meridian at its extremities, were here required; and these were obtained by observations of the Polar star when at its greatest distance from the meridian. For this purpose a lamp was lighted, or blue lights were fired at a given station, the azimuth of which was found by the Polar star observations, and afterwards its bearing was taken in respect of the line in question. Thus the angle which the meridian of Carangooly makes at the pole with that of Carnatighur, or the difference of longitude of these two places, was computed. It was then easy to calculate the amplitude of the arch between them; and thence the degree perpendicular to the meridian at Carangooly was found to be 61061 fathoms. Upon comparing this degree of the perpendicular with the degree of the meridian, the compression at the poles would appear to be equal to $\frac{1}{210}$. A writer in the "Philosophical Transactions" for 1812, p. 342, contended that, on account of an error in calculation which escaped Colonel Lambton, the foregoing measurement should be diminished by 200 fathoms, thus reducing the length of the degree of the perpendicular to 60861 fathoms, which would give $\frac{1}{330}$ for the compression. These measurements were made in 1803.

In May, 1804, a base of verification of 39793.7 feet (7.536 miles, reduced to mean sea-level) was measured by Lieutenant Warren, Colonel Lambton's assistant, near Bangalore; and though the distance was nearly 160 miles, the computed and measured lengths of this base differed only 3.7 inches, or about half an inch in the mile; a proof of the great care and accuracy with which the work was conducted. This base was adopted for the origin of the great Indian arc series, to which we shall presently refer more particularly. From it a series of triangles was carried across the peninsula to the Malabar coast, which they intersected at Mangalore on the north and Tellicherry on the south. The heights of the stations were all determined from the distances and observed angles of elevation. The most considerable heights were at Soobramanee and Taddiandamole, in the western ghauts, not very far from the coast, the former being 5583 feet, and the latter 5682 feet above the level of the sea; but notwithstanding having to cross such elevations, after carrying the survey over a distance of 360 miles, it was found that the sum of all the ascents, and of all the descents, reckoned from the level of the sea, differed from one another only by 8½ feet. From the triangles thus carried across the peninsula, a correct measurement of its breadth was obtained, and one considerably different from what was before supposed. The distance from Madras to the opposite coast, in the same parallel, was ascertained to be very nearly 360 miles; whereas, until then, the best maps made it exceed 400 miles. All the principal places on the old maps, which had been fixed astronomically, were also found considerably out of position: for example, Arcot was out 10 miles, and Hydrabad no less than 11 minutes in latitude and 32 minutes in longitude.

For a long period the operations referred to above were frequently interrupted by the disturbed political condition of the country, which was often the scene of warlike operations; for it was not until the Marquis of Hastings destroyed the Pindaree confederacies in 1818 that the peninsula and Deccan settled down into quiet and repose. The mysterious character of the instruments and operations, as well as the planting of flags and signals, always more or less awakened the apprehensions or excited the jealousy of the native princes; and it required, therefore, no ordinary tact, firmness, and patience, in order to conciliate their good-will.

Between the years 1802 and 1815, a network of triangles was, under the superintendence of Colonel Lambton, carried over the whole country as high as 18° latitude, whereby the peninsula was completed from Goa on the west to Masulipatam on the east, with all the interior country from Cape Comorin to the southern boundaries of the Nizam's and Mahratta territories. Subsequent to this achievement, the great arc triangulation was extended nearly to Takal Khera, in latitude 21° 6′. The greater part of the Nizam's eastern territories were triangulated by meridional series between the Kistnah and Godavery, and considerable progress was made in the longitudinal series from the Beder base towards Bombay. The area comprised by the whole of the operations prosecuted during the time Colonel Lambton was superintendent aggregated 165,342 square miles. In October, 1817, the Marquis of Hastings, impressed with the important utility of the trigonometrical survey, resolved to transfer the control over its operations to the Supreme Government of India, which took effect from the 1st January, 1818, and Colonel (then Captain) Everest was appointed assistant to the superintendent, whom he subsequently succeeded upon the death of Colonel Lambton on the 20th January, 1823. Colonel Everest first acted as chief assistant during the latter part of 1818, and he was employed, in the first instance, in the triangulation of the eastern parts of the Nizam's dominions, and subsequently on a longitudinal series of the great triangles emanating from the Beder base line towards Bombay. He was engaged on this important work at the time of Colonel Lambton's death, by which event he succeeded to the office of superintendent, and immediately proceeded to concentrate the resources at his disposal on the extension of the great arc series, which, after many difficulties, was at length carried up to latitude 24°, where a base line was measured at Seronj.

After this, Colonel Everest proceeded to England, returning thence, in 1830, provided with geodetical instruments and apparatus of every description executed by the most skillful artists of the day, including a complete base-line apparatus, the invention of Colonel Colby, precisely similar to that employed on the Ordnance Survey; a great theodolite, 36 inches in diameter; two 18 inch theodolites; and a variety of smaller instruments from 12 inches diameter downwards. The signals, all of the most efficient kind, and recently invented, consisted of heliotropes, reverberatory lamps, and Drummond's lights, of which the two former have been exclusively used.

In addition to the duties of Superintendent of the Trigonometrical Survey, Colonel Everest had, on his return to India, to perform those of Surveyor-General of India. In 1833 the offices of Deputy Surveyor-General at Madras and Bombay were abolished, and their duties devolved upon the Surveyor-General, so that Colonel Everest had now to perform the work which had hitherto occupied the undivided attention of four officers.

By the end of 1832 a longitudinal series of triangles had been completed from Seronj to Calcutta, where another base line was measured. Upon the completion of that work the measurement of the great arc was recommenced, after a cessation of seven years. It was carried on from that time unremittingly till December 1841, when the whole Indian arc from Cape Comorin to the Himalayas, forming the main axis of Indian geography, was finally completed. The area comprised by the great arc operations, principal and secondary, aggregated 56,997 square miles, including the revision of the section from Bedar to Kalianpoor, and the measurement of three base lines, each from 7½ to 8 miles in length, *viz.* those of Beder, in latitude 18°; Seronj, near Kalianpoor station, in latitude 24°; and the Dehra base, about 70 miles north of Kaliana station, in latitude 29° 30′, where the great arc actually terminates; this distance being observed on account of the proximity of the Himalayas. On comparing the actual measurement of the Debra Dhoon base by Colby's apparatus with that calculated from the Seronj base, measured by the chain in 1824, a difference of nearly 3½ feet was found. In former times this would have been considered a very satisfactory agreement, seeing that the length of the base is 7½ miles, and its distance from the new base upwards of 400 miles in a straight line; but Colonel Everest considered the difference as indicating a much larger error than ought to exist, regard being had to the precision of the new methods. In order to set the question at rest, he resolved to re-measure the old base with the more complete apparatus he now had at his command. This operation was completed in January, 1838, when it appeared that the length given by the chain measurement of 1824 was too short by nearly 3 feet, as compared with the new result.

In the year 1829 a trigonometrical survey in the Bombay Presidency was commenced by Lieutenant Shortrede, on an independent base and point of departure. This survey proceeded in an unsystematic manner until it was brought under Colonel Everest's control in 1831, when, finding that no use could be made of this confused net of triangulation, the Colonel directed that the longitudinal series should be taken up where he left off in 1823. This was concluded in 1841, the series extending over a distance 315 miles in length.

The space at our disposal will not admit of a detailed account of the several series of triangulation carried out by the Trigonometrical Survey Department; they will, however, be seen by reference to the accompanying map. Besides the great arc series, extending from Cape Comorin to Dehra Dhoon, there are two longitudinal series, the one extending from Cachar, in Assam, to Peshawur, and the other from Calcutta to Kurrachee: between these are numerous series of triangles, those to the east of the great arc being at distances of about one degree, or 60 miles apart, taking meridional directions, thus forming what is called a gridiron system, similar to that adopted in the French and Russian surveys. Base lines are measured at the extremities of the longitudinal chains, and at the points where the chains cross Colonel Everest's arc; thus the triangulation is divisible into large quadrilateral figures, with a base line at each corner.

Colonel Everest was succeeded in the appointment of Superintendent of the Great Trigonometrical Survey and Surveyor-General of India by Captain (afterwards Sir) Andrew Waugh, in December, 1843, who held the combined offices for seventeen years. Sir Andrew Waugh left the service in 1861, when he was succeeded by Colonel J. T. Walker, R.E., as Superintendent of the Great Trigonometrieal Survey, and by Colonel Thuillier, R.A., as Surveyor-General of India, both which officers respectively fill those appointments at the present time.

The charts of the Trigonometrical operations are zincographed on a scale of 4 miles to the inch, and the geodetical co-ordinates for each station with azimuths and linear distances are entered upon them, so that each chart forms a brief but complete record of the survey results. Skeleton charts of levels, on a scale of 2 miles to the inch, are also prepared and photozincographed; these show the combined results of both trigonometrical and spirit levelling reduced to the common datum of the mean sea-level of Kurrachee harbor.

Revenue Survey.—The Revenue Survey Branch, in the Bengal Presidency, first commenced in the year 1822. It comprises a scientific periphery admeasurement of the land by means of angular and linear measurements, performed with theodolites and steel chains; and its operations extend into such parts of the country as are under British administration and yield a fair revenue. It is a definition and survey of village boundaries and estates, and may also be termed a large scale topographical survey, as it affords accurate topography of every district falling within the scope of its operations. The system followed is that of traversing with the theodolite and steel chains, known as Gale's method of land-surveying, modified to secure greater accuracy and efficient checks on both the boundary and interior detail measurements. Large areas are first traversed with the better class of small theodolites having from 12 to 8 inch horizontal circles, starting from an initial station, where the azimuth is observed, to obtain the true bearings of stations in advance, the distances between stations being measured with steel chains twice over and repeated in rough ground, or wherever any doubts arise. These traversed areas, called main circuits, being in the first instance traversed and proved, afford a complete check on the minor or block circuits into which they are subdivided; and these minor circuits, being in their turn traversed and proved true on the basis of the main circuit containing them, reduce the errors in the village boundary work to a minimum. The trifling angular and linear discrepancies which may occur in the village traverse circuits are adjusted *inter se.*

The interior or detail survey, which is filled in by plane-table or compass and chain, rests on these small village polygons, plots of which are furnished to the native plane-tablers. The stations of the main circuits are permanently marked, and the masonry platforms which mark the tri-junctions of villages are, whenever practicable, made theodolite stations.

The boundaries of villages are measured by offsets taken to all boundary pillars from the lines enclosing the village polygons, these linear and offset measurements being carefully recorded in the village boundary field-book.

Along the Revenue Survey lines of levels, all masonry platforms marking the junction of three villages which fall on or near the line are invariably adopted as permanent bench-marks. These being all marked prominently on the maps of the Revenue Survey, the entry of the data will be readily and easily made, showing the height of each bench-mark above the mean sea-level, as determined by starting from and closing all the lines of Revenue Survey levels on the Great Trigonometrical stations, or the bench-marks of the principal series of levels of the Great Trigonometrical Survey of India.

In connection with the Revenue Survey, levelling operations were carried on, during 1868–69, in Oudh and Rohilcund, and they have subsequently been extended to the central provinces, Bhamulpoor and Bengal. The object of these is to run series of levels across districts not yet contoured, and to combine the results of the leveling operations of the Revenue Survey with those already completed, or about to be prosecuted, by the Irrigation Branch of the Public Works Department.

The field mapping is all executed on a scale of 4 inches to the mile.

In addition to the regular professional revenue survey of villages, there has always been a minute measurement of field for assessment purposes, conducted by native agency, entirely under the collector or settlement officers. These are crude operations after native fashion.

In the presidencies of Madras and Bombay, minute cadastral measurements of fields are in progress under European officers; these surveys are essentially for settlement and revenue purposes, and have no connection with the Indian Survey Department, nor are they under the direction of the Surveyor-General of India.

Topographical Survey.—The Topographical Branch of the Indian Survey Department is under the immediate superintendence of the Surveyor-General of India, and had its origin in the Revenue Survey. Its operations are confined chiefly to hilly and jungle-covered ground, yielding but little revenue, in parts of the country not actually under British management, and in friendly native states along the British frontier; and its object is to obtain a cheap, rapid, and reliable first survey for geographical and administrative purposes. The groundwork or basis of its operations is secondary and minor triangulation dependent, on the Great Trigonometrical Survey operations, from which all the initial elements of latitude, longitude, elevation, distance, and azimuth are derived. The triangulation is carried on in a network covering the ground with points or stations at about 3 to 4 miles apart. The instruments employed for the secondary triangulation are vernier theodolites with 12 and 14 inch azimuthal circles; the horizontal observations are taken on four zeros repeated and the vertical angles on two zeros. For the subsidiary or minor network of triangles, theodolites with 7 and 8 inch azimuth circles are used, and the angular measurements are made with two zeros repeated.

The detail work, or delineation of the configuration of the ground, is executed usually on the scale of 1 inch to the mile by means of the plane-table. Some topographical surveys in cultivated or valuable tracts are on a scale of 2 inches to the mile; and a few others, in very broken and wild ground, on a scale of 2 miles to the inch. In addition to the 1 inch survey, the Topographical Branch undertakes the plans of all the important cities, forts, and strongholds in native states; these are mapped on scales varying from 6 to 16 inches to the mile.

ECONOMY TRIALS OF AN AUTOMATIC ENGINE,

CONDENSING AND NON-CONDENSING.

By JOHN W. HILL, M. E.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

In the manufacturing districts of the *New England* States, where fuel for steam purposes is expensive and economy of power of the highest importance, the best class of stationary engine (automatic cut off) is the rule. In the *West* where fuel is less expensive, and economy of power (apparently) a second consideration; the best class of stationary engine is the exception, at least this is true of the history of steam power in the West up to a very recent period.

During the past four years there has been an introduction of better engines in many of the more important manufacturing establishments of the West; particularly in the large flouring mills, where the managers, keen to appreciate exact quantities, have sought to cheapen the power, by substituting for wasteful slide-valve engines such engines as the *Harris—Corliss, Buckeye,* and others of the same class.

Prominent among the wealthy milling firms of Indiana is the house of Gibson and Co., whose extensive mills located at Indianapolis, are known to the "trade" throughout the country. Some two years since Mr. David Gibson, the senior member of the concern, conceived the outlines of a model flouring mill; and, being a man of wonderful energy, the idea quickly took shape, and about one year ago he had his model mill ready to grind. Every piece of machinery, from the engine that furnished the motive power, to the "packer" that barreled the flour ready for shipment, was the best that money could purchase or experience suggest.

Twelve run of 48″ buhrs do the grinding, these are driven by belts, the line shaft being coupled to end of engine shaft, and revolving at same speed; the balance of machinery, "cleaners, rolls, elevators, conveyers, bolts, purifiers and packers," are all adapted to maximum capacity of grinding machinery.

The engine is a Harris-Corliss single cylinder, 18″×42″ speeded at 75 revolutions, with a 12000 pound fly wheel 16′ diameter. As furnished by the builder, the engine was non-condensing, but shortly after it was started, a jet condenser and air pump were added, and henceforth was run condensing; the condenser attached was provided with a system of "open way" valves, so arranged that the engine might be run condensing or non-condensing at will.

A rotary air pump driven by belt, delivered the contents of condenser into a hot well; the injection is obtained from the canal (distant about 80′) by gravity flow, the level of water in the canal being 18″ higher than spray pipe in the condenser. From the hot well the water is drawn by a Knowles direct acting steam pump, and forced against boiler pressure through a Berryman exhaust heater into the top forward end of boiler; thence through a Snowden pipe to rear of boiler, where the feed pipe bends down and delivers the water into the mud drum; the exhaust from engine passes through heater on the way to condenser.

The suction pipe connecting pump with hot well, is provided with a branch to the canal, to be opened when engine is run non-condensing, a stop valve in main pipe shutting off connection with hot well.

The boilers furnishing steam to the engine are two in number, 20′ long 54″ diameter, these are set in an independent boiler house alongside engine room; each boiler has (2) flues 11″ diam. and (4) flues 8″ diam.; the furnaces are continuous (with no division wall), and are 4′ deep with a width of 9′ 6″, a front connection, common to both boilers, conveys the waste furnace gases to a large brick chimney built outside of boiler house; the boilers have a mud drum connection below, and steam drum connection above at mid-length; a pipe rising from center of steam drum makes direct connection with engine.

The boilers were previously in use in another mill belonging to the Messrs. Gibson, and, as dimensions indicate, were not calculated for maximum economy. (Vide Table Boiler performance).

About ten months after starting the mill, the proprietors decided to subject the power plant to an economy test, making equal runs with engine, condensing and non-condensing.

The writer was employed to conduct the trials and work up the *data* obtained. Preparations were commenced *Aug.* 18*th.*, and first run (condensing) made *Aug.* 22*d.*, second run (non-condensing) *Aug.* 24*th.* The trials consisted in measuring coal charged to furnaces; water pumped into boilers; quality of steam furnished; power developed by engine; and work done in the mill. The mill is usually started at midnight, Sunday night, and stops at midnight the following Saturday night, running continuously 144 hours without moving the stop valve at the engine, providing no accident besets the machinery. Previous to the tests, however, the mill had been in operation about twenty-four hours, this length of time being considered sufficient to establish the "regimen" of furnaces, boilers and balance of machinery.

The run with engine condensing began at 7.30 A M. *Aug.* 22*d.*; after first trial engine ran without interruption until 8.15 A. M. *Aug.* 24*th.*, when run with engine non-condensing began, each trial was of eight hours, duration.

The coal was weighed in charges of 275 pounds and delivered to the fireman; the residue at close of run being deducted from aggregate weight and balance held to represent quantity fired. Previous to commencement of run the fires were sliced and leveled preparatory to a new charge; the same condition as nearly as possible was obtained at end of run, no drawback was allowed for ash or residual quantities in the furnace.

The water supplied to boilers was pumped by hand from the hot well into a large cask mounted on an elevated stage; this was provided with an overflow at the top, and a glass water gauge with a stick lashed and sealed to the tube, in the side of the cask near the bottom; at the top of the cask a number of baffle sticks were so arranged that when the water appeared at the overflow, they were immersed nearly the entire depth; these had the effect of diminishing the quantity of water necessary to make the final inch in filling the cask, and reduced the liability to error in regulating the volume of water pumped in; besides preventing sensible agitation of the water at the surface.

After the cask was pumped up, the assistant mounted the stage and read the temperature of water, entering the observation in his note book; the water was then drawn off into a similar cask placed alongside; the second cask was connected with the suction pipe of boiler feeder; both casks were of same capacity, about 230 gallons, although the quantity measured in the upper cask was some twenty-five gallons less than this. In drawing off the contents of first cask the assistant noted the decline of head by the glass gauge, and when this was coincident with the top of the stick attached to the tube the outflow was stopped; time of emptying recorded, and the cask pumped up until water appeared at overflow. In order that the time of emptying measuring cask might serve as an index of the rate at which water was used by the boilers, an overflow was provided in the suction cask; the run being commenced with the water in this cask at the overflow, and each time the measuring cask was emptied the assistant was expected to return the level to this point (with a single error at commencement of condensing run this was done); the overflow in the suction cask was provided with a drip pipe, and the water caught in a pail and returned to the tank when level had subsided. In order to check the record of the assistant operating the water casks, the men at the pumps reported to the writer in the engine room each cask as it was pumped up and a tally was made, and the number of tallies held to represent the times measuring cask was filled.

At end of run the water was returned to overflow in suction cask, and remainder of water in measuring cask drawn off and weighed; the weight deducted from weight at time temperature of water was noted with full cask, was held to represent the partial draft.

The capacity of measuring cask was obtained by filling to overflow, noting temperature, drawing off and weighing, when level of water was reduced to gauge point on glass tube, the temperature was again noted, and corrected weight for difference of temperature.

To guard against error in fixing volume of cask, the measurement was repeated with water at reduced temperature; the difference by weight corrected for temperature being less than five pounds or about 00.3 per cent. The last test was made with great care, and capacity of cask was based on this record.

Previous to trial of engine non-condensing, the suction of hand pump was run out and turned down into canal, from which source water was pumped for this run. Measuring and suction casks were unchanged for non-condensing trial.

The temperature of water to boiler feeder was noted in the suction tank.

The temperature of feed water to boilers was taken in the feed pipe between heater and check valve.

The temperature of injection was taken in the canal at entrance of injection pipe.

The temperature of delivery to hot well was taken at the overflow.

The pressure of evaporation was taken in "connection" from steam drum; the gauge was so situated as to avoid effect of flow into steam pipe of engine.

The pressure was again taken in the steam pipe immediately above stop valve. [The gauge used at this point (10″ Ashcroft) was an exceptionally good one, having been in constant use two years in connection with the indicators, and being regularly proved after each "test" can be accepted as near reliable as metallic steam guages ever are.]

The vacuum was taken in the engine room by connection with condenser; the guage in use previous to the trials had suffered a derangement, the index reading 4.5 inches too high; this deduction was made from each reading.

The water primed with the steam was determined by weighing into a half barrel mounted on a nicely balanced (Fairbanks) platform scale 98.5—99.5 pounds of water at normal temperature, connection by $\frac{3}{4}''$ pipe with steam drum was then opened, and water of condensation and steam blown out, until scale balanced at 100 pounds; initial temperature was then read and five pounds of steam blown out of the pipe and condensed, and final temperature read. The range of temperature—initial and final weights—and final temperature of contents of barrel, in connection with the pressure of steam at time observation was made, comprised the data for estimating the "per centum" of water mechanically mixed with the evaporation.

A pair of Thompson indicators one at each end of cylinder was used in taking the diagrams; motion being obtained from cross head of engine by links and bell cranks mounted on a substantial gallows frame, a long $2'' \times \frac{3}{4}''$ rod playing through guide standards braced from the floor, passed back to cylinder and communicated the reduced motion to indicator drums.

During condensing run (30) springs were used in the indicators, and during non-condensing run (40) springs were used.

A positive counter mounted on the

gallows frame and taking motion from small bell crank noted the revolutions of engine; this was thrown in gear at commencement of run, having been read and entered in the log, and eight hours later, to the second, was thrown out of gear and read; the next reading held to represent the revolutions of crank during run; as a check upon the counter it was read at irregular intervals, and speed of engine for intervals compared with average speed for whole run.

The boilers were opened and cleared of scale and sediment previous to trials.

The coal fired is known as *Highland* (Clay County) a species of the celebrated Indiana block coal.

During both "runs" the mill was worked at maximum capacity, quality and condition of wheat considered; it may not be out of place to remark, that in this mill the Messrs. Gibson made what is known as "patent process flour," thus of the (12) run of buhrs, (7) run operated on wheat, and (5) run on "middlings," "bran" and "tailings." In making flour by this process it is customary to grind 7–8 bushels of wheat per hour per run of buhrs (48″); during the trials 7.5 to 8 bushels were ground per hour per run of wheat stones.

The record of work in the mill was based on the barrels of flour packed; this being regarded as an approximate index of the amount of work done by balance of machinery; the amount of work done by storage elevator was greatest during non-condensing run.

Hourly reports of condition of machinery in the mill were made by the chief miller, and entered in the writer's notes. The following table exhibits the amount of machinery driven and disposition during each run,

TRIAL "CONDENSING."

Started 7.30 *A.M.* *Aug.* 22*nd.*

7 Run on wheat.
3 " middlings.
2 " red dog.
Cleaner.
Rolls (2).
Bolts (all).
Purifiers (5).
Elevators and Conveyors (all).
Packer (1).

At 9.15 A.M. Lightered 3 run on "middlings."
" 10.15 " Changed 2 run from "red dog" [to "bran."
" 10.30 " Storage elevator on.
" 10.30 " Lightered 2 run on "bran."
" 1.00 P.M. Storage elevator off.
Stopped 3.30 *P.M.*

TRIAL "NON-CONDENSING."

Started 8.15 *A.M.* *Aug.* 24*th.*

7 Run on wheat.
3 " middlings.
2 " bran.
Cleaner.
Rolls (2).
Bolts (all).
Purifiers (5).
Elevators and Conveyors (all).
Packer (1).
Storage elevator.

At 8.30 A.M. Changed 2 run from "bran to "red dog."
" 9.55 " Changed 2 run from "red dog" to "bran."
Storage elevator irregular.
Stopped 4.15 *P.M.*

Remark.—Starting and stopping in above table merely denotes the time run was held to commence and end; the engine and machinery ran without interruption.

In the following tables under the head "Dimensions of Boilers," dimensions of grate are also given; under the head of "Dimensions of Engine," *static* dimensions only are given; in this table the "factor of H.P. due area of piston" is a constant for an 18″ piston corrected for diameter of rod, and is obtained thus: Let "A" represent area of piston, "*a*" area of rod thus, $\text{“F”} = \frac{A-\frac{a}{2}}{33000}$ piston speed regarded as unity. If piston speed be expressed in "feet per second," substitute for 33000 ÷ 550.

"DIMENSIONS OF BOILERS."

Boilers		2
Length	ft.	20
Diameter of shell	ins.	54
Flues (each boiler)		2—11″ 4— 8″
Heating surface in shell (⅔ exposed)	sup. ft.	170
" " flues	"	282.72
" " end plates	"	13.12
" " (both boilers)	"	931.68
Grate surface	"	38.
Ratio grate to heating surface		24.52
Calorimeter (flue vent)	sup. in.	782.24
Ratio calorimeter to grate surface		7

"DIMENSIONS OF ENGINE."

Diameter of cylinder	ins.	18
Stroke	"	42
Area piston	"	254.46
" " rod	"	7.068
Factor of H.P. due piston		.0076038
Clearance in parts of Stroke "Stroke 100"		2.50

In the following table of boiler performance, the averages of observations only are given; in making the "log" of Trials, however, observations of temperatures and pressures were made quarter-hourly, and observations for "primage" data made half-hourly, observations of temperatures and pressures were made at beginning and end of run, and the mean of first and last observations taken in the average.

The coals fired were from same invoice, and assumed to be similar in quality for both runs.

"PERFORMANCE OF BOILERS."

	Aug. 22.	Aug. 24.
Duration of run.......Hours	8	8
Pressure of evaporation....	63.75	81.37
Temperature water in hot well..................	112.	
Temperature water in canal		76.05
" " from heater................	144.40	195.00
Temper'ture added by he'ter	32.40	118.95
Percentage water primed with steam..........	18.04	11.68
Water pumped into boilers: 11 casks at 1681.76, partial draft 735.00	19234.36	
15 casks at 1690.78, partial draft 1294.00		26655.70
Water per hour pumped into boilers...........	2404.30	3331.96
Water entrained..........	433.73	389.17
" per hour into steam	1970.57	2942.79
Coal charged during run...	3447.	4814.
" " per hour.....	430.87	601.75
" " " per sup. ft. of grate.......	11.34	15.84
Steam per pound of coal...	4.573	4.890
" " sup. ft. of heating surface...............	2.115	3.159

"CALCULATED PERFORMANCE."

Thermal units accounted for by steam per pound...	1064.2	1017.9
Thermal units accounted for by water per pound...	166.48	129.60
Steam per pound of coal from 212.............	5.212	5.240

In the following Table the performance of engine has been detailed; the friction of engine was not determined by trial but is based upon previous experiments by the writer on a 16″ × 48″ Harris-Corliss engine at the *Cincinnati Expositions* of 1874 and 1875. In the trial of the first year the friction pressure with engine empty and running at load speed was 1,888 pounds, and for second year under same conditions was 2,039 pounds; the friction pressure of Gibson engine has been assumed as a mean of above, or 1.963 pounds per sup. inch of piston. Extra friction due to load has been taken at 5 per centum of gross load; the net effective horse power in this case is the dynamometer horse power, as the manner of taking off motion from engine, by coupling line shaft to out board end of main shaft, dispenses with slip of belt or friction of gearing.

In determining steam to engine per hour by casks, two deductions have been made from "steam per hour" under head of boiler performance, the first for steam and water to calorimeter for primage tests, and the second the steam used in operating the boiler feeder; the first deduction has been estimated as follows: there were for condensing run 13 pounds of water of condensation blown through calorimeter pipe to "clear" it, or 1.625 pounds per hour; although this was steam and water in proportions as shown by "boiler performance," when it entered the pipe it was blown out as condensation and should be deducted from "net steam to engine;" of this however 18 per centum or .29 pounds per hour has already been deducted as "water entrained," leaving 1.333 pounds in the steam per hour to engine. The steam drafts to calorimeter were uniformly 5 pounds, and 16 drafts were made during run; of the quantity thus diverted 18 per centum has been deducted (from water charged to boilers) as water entrained: the remainder 65.6 pounds is in the "net steam"; it is obvious that this was unavailable for useful work, and 8.2 pounds of steam (per hour) are therefore deducted from net steam to engine.

For non-condensing run the deduction from steam to engine per hour are for condensation blown out of calorimeter pipe $\frac{24-(24\times.1168)}{8}=2.65$ pounds and for calorimeter drafts

$$\frac{81.5-(81.5\times.1168)}{8}=9 \text{ pounds.}$$

It is generally conceded by engineers that the most economical method of pumping in feed water is by motion direct from engine; providing capacity of pump (allowance being made for loss of action) is exactly proportioned to requirement of boilers; estimating power expended in feeding boilers in this manner would be to charge the "steam pump" with less steam than it consumed.

The writer is not aware of any published reports on the *economic* efficiency of direct acting steam pumps; and even if there were such they would probably be of no real utility in this direction, as the builders change their plans so rapidly that new tests every few weeks would be necessary to keep pace with them.

From such indicator diagrams (as have come under the writer's observation) of steam development in direct acting steam pumps it appears, that the efficiency of steam is considerably less than in a well made slide valve engine of same cylinder bore; when we consider the piston speed—expense of steam in moving the valve—and use of steam without expansion in the steam pump; it is not difficult to understand this.

Assuming the expense of 60 pounds of steam per hour, per H. P. developed in feeding the boilers, then it remains to show what power was required for each run. For condensing run 2404.3 pounds of water per hour were pumped in; neglecting difference of areas of check valve, this quantity was forced in against an average pressure of 63.75 pounds per square inch. The hydrostatic head corresponding to this pressure has been calculated thus: resistance discharge side of pump piston 63.75 pounds; resistance on suction side of pump piston (vacuum corresponding to lift of $12''=.4332$ pounds); total pressure and vacuum 64.203 and $64.203\times2.308=148.18$ feet, hence, power due water pumped in becomes $\frac{148.18\times2404.3}{33000\times60}=.18$ H.P. allowing 10 per centum for loss of action and 10 per centum for friction, the power becomes $\frac{.18}{.80}=.225$ and steam per hour $.225\times60=13.50$ pounds.

For non condensing run the water per hour pumped into boilers was 3331.96 pounds, pressure 81.37, vacuum 1.09 pounds, total pressure and vacuum 82.46, corresponding head 190.31 and power .3192; allowing for loss of action and friction as before, the power becomes .399 H.P. and steam per hour 23.94 pounds.

The ratio of total indicated work of engine to power expended in feeding the boilers becomes for

"*Condensing*" *run*..........	.00213
"*Non-condensing*" *run*.......	.00347

"Performance of Engine."

	Condensing.	Non-Condens'g.
Duration of run.......Hours	8	8
Pressure in the pipe...Pds.	58.50	76.37
Temperature of injection..	81.80	
" " hot well..	112.00	
" " steam at terminal pressure......	203.19	
Barometer (assumed) inches	29.53	29.53
Vacuum in condenser "	21.83	
Revolutions...............	74.288	73.600
Piston speed..............	520.016	515.200
H. P. due piston speed.....	3.954	3.917

"By the Diagrams."

Initial pressure (above atmosphere)..........	57.015	74.042
Cut off in parts of stroke, Stroke 100............	10.827	18.891
Release in parts of stroke, Stroke 100............	100.000	99.000
Termin'l pressure (absolute)	12.329	19.581
Counter " " at mid stroke...	4.594	15.933
Exhaust closure in parts of stroke................	3.478	8.816
Mean effective pressure....	26.928	29.471
Friction pressure (assumed)	1.962	1.962

"The Power."

Indicated horse-power.....	105.473	115.437
Friction " "	7.757	7.685
Gross load " "	97.716	107.752
Extra frict'n " " due to load................	4.885	5.387
Net effective horse-power..	92.831	102.365
Per centum of indicated power available.......	88.014	88.684

"Economy."

Net steam per hour to engine	1961.03	2931.14
" " per indicated horse-power.....	18.593	25.391
Net steam per hour per indicated horse-power by the diagrams..........	16.587	21.983
Per centum steam accounted for by indicator.......	89.211	86.578

"Cost of the Power."

Coal per indicated horse power per hour.......	4.085	5.213
Do. do. evaporation 9 to 1	2.076	2.832
Relative efficiency.........	100.00	73.30
Economic gain by use of vacuum..............	36.41	

"Cost of the Flour."

Barrels ground and packed	89	93
Horse power per barrel per hour..................	9.480	9.947
Coal per barrel (actual)....	38.73	51.76
Do. do. evaporation 9 to 1	19.71	28.11
Relative efficiency.........	100.00	74.826
Economic gain by use of vacuum..............	33.34	

To grind and manufacture a barrel of flour ready for shipment with 30 pounds of coal is regarded by Western millers as extraordinary; indeed with many millers using steam power it is believed to be *impossible;* from the above record it would appear that, with standard boiler performance the Messrs. Gibson can do it on two-thirds of this quantity, or 20 *pounds of coal per barrel of flour ready for delivery.*

CANADIAN NARROW-GAUGE RAILWAYS.

By EDMUND WRAGGE, M. Inst. C. E.

Minutes of the Proceedings of the Institution of Civil Engineers.

As the narrow-gauge railways constructed into the back country from Toronto have now been in operation for several years, it may not be uninteresting to present the results which have been obtained in working them, in comparison with railways having a gauge of 4 feet 8½ inches and of 5 feet 6 inches passing through somewhat similar country, and having traffic similar in character.

The Toronto, Gray, and Bruce, and the Toronto and Nipissing railways were projected to open up the country, the former in a north-westerly, the latter in a north-easterly direction from Toronto. The Northern railway of Canada, 5 feet 6 inches gauge, occupied the territory between these two railways, and it was felt that unless a railway could be constructed at a much less first cost than had previously been done in Canada, the country could not for some years enjoy railway facilities; hence the determination to construct these railways of a gauge of 3 feet 6 inches.

The Toronto, Gray, and Bruce railway extends from Toronto on Lake Ontario to Owen Sound on the Georgian Bay, a distance of 122 miles, with a branch from Orangeville to Teeswater 69 miles in length, giving a total mileage of 191 miles. It passes over an elevation of 1,462 feet above the level of Lake Ontario, and has ruling gradients of 1 in 60 towards Toronto against the heaviest portion of the traffic, and of 1 in 50 from Toronto, in which direction the traffic is lighter. There is one curve of 462 feet radius, and a few others of 480 and 500 feet radius, having a total length in all of 6,000 feet. The curved portion of the railway is 21½ per cent. of the total length, 78½ per cent. being straight. The extent of level railway is 21 per cent.; with gradients easier than 1 in 100, it is 51 per cent., and with gradients of 1 in 100 and steeper, 28 per cent.

The Toronto and Nipissing railway leads from Toronto on Lake Ontario to Coboconk, a distance of 87 miles, the first 9 miles from Toronto being overcome by means of a third rail laid on the Grand Trunk railway. The summit elevation is 893 feet above Lake Ontario, and is reached at a distance of 35 miles from Toronto. The ruling gradients in either direction are similar to those on the Toronto, Gray, and Bruce railway. There are two curves of 600 feet radius (1,650 feet length in all), but no others sharper than 800 feet radius. The curved portion of the railway is represented by 23½ per cent. of its total length from the point at which it diverges from the Grand Trunk railway, and the straight portion by 76½ per cent.; for 19¼ per cent. of the same distance it is level, for 57½ per cent. it has gradients less than 1 in 100, and the remainder, 23¼ per cent., has gradients of 1 in 100 or steeper.

The structures on both railways, station buildings, bridges, culverts, cattle guards, &c., are of timber, and both are fenced throughout their entire length.

The permanent way consists of iron rails weighing 40 pounds per lineal yard, fished at the joints, and spiked to the sleepers by dog spikes. The sleepers, which are at an average distance of 2 feet 6 inches apart from center to center, are 7 feet 6 inches long by 8 inches by 5 inches, and are either of tamarac, hemlock, or black ash. Cedar and rock elm have also been used for renewals, and the new sleepers are 6 inches in thickness, instead of 5 inches as at first. Ballast to a thickness of 12 inches under the sleepers has been used, and the sleepers are well boxed up.

The rolling stock comprises several classes of engines :—1. Engine weighing 16 tons, which have been found too light for the traffic to be accommodated. 2. Engines of 21 tons, having 6 wheels coupled, which are very useful; those on the Toronto, Gray, and Bruce railway making a daily run of 122 miles, with passenger trains weighing 60 tons, and carrying an average of one hundred passengers. These engines make an average journey of 68 miles while consuming a cord of wood of 128 cubic feet, or 125 miles for 1 ton of 2,000 pounds of bituminous coal. The schedule time of these trains is 16 miles an hour. 3. Engines of 26 tons, having six wheels coupled, are used for mixed trains on the Toronto, Gray, and Bruce railway, and take a gross load of 180 tons at a schedule time of 14 miles an hour, running 54 miles for a cord of wood. On the Toronto and Nipissing railway they are used entirely for freight purposes, and haul a gross load of 300 tons at an average speed of 10 miles an hour. 4. One Fairlie engine of 42 tons on each railway is found to travel easily on the rails, and is capable of taking considerably heavier loads than the other engines. 5. Engines of 30 tons, with eight wheels coupled, "Consolidation" pattern, are in use only on the Toronto, Gray, and Bruce railway, and haul a gross load of 360 tons up the ruling gradients of 1 in 60. Their average journey is 35 miles for a cord of wood.

The passenger carriages were at first 30 feet in length, with an extreme width of 8 feet 6 inches. Those lately put on the railway are 43 feet in length and 8 feet 8 inches in width, and they run more steadily and with greater ease to the passengers.

The wagon stock was in the first instance 15 feet and 18 feet long by 8 feet wide, and upon four wheels, in the English style; but all new stock has been constructed after the American model, and, as well as the passenger carriages, is placed upon four-wheel trucks. All wheels are now 30 inches in diameter, 24 inch wheels having been at first adopted with the view of keeping the center of gravity as low as possible.

The box cars are 29 feet 1½ inch long by 8 feet wide; they have a carrying capacity in weight, of 24,000 pounds; in area, of 205 square feet; in bulk, of 1,218 cubic feet; and a dead weight of 15,500 pounds.

The platform cars are 30 feet long by 8 feet wide, having a carrying capacity in weight, of 24,000 pounds; in area of 240 square feet; they have frequently been loaded to a height of 6 feet, giving a capacity in bulk, of 1,440 cubic feet; the dead weight averages 13,000 pounds.

The floor-level is 3 feet 4 inches above the rail, and the center of the draw-bar, which is of the common American link and pin pattern, is 2 feet above rail-level.

Owing to the increased weight of rolling stock put on the railways since they were opened, and taking into consideration the increased traffic, the rails now being laid on both railways weigh 56 pounds to the yard. Already on the Toronto, Gray, and Bruce railway a length of 40 miles at the Toronto end has been laid with the heavier rail, the lighter one having been taken up and used on the branch to Teeswater. These rails are of iron, and on the Toronto and Nipissing railway a length of 7 miles of permanent way has been renewed with steel rails weighing 56 pounds to the yard.

It may be interesting to remark, in connection with the rolling stock, that two passenger carriages on the Toronto, Gray, and Bruce railway have each travelled upward of 100,000 miles, the trucks of which have never been repaired since they commenced running; they have the same wheels (chilled cast iron), the same springs, the same brasses, &c.

For the purposes of comparison of traffic and working expenses, the following information respecting the Northern railway and the Midland railway, the former previously referred to, the latter extending from Port Hope to Orillia and Wabushene, and crossing the Toronto and Nipissing railway at Woodville, is given. The length of the Northern railway of Canada in the year ending 1875 was 165 miles; the sharpest curve was of 1,432 fete radius; the steepest gradient going north was 1 in 88, going south (towards Toronto) 1 in

100. The summit-level is about 750 feet above Lake Ontario, the weight of rail 56 pounds per yard. The weight of the engines is from 30 to 35 tons each. The gauge is 5 feet 6 inches. The length of the Midland railway of Canada in the year 1875 was 129 miles. The summit-level is about 700 feet above Lake Ontario; the weight of rail 56 pounds per yard. The gauge is 4 feet 8½ inches. The stations on the Toronto and Nipissing railway are at an average distance apart of 4.8 miles; on the Midland railway of Canada 5.1 miles; on the Northern railway of Canada 5.4 miles; and on the Toronto, Gray, and Bruce railway 8.7 miles; the latter railway passing through a more thinly settled country than the others, although it is now being fast developed.

The traffic and working expenses of these four railways are shown in the following:

Tabulated Statement of Receipts and Expenditure on Railways of three feet six inches Gauge compared with Railways of five feet six inches and four feet 8½ inches Gauge.

Description.	Toronto, Grey, and Bruce Railway. 3 feet 6 inches.	Toronto and Nipissing Railway. 3 feet 6 inches.	Northern Railway of Canada. 5 feet 6 inches.	Midland. Railway of Canada. 4 feet 8½ inches.
	Twelve months ending 30th June, 1876.	Twelve months. ending 30th June, 1876.	Twelve months. ending 31st December, 1875.	Twelve months ending 31st December, 1875.
Total earnings.........	$372,336	$207,234	$744,598	$284,322
Total working expenses.	$233,428	$120,468	$473,963	$179,221
Percentage of working expenses...........	62.9	58.1	63.6	63.03
Miles open for traffic...	191	87	165	129
Earnings per mile of railway............	$1,949	$2,383	$4,512	$2,204
Total train miles.......	398,681	205,105	641,827	275,560
Earnings per train mile.	93.39 cents	100.10 cents	116.01 cents	103.18 cents
Working expenses do...	58.55 "	58.73 "	73.84 "	65.03 "
Net income do.........	34.84 "	41.37 "	42.17 "	38.15 "
Total car miles.........	2,724,696	1,195,291	4,690,245	1,561,729
Earnings per car mile...	13.66 cents	17.33 cents	15.87 cents	18.20 cents
Working expenses do...	8.56 "	10.08 "	10.15 "	11.48 "
Net income do.........	5.10 "	7.25 "	5.72 "	6.72 "
Total capital account including all discounts, etc.).......	$4,159,282	$1,600,000	$7,893,569	$4,132,034
Cost per mile of railway.	$21,776	$18,390	$47,840	$32,031

Upon making a comparison of the above figures, it will be found that:

Broad-gauge railways, average cost per mile =$39,935
Broad-gauge railways, net income per mile =$1,244
Equivalent to 3 per cent. on the cost.
Narrow-gauge railways, average cost per mile =$20,083
Narrow-gauge railways, net income per mile =$812
Equivalent to 4 per cent. on the cost.
Working expenses per train mile, broad-gauge railway....... 69.44 cents.
Working expenses per train mile, narrow-gauge railway......58.64 cents.
Working expenses per car mile, broad-gauge railway........10.81 cents.
Working expenses per car mile narrow-gauge railway...... 9.32 cents.
Net income per train mile, broad-gauge railway..............40.16 cents.
Net income per train mile, narrow-gauge railway..............38.10 cents.
Net income per car mile, broad-gauge railway............. 6.22 cents.
Net income per car mile, narrow-gauge railway............. 6.17 cents.

These facts, taking into consideration the difference in outlay between the two classes of railways, afford a favorable comparison for the narrow-gauge railways, which pass through a country only lately opened to railway facilities, while the broad-gauge railways have for a great portion of their length been open for many years, the main line of the Northern railway having been in operation upwards of twenty years.

REPORTS OF ENGINEERING SOCIETIES.

American Society of Civil Engineers.—The last issue of the Transactions contains:

Proportions of Eye-Bar Heads and Pins, as determined by Experiment, by C. S. Smith. Wing-Dams in the Mississippi, above the Falls of St. Anthony, by E. P. North.

The board of officers elected on November 7th, stands as follows:

President, E. S. Chesborough: Vice Presidents, Albert Fink, W. M. Roberts; Secretary, John Bogart; Treasurer, J. James R. Croes; Directors, William A. Paine, Joseph P. Davis, George S. Greene, C. Shaler Smith, C. Vandervoort Smith; Librarian, John Bogart.

Liverpool Engineering Society.—This Society held its usual fortnightly meeting on the evening of the 26th ult., in the rooms of the Royal Institution, Colquitt street. The chair was occupied by Mr. H. O. Baldry, A. I. C. E., Vice-President. On the completion of the business of the Society, a paper was read by Mr. Dukinfield Jones, member, entitled "Some of the Advantages of the Metrical System in Engineering." After first pointing out that the metre is generally supposed to be the ten millionth part of a quadrant of the earth's meridian, he did not invite criticism on this point, as he observed that for all practical purposes it would be just as useful if it were the eleven millionth part. All that it is necessary to admit is that the metre is a convenient unit of measure, and that its length is equal to the distance between two points on a platinum bar in Paris, and is equivalent to 39.37079 English inches. The subdivisions are one-tenth part or decimetre, the hundredth or centimetre, and the thousandth or millimetre. He dwelt at some length on the inter-dependence of the metrical measures of length, capacity, and weight, pointing out that the litre or cubic decimetre is the unit of capacity, and the kilogramme the weight of a cubic decimetre of distilled water at the temperature of 4 deg. Cent., and with the barometric pressure of 760 milimetres. To illustrate the great saving in figures which may be effected in engineering operations by the metrical system, he exhibited two diagrams representing ordinary railway working sections, and stated that the one prepared on the metrical system had nearly 20 per cent. less figures than the section prepared with English measures, notwithstanding that they both represented precisely the same cutting, and gave identical information at every point. He concluded his paper by suggesting the probability of a decimal coinage being introduced before the metrical system of weights and measures.

Manchester Scientific and Mechanical Society.—The winter session of the above society was opened recently by an inaugural address to the members, delivered by the President, Professor Osborne Reynolds, M. A., F. R. S., who selected as the chief subject of his discourse the possibility of reducing the quantity of smoke and pernicious gases poured into the atmosphere at present. The ways in which this could be done, if it was possible, might be summed up as follows: (1) By the better burning of the coal and purifying of the products from soot and sulphur. (2) The more economical use of coal, and hence the reduction of the quantity consumed. (3) The somewhat transcendental, but much more complete method, if it was possible, of substituting some other power for that now derived from coal. The sources of smoke were threefold: the household consumption of coal, the coal consumed in manufactures, and the coal consumed in the production of steam. The first of these sources was altogether beyond the reach of the engineer. With regard to the other sources of the smoke nuisance, he observed that when coal was completely consumed there would be no smoke; but it did not follow that where there was no smoke the coal was completely consumed. It was quite possible to consume the coal under the furnace of a boiler, and that with great advantage in the point of economy; but to do so required constant care and attention, and in looking forward to further reforms it was the small engines which formed the difficulty, for not only did they fail properly to burn their own coal, but they prevented the adoption of measures which might be satisfactorily carried out where the consumption was large. Their chance of further reducing the impurities turned into the air depended therefore greatly on their ability to do away with small engines, which raised the question whether they could supplant their small engines by power derived either from large engines or some other source. Professor Reynolds then proceeded to review the possibility of this at some length, and, in conclusion, said he could not help thinking there was open to the engineer a field of enterprise in which he might not only find remunerative employment for his talents, but in so doing confer a great benefit on his fellow creatures.—*Iron.*

IRON AND STEEL NOTES.

Hardening of Boiler Steel.—Perhaps the greatest development of steel for structural purposes up to this time may be found on railroads. The question of steel rails may be regarded as settled; also of steel tires, crank pins, guide bars, connecting rods, &c. In case of axles and boilers there seems to be some discussion, but no close observer can doubt the ultimate result. In boiler steel the only danger to be apprehended is that there may be enough carbon in the steel to cause hardening in use, although the sheets may have been annealed so as to endure all the cold bending, twisting, punching, and flanging tests successfully. That such annealed sheets will harden very hard in use is well known. A very simple preventive may not be so generally known. Let a piece from each sheet be heated white hot and quenched in cold water or brine. If, after this treatment, it will double over cold, punch, twist, flange, &c., it will never harden in use, simply because it has not enough carbon to cause it to harden under any circumstances. There are instances of boilers that have been in active service for nearly ten years, where only 20 per cent. of a large number have required any repairs, and all are reported in good working condition. It is evident that the life of a boiler must be very long under fair treatment, after it has run for about nine years subject to ordinary wear and has not required any repairs whatever.—*Metallurgical Review.*

On the Cause of the Blisters on "Blister Steel."—In the process of making steel, which is so largely practiced at Sheffield, bars of iron, usually of Swedish or Russian manufacture, are embedded in charcoal powder, and kept heated to bright redness during about a week or ten days, according to the degree of carburization desired. Carbon is thereby imparted to the iron, and steel is the product. The bars operated upon are generally about 3 inches broad and ¾ of an inch thick. How the carbon finds its way even to the center of such bars, is a question not yet satisfactorily solved, though it possesses high scientific interest, and has been much discussed. It is not, however, my intention to consider that question on the present occasion; but to communicate to the Institute experimental evidence as to the cause of the singular phenomenon which accompanies this process of converting iron into steel, viz., the occurrence of blister-like protuberances on the surfaces of the bars. This appearance is so characteristic and so constant, that the name of "blister-steel" is applied to such bars. The protuberances are hollow, exactly like blisters, and vary much both in number and size; some are not larger than peas, while others may exceed an inch in diameter, and they are always confined to the surfaces of the bars, for I have a specimen of "blister-steel" in my collection, in which there is a single blister as large as a small hen's egg, protruding equally from each of the flat opposite surfaces of the bar.

With regard to the cause of these blisters, there has been a difference of opinion. I will take the liberty of making the following quotations on the subject from my volume on "Iron and Steel," published in 1864 :—"They, (*i.e.*, the blisters) appear to be due to internal local irregularities and gaseous expansion from within, while the iron was in a soft state from exposure to a high temperature. There is no doubt that all forged bars, for reasons previously assigned (and which I stated in considerable detail), contain more or less interposed basic silicate of iron irregularly diffused throughout. Now, what should be the effect of the contact of carbon, at a high temperature, with particles of this silicate ? Most probably the reduction of part of the protoxide of iron with the evolution of carbonic oxide; and, if this be so, then it seems to me, the formation of blisters may be satisfactorily accounted for. Admitting this explanation to be correct, a bar, which has been made from molten malleable iron, should not blister during cementation (the term used to designate the process in question of making steel); and, should this prove to be the case, it would not be difficult to prepare such a bar with particles of cinder (ferrous silicate) imbedded, and, by subsequently exposing it in a converting furnace, ascertain positively whether blisters would occur only in places corresponding to the cinder" (page 772),

It has, I think, been conclusively proved that all bar iron manufactured by charcoal finery processes, or by puddling, must contain, intermixed, some of the slag, which results from the conversion of pig-iron into malleable iron by such processes, in which, let it be remembered, the malleable iron is never actually melted. In the quotation which I have given, I mentioned only ferrous silicate as constituting the slag; but I ought, also, to have included free oxide of iron, doubtless magnetic oxide. The bars converted at Sheffield are chiefly Swedish, and are generally manufactured by the so-called Lancashire process.

On a visit to the great steel works of Messrs. Firth, at Sheffield, in February last, Mr. Charles H. Firth was so good as to undertake, at my suggestion, to settle the question whether blistering would occur in the converting process in the case of a bar of iron which had been actually melted and so freed from all intermixture of ferrous silicate or magnetic oxide of iron. The experiment was accordingly made, and with good effect, of confirming, and, I think I might almost say, establishing the correctness of the explanation which I ventured to submit concerning the cause of the formation of the blisters. On the 9th of last May, Mr. Firth informed me that he had melted Swedish bar iron, and cast it into a flat ingot, which he had carburised in the converting furnace in the usual manner; and, at the same time, he forwarded to me a piece broken from the ingot after conversion; this piece was about six inches long, three inches broad, and a little more than half-an-inch (exactly $\frac{7}{12}$) thick; it showed a fracture at each end, characteristic of converted steel, but there was not the slighest indication of a blister.

The other experiment, which I suggested, seems scarcely to be needed, namely, that of cementing a cast bar of malleable iron, in which bits of slag, or magnetic oxide of iron, had been imbedded. But should any one be willing to make such an experiment, probably the best way would be to cast an ingot of Swedish iron, drill a hole or two in it, to the depth of about the center, insert a bit of slag in one hole and a bit of magnetic oxide of iron in another, then plug up the holes hermetically by means of a screw or otherwise, and convert in the ordinary way.

I have great pleasure in publicly acknowledging my obligation to Messrs. Firth for permitting me on several occasions, to visit their works, and for their uniform kindness in other respects; and, I may add, that I have never visited any works from which I have derived more instruction and pleasure.—*Iron.*

RAILWAY NOTES.

A report has been recently published containing railway statistics of Canada and the capital traffic and working expenditure of the railways of the Dominion for the fiscal year ending 30th June, 1876. The total mileage of railways opened in Canada on the 30th June, 1876, was 4,174½, an increase of 330¾ miles as compared with the same period of the previous year. The actual number of miles of railway built during the year was 524, but the length of the European and North American Railway had been over estimated in the former report, and the Maine portion had been counted in, making a deduction of 188½ miles necessary. This, with 4¾ miles of railway returned by three companies as decreased mileage, reduced the net increase during the year over the figures of the previous year to 330¾. The mileage of new railways opened in the fiscal year ending 30th June, 1876, was as follows:—Brantford, Norfolk and Port Burwell, 33 miles; Brockville and Ottawa Extension, 29 miles; Chatham Branch, 9 miles; Great Western, 69 miles; Intercolonial, 185 miles; Kingston and Pembroke, 47½ miles; Montreal and Vermont Junction, 23 miles; New Brunswick, 33 miles; Port Dover and Lake Huron, 63 miles; South-Eastern, 21 miles; Whitby and Port Perry, 11½ miles; total, 524 miles. From the total mileage—5,157¼—has to be deducted the mileage of railways in the United States owned by Canadian companies. This makes the total mileage in Canada 4,929¼ miles, all single track, excepting 79 miles of double track on the Great Western Railway. The gauge of the total mileage is divided as follows: Five feet six inches, 618¼ miles; four feet 8½ inches, 3,938½ miles; three feet six inches, 600½ miles; total, 5,157¼ miles.

Railways of New South Wales.—From Mr. John Rae's excellent report for 1876, we abstract the following:

"The progress of our railways, as exhibited in this return, has been most satisfactory. In 1855, when 14 miles were open for traffic, the number of passengers was 98,846, the tonnage of goods 140, the capital expended £515,347, the net earnings were £3,290, and the interest on capital was only .638 per cent. In the next decade (1865) 143 miles had been opened for traffic, the number of passengers had increased to 751,587, the tonnage of goods to 416,707, the capital expended to £2,746,373, the net earnings to £57,106, and the interest on capital to 2.079. Another decade brings us to 1875, when the number of miles open had increased to 437, the number of passengers to 1,288,225, the tonnage of goods to 1,171,354, the capital expended to £7,245,379, the net earnings to £318,474, and the interest on capital to 4.396 per cent., the largest per centage of net earnings to capital during any year from 1855 to 1875 inclusive.

"19. *Recapitulation.*—The transactions to the end of 1876 may be thus summarized. The Government expenditure for construction amounted to £8,638,362, the interest on which was £416,640, or 4.82 per cent. The capital expended on open lines was £7,990,601, and the net earnings were £353,819, or 4.43 per cent. The interest paid by Government was therefore only .39 per cent. in excess of the percentage of net earnings to capital. At the close of the year, 509 miles of railway were open, and 179½ miles under contract to be completed by the end of 1877. The rolling stock consisted of 101 locomotives, 344 passenger-carriages, and 2,217 goods-trucks. The number of railway employees amounted to 2,243, and the wages to £237,176 18*s.* 10*d.*, an increase over 1875 of £34,104 8*s.* There were 32 vessels employed in the conveyance of railway materials, the cost of which amounted to £50,137 15*s* 4*d.*, and the freight and English charges to £3,268 3*s.* 3*d.*

"During last year, 29,230 passenger-trains and 23,303 goods-trains were run over 1,688,964 miles of railway, the total earnings from which amounted to £693,225, and the cost of working to £339,406, or 48.96 per cent. of the earnings. The number of passengers who travelled was 1,727,730, of whom 301,587 were first-class, and 1,426,143 second class, or in the ratio of 17.47 per cent. of the former to 82.53 per cent. of the latter, besides 5,680 season ticket holders, representing an additional number of 751,216 passenger journeys. The merchandise traffic consisted of 438,025 live stock, 120,397 bales of wool, and 1,244,131 tons of goods. The average earnings per open mile were £1,507, the average expenditure was £738, and the net earnings were £769. The average earnings per train mile were 98.50*d.*, the average expenses 48.22*d.*, and the average net earnings 50.28*d.*

"There was an increase of 94,472 in the number of first-class passengers, of 345,033 in the number of second-class passengers, and of 1,004 in the number of season ticket holders; an increase of £27,929 in the earnings from passenger traffic, of £50,648 in the earnings from goods, and of £78,578 in gross earnings. The working expenses were increased by £43,232, and the net earnings by £35,346. The interest on capital was different on the different lines. On the South and West there was an increase of .21 per cent., on the North a decrease of .45 per cent., and on all the lines combined a slight increase of .04 per cent.; and as this increase of interest was confined to those lines on which alone there was any additional mileage, we may reasonably anticipate a further increase with further extensions of our railways, till the net earnings shall equal or exceed the annual amount of interest paid from the Consolidated Revenue Fund. The public debt for railways would then be practically extinguished, and no difficulty should be experienced in procuring the necessary funds for carrying our main trunk lines to the borders of the adjacent colonies, and connecting them by branches with the chief centres of population in the interior, where there is such a total want of water-communication, and in many parts of which the railway is the cheapest, if not the only possible road.

"It is by reticulating the country with such a network of railways as is indicated by the routes and surveys on the appended sketch map, that we hope to provide means of transit for the produce of our settlers in the pastoral districts, and be enabled to compete successfully with our energetic neighbors for the transport of the staple commodities of our own colony. But the advantages of such an extended system of railroads would not be confined to the mere increase of traffic; for, to adopt the language of His Excellency, Sir Hercules Robinson, in his able speech at the opening of the line to Bathurst, 'with facilities for regular and constant railway communication with our neighbors, the identity of the interests of adjoining colonies will day by day become more apparent; and petty provincial jealousies and rivalries will give place to those feelings of reciprocal sympathy, which will tend to bind these Anglo-Saxon communities in Australia still more closely to each other, and to unite them in the advancement of the glorions mission of their race—the mission of peaceful commerce and human progress.'"

ENGINEERING STRUCTURES.

The Channel Tunnel.—The reports submitted in June last to the "Association du Chemin de Fer Sous-Marin" have been published in excellent style.

We are indebted to the courtesy of M. Michel Chevalier for a copy of this recent publication.

The report gives details of the work of the surveys during 1876. These were, 1st, a thorough examination of the strata of rock upon both shores of the channel. This, of course, included a considerable width of land on either side.

2d. Examining the bottom of the channel by numerous soundings, so that its exact contour is known, together with the character of the bed.

3d. A boring to the depth of 130 metres at Sangatte, on the French side of the channel. This boring extends to about 20 metres below an important clay stratum which underlies the true chalk of this region. Being impermeable to water it affords a guaranty against irruptions of sea water in tunneling operations below it.

These three departments of exploration have been worked with much vigor, and the results are regarded as highly satisfactory.

The report testifies abundantly to the care and skill brought to bear upon these different surveys. Colored maps are made to exhibit, by tints and figures, the difference of stratified deposits and their thickness. The numerous soundings of the "Straits" are carefully reduced to the same phase of the tide.

The examinations resulted in indicating a regular succession of cretaceous strata quite continuous across the Straits, and maintaining a parallelism quite remarkable. In fact, the theory of a quiet and slow subsidence of what is now the bottom of the channel, is well sustained by these careful surveys.

The characters of the successive strata of chalk are therefore well known by previous acquaintance with them near their outcrop. The important property of permeability to water exhibited in different degree by different beds of chalk, is, therefore, satisfactorily settled.

The further progress of this magnificent enterprise will be watched with great interest.

The *Baltimore Gazette* says:—Few people know how great an engineering enterprise is going on in Baltimore county. For one thing alone, a tunnel, six miles and four-fifths long—36,510 feet—is being built underground, for over four-fifths the distance through hard gneiss and granite. It will be the longest tunnel in the country, and there will be only two larger in the world, the Mont Cenis, which is eight miles in length, and the St. Gothard, now in progress of construction, and which is to be nine miles and a-quarter. The fact that the water supply tunnel lies near enough to the surface to allow of numerous shafts, greatly facilitates its construction. The tunnel is a circle, 12 feet in diameter, and extends from the Gunpowder River, about eight miles from the city, to Lake Montebello, the distributing reservoir, near the Hartford turnpike, about a mile and a-half from this city, the direction being 26 deg. west of south. This tunnel will conduct the water from the Gunpowder River to Lake Montebello; thence a conduit 4120 feet long, known as the Clifton Tunnel—from the fact that it passes under a portion of the Clifton Park—conducts the water to a point just south of the Hartford road, where it enters six mains, each four feet in diameter, which conveys the water to the city, a distance of 1900 feet. The country along the line of the works is hilly, and the tunnel varies in depth below the surface from 67 feet to 353 feet. There are fifteen shafts in the main tunnel, the deepest extending 294 feet below the surface. The water rains down the crevices of the rocks, and pours along the bottom of the drift. Gangs of men, each with his miner's lamp attached to his hat, are hard at work, picking and delving in the flinty bowels of the earth, and the monotonous clang of the hammer upon the drill is constantly heard, except when everything is in readiness for firing a mine, when all retire to a safe distance, and thunderous reports roll through the rocky corridors. The work of the tunneling is all done by hand, it being cheaper than the machine-work in a drift of such narrow diameter.

Hydraulic Machinery for Boring Rock.—In connection with the boring or working of rock some improved hydraulic machinery has been invented by Mr. Alfred Brandt, of Hamburg, who proposes to use hydraulic power for pressing a toothed steel boring tube into the rock, and for simultaneously rotating the said tool. The waste water passing from the engine is used for keeping the cutting edges of the boring tool constantly cool, and for removing the abrading material resulting from the boring. A firm abutment for the boring engine is formed by a hydraulic press consisting of a hydrrulic cylinder and piston forced by hydraulic pressure against the sides of the rock or other material. The boring tool is formed of a slightly conical tube of steel, provided at its cutting end with teeth which perform the boring operation and are hardened for this purpose. In the other end of this boring tool a screw thread is cut which serves for connecting the said tool with a tube forming the hollow boring rod. The entire boring rod is composed of a number of such tubes fitted together by bayonet joints. Each tube is of about the length to which the boring engine works. The number of tubes to be used depends upon the depth of the hole to be bored, and one great advantage of the invention is that the depth may be almost unlimited, as the boring rod has only to resist torsional strain, and can, therefore, be made of great length. By means of a movable cylinder working on a fixed piston the pressure against the boring rod is effected; this pressure cylinder moves towards the rock when water under pressure is admitted into it, and returns when the water is allowed to escape. The fixed piston is screwed to the cylinder of a hydraulic press, forming a most firm and solid stand; this hydraulic stand must be regarded as a substantial part of these improvements, as without it there would be much difficulty in obtaining a suitable abutment. The rotation of the boring tool is effected by a very simple arrangement. On two guide pieces are arranged two hydraulic engines coupled together and serving to rotate a shaft which by suitable gearing communicates its motion to the pressure cylinder. The water under pressure is conducted to the machinery by three pipes. The largest of these pipes is composed of pieces provided with joints and ends in a valve supplying the two hydraulic engines with the requisites motive fluid; another pipe entering into the fixed piston supplies the water requisite for moving the pressure cylinder to and fro; the third pipe conducts into the stand the water requisite for forcing out the pressure piston. These three pipes branch out of a single pipe near the boring machinery. The waste water from the hydraulic engines takes its way through short pipes leading into the hollow piston through a tube, and then through the hollow boring rod, escaping finally through the crumbled core and the interstices of the cutting edge or the boring tool, thus constantly carrying off the abraded material. The machine may be calculated to be used with an average water pressure of fifty atmospheres, and the borer may make ten rotations per minute; but these particulars may be varied without departing from the substance of the invention.

RAILWAY BRIDGE OVER THE RIVER TAY.—About six years ago the North British Railway Company found that the increase of traffic on their line rendered the ferries over the Friths of Forth and Tay not only insufficient, but likewise a cause of pecuniary loss. A bridge over the Frith of Forth was projected, approved by Parliament, and actually commenced, but, the difficulties being insurmountable, the scheme was abandoned. Another bridge over the Frith of Forth, unrivalled for novelty of design and for scientific excellence, has since been carried through Parliament by the North British Railway Company. The bridge over the estuary of the river Tay, commenced in 1871, has just been completed. It spans a part of the river nearly two miles wide. It forms a constituent portion of a new branch railway from Leuchars, on the Fife line, to Dundee. Mr. Thomas Bouch, the engineer who designed the bridges over the Deepdale and Beulah gorges in Westmoreland, was the designer of the Tay bridge. Messrs. Charles De Bergue and Co., of Strangeways, Manchester, who obtained the contract in May, 1871, were represented by Messrs. Austin and Grothe. The original design comprised eighty-nine spans, of varying lengths—48 on the Dundee side of the centre, viz., 6 of 28 feet, 25 of 66 feet, 16 of 120 feet, and 1 of 160 feet; and 41 on the Fife side, viz., 3 of 60 feet, 2 of 80 feet, 22 of 120 feet, and 14 of 200 feet. In the course of construction it became necessary to make many changes in the arrangement. Alterations were made in the plan, and modes of erection were changed. The foundation stone of the land abutment on the Fife side was laid. Several difficulties intervened, and six of the workmen lost their lives by the bursting of one of the cylinders in the process of sinking, in August, 1873. The progress of the work, as a whole, was in some degree arrested by the death of Mr. Charles de Bergue, when it became necessary to make new arrangements for the completion of the undertaking. The work was taken in hand by Messrs. Hopkins, Gilkes and Company, in July, 1874. On a careful reconsideration of the extent of work remaining to be executed, and the time which it must necessarily take if carried out as originally intended, it was judged expedient to widen the centre spans, to adopt single large caissons, instead of two to each pier, and to make other important changes, with the view of adding to the strength of the work, and shortening the time still to be employed in completing the bridge. The revised plan, instead of 89 spans, gave 84, namely, 6 of 27 feet, 14 of 67 feet 6 inches; 14 of 70 feet 6 inches; 2 of 88 feet; 21 of 129 feet 6 inches; 13 of 146 feet; 1 of 162 feet; 1 of 170 (bowstring girder), and 13 of 245 feet. Of the piers, 14, on the Fife coast, in accordance with the original design, were built of brick, set in cement, the foundations being produced by sunk cylinders filled with concrete. The rest were constructed of combined cast and wrought iron, of various strength, and composed of varying numbers of columns, according to the place they take in the structure, and the work they have to do. The largest girders rise to a height of 88 feet above high-water in the Tay, the line of rails being on a rising gradient from the Dundee end of about 1 in 73. The girders are of the lattice construction with double triangulation, and trough booms at top and bottom, from 15 to 24 inches in width, according to the span, a vertical tie being fixed from the top boom to the crossing of the struts and tees, at every alternate crossing. The depth of the girders is one-eighth of the span, a proportion adopted by Mr. Bouch and Mr. A. D. Stewart, C.E., as the result of many experiments, and as giving the greatest strength with the least material. The cross-girders are of pitch pine, 12 inches by 9 inches, the rails being carried on longitudinal beams, 17 inches by 7 inches, and the whole planked with 3-inch Memel, covered with asphalte, as a protection from weather, and from the ashes falling from the locomotives. Two light hand-rails of $2\frac{1}{4}$ inches (inside) gas-tube run the whole length of the bridge, carried by metal stancheons. These tubes are intended to convey, one water and the other gas, from one side of the Tay to the other. Over the space occupied by the thirteen large spans of 245 feet, which are placed over the navigable part of the river, the engines and trains are to run between the girders, the rail platform resting on the bottom booms; as to the other spans, the trains are to run on platforms fixed on the upper booms. By this arrangement the cost will be less, and the gradient will not be so unequal. The girders are continuous in sets of four, with sliding beds on those at each end of each set.

In the construction of this remarkable bridge, some novel appliances and expedients have been employed. The sand pumps drew the sand through the centre of the caissons with great rapidity and ease, the caissons sinking by their own weight as the sand was removed. By the process the sand which was taken out of the river inside the pier was returned to the river outside the pier. Enlarged sand-pumps were applied successfully in sinking the large 81-feet caissons which form the foundations for the piers of the large spans. The general dimensions of the land-pumps are: Diameter of cylinder, 7 inches; stroke, 12 inches; diameter of air-cylinder, 12 inches; stroke, 12 inches; steam, 50 lbs. Each tank holds 50 cubic feet; and from one to three minutes, according to the depth, are required to fill one tank. The building, floating and sinking of caissons of so large a diameter as thirty one feet was a matter of no ordinary difficulty, and required the most careful calculations, and special machinery of an original, although simple character, and the conditions of time, tide, and weather had to be taken into account, especially in respect of the sinking operations. In the construction of the piers, and in the placing and raising of the large girders, there were new applications of old ideas and of mechanical and natural forces. Extensive use was made of divers in the progress of the work. Four steamers were always in use, besides barges of all sizes, and a fleet of small boats, requiring a constant staff of boatbuilders for necessary repairs. Light was obtained during the dark evenings and nights of the winter of 1876 from two of Gramme's electro-magnetic machines, which were fixed in a building close to the foundry engine and driven from it. The electric current so generated was conveyed through insulated wires to two of Serrin's lamps, fixed in sentry-boxes on the top of the hill overlooking the works. Each lamp gave the light of 1,000 candles.

ORDNANCE AND NAVAL.

Two more of the 100-ton guns in course of manufacture by Sir. W. G. Armstrong & Co., for the Italian Navy, have just been completed at the Elswick Ordnance Works, Newcastle on-Tyne, and await shipment on board the Europa. The vessel is expected to arrive from Spezia in the course of the present month, bringing back the first 100-ton gun, which has given the highest satisfaction to the Italian Government, and is now to be returned to Elswick for the purpose of being chambered and having its bore enlarged. The two 100-ton guns about to be conveyed from the Tyne to Spezia are considered capable of producing much better results than those exhibited by the first of these monsters, some important modifications having been introduced in these later specimens. These guns have a calibre of $17\frac{3}{4}$ in. and a powder chamber of $19\frac{3}{4}$ in. The highest charge of powder fired from the first 100-ton gun was 397 lbs., the projectile weighing 2,000 lbs. The Italian authorities will probably fire the new guns with a charge of 470 lbs., and the projectile may be expected to weigh 2,280 lbs., or a little more than a ton. The highest charge yet fired from the 80-ton Fraser gun is 425 lbs., with a projectile of 1700 lbs., the bore of the piece being 16 in. and the powder chamber having a diameter of 18 in. In reference to the guns about to be conveyed to Spezia, it should be stated that they will be accompanied by the hydraulic carriages and gear devised by Mr. George Rendel, of the Elswick firm. It will be remembered that the Italian Government required eight of these great guns. The experimental gun, when altered, will form one of the eight. Thus five remain to complete the set, and these are in various stages of manufacture, together with their carriages and gear.

Experiments with Artillery made from Steel without Blows.—At the last meeting of the Institute I exhibited several 8 and 10 inch shells, made out of steel without blows, which had been simply cast, tempered and reheated. These had penetrated armor-plate of a thickness equivalent to their diameter at an angle of 50°. These remarkable results were obtained not only in France, but also by the Italian and Russian navies. In such oblique firing the work of perforation becomes complex. The first action on contact is one of compression; this is followed by a certain amount of flexion, which tends to bring the projectile to a position perpendicular to the plate at the point of least thickness; at last after penetration of the conical head, comes the friction of the cylindrical portion against the rough edges of the hole, which tends to elongate the shell longitudinally. Such a metal resisting both tension and compression against instantaneous deformation is remarkable. We then confidently expressed a hope that the time was nigh when artillery would be made from steel without blows as powerful as from the best forged steel, and not much more difficult to manufacture than the old cast-iron and gun-metal cannon. This result has now been obtained, the first experiment having been tried on the 17th August last, in one of the French Government arsenals.

On whatever basis we study the resistance of elastic tubes, it is easy to see that the portion of a cannon made in one piece which suffers the most must be that portion wherein deflagration of the powder occurs. This action decreases rapidly, so that in certain cases the doubling of the thickness of metal does not increase the resistance by more than 10 to 19 per cent. In order to overcome this inequality of molecular work in heavy ordnance, it is well-known that a succession of concentrical envelopes has been employed, applied in close contact to one another and compressing the central portion in a permanent manner. When the powder is exploded this compression, which decreases from inside out, must be overcome, after which the metal acts only by traction in a regular manner, from the centre to the periphery. The French heavy artillery is based on this principle, and is formed of three elements: (1) the central tube which has most duty to support and is made from hammered cast steel; (2) the body which is cast iron, although a better material might be found; (3) the fretes in puddled steel, which produce the desired compression.

The most severe test, which the steel without blows could be made to resist, was by employing it in the shape of a simple tube. Such a one was cast at Terrenoire of a diameter of 8 inches, bored by a hole 5 inches diameter, so as to leave only 1¼ inch of metal on the outside. Nothing was done besides a tempering and reheating, after which it was grooved and a screw head adopted to carry the breech. Before proceeding to the conclusive experiments, several pieces of the metal cut perpendicularly to the axis of the tube were tested with the following results:

		Limit of Elasticity.	Charge of Rupture	Lengthing.
		Tons per square in.		Per cent.
At the back...	No. 1	22.0	42.5	11.1
" ...	" 2	22.2	39.6	8.7
In front......	" 1	22.5	38.1	15.1
"	" 2	22.7	38.5	15.0

Several pieces 1¼ inch square and 6 inches long were next submitted to the shock of a ball weighing 40 lbs., and allowed to fall from increasing heights. The supports were 5 inches apart, and rested on an anvil weighing 1800 lbs. These pieces resisted well, and one of them did not break when the ball fell from 8 feet in height, which gave it a bend in center of about 1 inch. The French Government have imposed for the trials of all steel tubes destined for the navy:—Limit of elasticity, 21 tons; and charge of rupture, 38 tons per square inch.

It will be seen from the above that the tube in steel without blows answered more than the requirements. For the experiments with powder the tube was mounted on a portable carriage, after having been placed in a suitable collar.

Twenty shots were first fired with the ordinary war charge of 9 lbs. of powder and the 40 lbs. shell; after this 10 shots with a shell weiging 47 lbs., and from this time forward the charge of powder was successively increased by ½ of a lb. every ten shots, the shell remaining identical, until the 100th shot was fired. At this point the chamber was quite full, and the charge had to be rammed in order to get it into place, and much difficulty was found in closing the gun. The experiment was stopped at this point as the official regulation test had been accomplished. After each 10 shots, the tube was washed out with care and measured by means of precise instruments in every portion of its course. No fissure of any kind was discovered, and the deformation of the chamber was found to be less than half of the average in forged steel tubes. Other tubes will be submitted to trial very shortly, two of which will have an internal diameter of ¼ inches only. One has been cast of 13½ inches outside diameter for a gun the body of which in cast steel is to weigh 19 tons. As the central tube is the more delicate portion of the cannon, I have little doubt of success in manufacturing the body also in steel without blows, and that we will soon turn out, with very limited means of manipulation, artillery castings weighing 20 tons each. Experience will soon teach us if we are right in this matter.

The remarkable results of those first experiments show, if nothing else, that we have had to deal with a metal possessing very interesting and valuable properties, well worthy the attention of engineers.

BOOK NOTICES.

MINUTES OF PROCEEDINGS OF THE INSTITUTION OF CIVIL ENGINEERS. London: Printed by William Clowes & Sons.

We have received through the kindness of Mr. James Forrest, Secretary of the above Institution, the following late publications:

THE MAIN ROADS OF SOUTH AUSTRALIA. By CHAS. TOWNSHEND HOWE, A.I.C.E.

Also FOUNDATIONS. By JULES GAUDARD, CIVIL ENGINEER.

The latter, which is quite fully illustrated, we shall republish in this Magazine nearly in full.

A GUIDE TO THE DETERMINATION OF ROCKS: Being an Introduction to Lithology. By EDWARD JANNETTAZ. Translated from the French by George W. Plympton, A. M. New York: D. Van Nostrand. Price $1.50.

The study of rocks is assuming an importance in this country not appreciated till quite recently. A course of Geology, as pursued in the technical schools, is incomplete without some knowledge of the classification of Rocks adopted in Europe.

This little work of Jannettaz aims to supply the student with a systematic method of determining rocks, by means quite familiar to any worker with the blowpipe. So far as we know it is the only work in the English language which covers similar ground.

SANITARY ENGINEERING: A Guide to the Construction of Works of Sewage and House Drainage. By BALDWIN LATHAM, C.E. Chicago: George H. Frost. For sale by D. Van Nostrand. Price $3.00.

This excellent work first appeared in London in 1873. The edition was exhausted, although the demand continued as brisk as ever.

This reprint is a copy of the original work and is now the only treatise in English that enters into all the details of sewage and house drainage.

CABLE MAKING FOR SUSPENSION BRIDGES. By WILHELM HILDENBRAND C.E. New York: D. Van Nostrand, Price 50 cts.

This is No. 32 of the Science Series, and has already appeared in full in this Magazine. This little volume contains a rare amount of technical science in small space.

THE WORKSHOP.—Nos. 10 and 11. New York: E. Steiger.

This periodical is for the designer in wood or metal.

The illustrations are of the highest artistic merit, and exhibit the best style of execution. Each number contains eight large plates.

ELEMENTS OF THE METHOD OF LEAST SQUARES. By MANSFIELD MERRIMAN, PH. D. London: Macmillan & Co. For sale fiy D. Van Nostrand. Price $2.50.

This elegant little treatise will be welcomed by many students who have felt the need of a better knowledge of this important principle, and of the proper limits of its application.

It is written for learners and not for mathematical experts, and will prove of wider benefit than former treatises on the same subject. We recommend the work to the favorable notice of Engineers for whom it was in great measure prepared.

SULPHUR COMPOUNDS IN GAS. Report of a Parliamentary Committee. London: Wm. B. King. For sale by D. Van Nostrand. Price $2.50

This is a technical work, of interest chiefly to Gas Engineers. It is a report of the Proceedings before a Committee of the House of Commons, on the Crystal Palace District Gas and The Gaslight and Coke Company's Bills.

The discussion is upon the proper degree of purification of gas from sulphur, with proper regard to the comfort of residents of the neighborhood of the works, and on the other hand the comfort of the consumers.

RAILWAY REVENUE AND ITS COLLECTION. By MARSHALL M. KIRKMAN. New York: Railroad Gazette. For sale by D. Van Nostrand. Price $2.50.

In the preface to this work the author says that under a system similar in many respects to that elucidated in this book, he has collected one hundred and fifty millions of dollars of railway receipts, without the loss of a dollar through dishonesty, although the money passed through the hands of some four hundred agents.

The book details the duties of railway officials and their agents, and sufficiently explains the necessity for a carefully elaborate system.

BAILLAIRGE'S STEREOMETRICAL TABLEAU: A New System of Measuring all Bodies, Segments, Frusta, and Ungulae of such bodies by one and the same rule. By CHS. BAILLAIRGE. Architect and Engineer. Quebec: C. Darveau.

Those who are desirous of knowing the number of applications of which the well-known Prismoidal Formula is capable, should study this work. For the use of teachers, a set of models is prepared, so that the instruction in Mensuration is made practical.

The Key to this Stereometrical Tableau, of which we have before us copies in English and French, begins with the measurement of surfaces, and then proceeds to the treatment of solids of many kinds, including a large list of volumes bounded by curved surfaces.

The author has received many flattering notices from widely different sources, and several gold medals.

THE ELEMENTS OF MECHANISM DESIGNED FOR STUDENTS OF APPLIED MECHANICS. By T. M. GOODEVE, M. A. Lecturer on Applied Mechanics at the Royal School of Mines, and formerly Professor of Natural Philosophy in King's College, London. Second Edition. London, 1871. For sale by D. Van Nostrand. Price $1.50.

PRINCIPLES OF MECHANICS. By T. M. GOODEVE, M. A., Barrister at Law, Lecturer on Applied Mechanics at the Royal School of Mines. Second Edition. London 1876. For sale by D. Van Nostrand. Price $1.50.

Perhaps there was a time when a mass of facts thrown together without order, and illustrated by a few wood cuts, was accepted as a text-book. We hope we do not puff ourselves up when we say that that time has passed. At the present day the logical arrangement of a book is fully as much considered as the material which it contains, and any work that does not stand examination on both of these points is discarded as unnecessary, indigestible or worthless mental food. A book which is founded on mathematics ought, above all others, to be logically deduced from definitions and principles clearly stated in the beginning.

In the two books under consideration we find many faults, a few of which we will mention, although the title of the first in a measure disarms us. If the author has confined his attention to the Elements of Mechanics, we can hardly expect more of him than simplicity and perspicuity. It would be reasonable to expect to find somewhere early in the book a description of the elements of all mechanism, the lever and the inclined plane, but these are not contained in the first book. The author rather—boldly dashing off an incoherent statement of what he intends to do—begins his work by talking of wheel-gearing. Then, by the magic which a gentleman of such varied accomplishments must possess, he changes through "straps and bands" to the "screw." In the next chapter we find, under the heading "The conversion of circular into reciprocating motion," a discussion of "Stanhope's Levers," and many descriptions of Reversing Motion. And we may note that the author in several places (pages 20, 21, and 183) has endeavored to prove simple principles of mechanics in simple ways, and failing, has taken refuge in a foot-note where he has used the Calculus. Then again, it seems rather out of taste to introduce definitions and deductions from the "Principles" in this connection; surely Newton's ideas and words are worthy of a better setting. On pages 106 and 107 we find a few representations of all the faults which ever exist in text books. We have there unnecessary mathematical work which is certainly beyond the scope of one who would study the elements of mechanism ; we have blind definitions, a thorough understanding of which presupposes some knowledge of the Principia ; and we have an error in a mathematical expression.

It is, however, in the second of these works that we find the most to condemn. We can not deny that the author has described many new and late inventions, and, from this fact, his book may possess a value. In the place which it was made to occupy, we think it is worse than worthless.

In an introduction of sixty pages we find an account of the labors of Poisson and Adams on the acceleration of the moon, a popular exposition of the conservation of energy, some remarks on the ventilation of coal mines and tunnels, and an explanation of Siemens Steam Jet. Throughout this chapter the "Law of gravity" is considered as a thing which can be reversed (p. 54) and contradicted (p. 55); although on page 3 the author states most emphatically that "no truth in science can be deemed to rest upon more sure foundation." And g, that ever troublesome g, is at last quieted ; for we are told on page 57 that "it is usual to take g as 32 when the answer works out easily by so doing." It is a question whether the author has not as arbitrarily defined inertia as he has disposed of g; for he tells us that when a shot penetrates water there is no resistance from inertia of the water, but when the shot rebounds from the water the inertia is "palpable enough." He gives in many places new and interestiong ideas on ballistics ; for example, we are told (p. 44) that a solid shot ranges further than a shell of the same dimensions, because it is retained longer in the bore of the gun ; and, on page 306, we are told that the length of a projectile should not be less than three times its diameter.

The author fails to define the difference between the "center of parallel forces" and the point of application of the resultant of these forces, although one is a single point and the other is any point in the line of direction of the resultant. These are a few of the many faults of the book, which the attentive student can not fail to notice. The great failing of this book is the chaotic confusion of subjects and ideas. Watts' governor we find described on page 297 and discussed on page 260. Whitworth's true plane is described on page 143 and again on page 309. Throughout we can only notice that lack of logical arrangement which has done much to make Willis' Principles of Mechanism a classic work. We think it is time that Oliver Evans should be credited with the invention of what Mr. Goodeve calls Seaward's parallel motion.—*Imperial College, Tokio, Japan.*

MISCELLANEOUS.

THE tin deposits of Banca were first worked from 1700 to 1720, but with small production. After that period numerous Chinese miners flocked thither, and by degrees extended the workings over the entire island. The most prosperous season was between 1770 and 1775, when about 3800 tons of metal were annually produced. Subsequently the production fell off very much, but has lately increased again. The entire product, however, of both the Malay Peninsula and the islands was not, in 1848, as large as the amount formerly derived from Banca alone. The process of mining is very simple, and the ore is found intermixed with yellowish sand, or in fragments of felspar and quartz, resulting from the decomposition of granite. The smelting is effected on the spot, with charcoal, in rude furnaces; the bellows employed being the hollow trunk of a tree, in which a piston is worked backward and forward ; 80 to 90 picules being produced in a night's work ; and, from the good quality of the ore, no refining is needed. The Australian tin deposits, discovered to be workable only in 1870, have increased so largely in productiveness that, up to the end of 1874, ore worth 4,300,000 dols. had been obtained in New South Wales (the exports from that colony, in 1874, amounted to $2,400,000), while the area of tin-bearing land, in the same colony, was estimated at 6250 square miles, and in Queensland at 22,000 acres. The tin ore of New South Wales is partly from veins in granite, but mainly from washings. Some of the specimens of stream tin at the Centennial Exposition were almost unsurpassed in size and beauty. Queensland is stated to have produced, for some years after 1872, about 5000 tons annually.

JABLOCHKOFF'S ELECTRIC CANDLE. A description of the apparatus which bears the above name appeared in our number for November 25th of last year. Since then repeated experiments have been made with it, confirmatory, on the whole, of the good reputation it had already achieved. The first practical trial of this system was made a fortnight ago at the Magasins du Louvre. The Marengo Hall was the apartment lighted, and six electric candles were sufficient to shed around a very bright light, softened by opal glass globes. Some idea of the comparative value of gas and the electric light may be formed when we state that the Marengo Hall is ordinarily illuminated by means of 100 argand gas burners of the largest size. The cause of the wide difference between this and other electric lights lies in the fact that electricity plays, so to speak, only a secondary part in producing the light. The light is principally the result of the combustion of the refracting material which occupies in the electric candle the same position as does wax or tallow in ordinary candles. The electric candle, as originally designed by M. Jablochkoff, consisted of what may be termed a double wick and a surrounding material. The wick consisted of two carbon points, about 4 inches long, embedded parallel to each other in an insulating substance, by which also they were separated from each other. This material, which was consumed as well as the double wick, was composed of several ingredients, forming a combination known only to the inventor.

Each of the carbon points terminated at the bottom in a small metal tube into which the conducting wires were led. The next development of the electric candle by M. Jablochkoff was to denude it of its outer casing, leaving merely the double wick with a strip of the insulating compound between the carbon points, which terminated at the bottom in metallic tubes, as before. It was with the electric candle in this form that the hall at the Magasins du Louvre was illuminated, as previously stated. In either case only one electrical machine is needed to produce a number of lights. The positive and negative wires are led from the machine, and branch wires are simply conducted from them at the necessary points to the candles. In this way M. Jablochkoff succeeded in getting as many as eight candles to burn at the same time in the circuit of a single machine of the ordinary kind, with alternating currents.

Arrangements are being made in England to light up one of the East and West India Dock Company's docks in London upon M. Jablochkoff's system. Experiments were to have been primarily made in order to test it, but since the exibition of the electric candle at the Louvre M. Jablochkoff has still further improved his system, so that the experiments have been postponed for the completion of the details of the improvement. In the new form of candle the inventor dispenses with the carbon points which constituted the wick, and uses only the outer surrounding material answering to the tallow of an ordinary candle. We have already seen that this material, which consists in large part of kaolin, consumes at the same rate as the carbon points. From this material alone M. Jablochkoff now produces results superior in many respects to those which he previously obtained. One point of superiority consists in the fact that he is now enabled to produce as many as fifty constant and uniform lights from a single machine of the ordinary kind. In short, M. Jablochkoff appears to have satisfactorily solved the question of dividing up the electric light by a method capable of practical application, of insuring perfect steadiness in the light so divided, and of distributing throughout a building lights of varying degrees of intensity.—*Journal of the Society of Arts.*

In a paper recently read before the Ashmolean Society, Mr. W. H. White, C.E., engineer to the Oxford Local Board, has given the following particulars of the drainage of Oxford: The total length of new sewers and surface drains is $32\frac{3}{4}$ miles. Of this length $7\frac{3}{4}$ miles have been constructed of brick and twenty-five miles of stoneware pipes. The temporary pumping power supplied consists of a portable double clyinder engine of 14 nominal horsepower and a 12 inch centrifugal pump driven by a belt from fly-wheel of engine. At a fair working speed of ninety revolutions of engine, 2300 gallons of sewage are discharged per minute, and at present about a million and a-half are dealt with daily. The water supply of Oxford is about two millions per day—that is, sixty gallons per head of the entire population, and in very wet weather the 100 acres contributing surface water to the sewers would yield about the same quantity. Therefore, if the present amount of water continued to be pumped into Oxford the ultimate wet weather flow would be something like four millions per day. He concluded by stating that 370 acres of land had been purchased at Sandford for an irrigation farm to connect which with the pumping station a rising main of cast iron pipes $1\frac{1}{2}$ mile long has lately been commenced. The sewage will have to be lifted a height of 57 feet, and the engines will be from 56 to 60 horse power in duplicate.

Metals may be colored quickly and cheaply by forming on their surface a coating of a thin film of a sulphide. In five minutes brass articles may be coated with any color, varying from gold to copper red, then to carmine, dark red, and from light aniline blue to a blue-white, like sulphide of lead, and at last a reddish white, according to the thickness of the coat, which depends on the length of time the metal remains in the solution used. The colors possess a very good lustre, and if the articles to be colored have been previously thoroughly cleaned by means of acids and alkalies, they adhere so firmly that they may be operated upon by the polishing steel. To prepare the solution, dissolve one half ounce of hyposulphite of soda in one pound of water, and add one half ounce of acetate of lead dissolved in half pound of water. When this clear solution is heated to from 190 deg. to 200 deg. Fah., it decomposes slowly, and precipitates sulphide of lead in brown flakes. If metal be now present, a part of the sulphide of lead is deposited thereon, and, according to the thickness of the deposited sulphide of lead, the above colors are produced. To produce an even coloring, the articles must be evenly heated. Iron treated with this solution takes a steel-blue color; zinc, a brown color; in the case of copper objects, the first gold color does not appear; lead and zinc are entirely indifferent. If, instead of the acetate of lead, an equal weight of sulphuric acid is added to the hyposulphite of soda, and the process carried on as before, the brass is covered with a very beautiful red, which is followed by a green (which is not in the first scale of colors mentioned above) and changes finally to a splendid brown with green and red iris glitter. This last is, according to the *American Art-Journal,* a very durable coating, and may find special attention in the manufactures, especially as some of the others are not very permanent. Very beautiful marble designs can be produced by using a lead solution, thickened with gum tragacanth on brass which has been heated to 210 deg. Far., and is afterwards treated by the usual solution of sulphide of lead. The solution may be used several times.

The first bogie engine ever made in Australia was recently erected in the Williamstown shops, for the Victoria Railways, under steam, and made a trial trip between Williamstown and Melbourne, running the distance in very quick time. The engine was found to work quite up to the expectations formed.

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